DOWN TO EARTH

Jozef Visser

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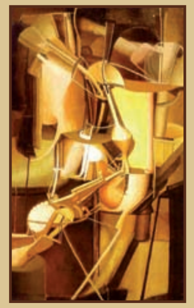

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een bijdrage voor Stichting A Rocha, een internationale beweging van christenen die geboeid zijn door Gods schepping en zich inzetten voor het behoud ervan.

Down to earth

A historical-sociological analysis of

the rise and fall of 'industrial' agriculture

and of the prospects for

the re-rooting of agriculture

from the factory to the local farmer and ecology

Jozef Visser

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A historical-sociological analysis of the rise and fall of ‘industrial’ agriculture and of the prospects for the re-rooting of agriculture from the factory to the local farmer and ecology

Jozef Visser

Thesis

submitted in fulfilment of the requirements for the degree of doctor

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Preface

On my desk I have a text from the ancient 'wisdom literature'. Some three millennia old, its proverbs are as fresh as ever. With an important focus on stewardship, they are solidly geared to ordinary people. As to their thrust, just ponder the following one: '*Better a plate of vegetables where love rules, than a fattened ox with hatred*'.
In a way post-war, all-out modernization focussed on the fattened ox, loudly declaring that all the rest would follow. For it was decidedly not only the communists who believed in an economic base, that then decides about all the rest as its super-structure. When the learned van der Leeuw, in the first post-war Dutch cabinet, made a serious effort to approach the arts as a truly communal endeavour, mainliners indignantly dismissed him. That signalized the end of a remarkable episode, in the Netherlands, in which culture was stimulated with modest means, geared to common people at the local level. In the decades leading up to this remarkable episode quite a few of the best minds from the youth movement and from the Open University's precursor invested more of their time and energy than was good for them. Independent minds like van Eijk and Kohnstamm were among them. But after the war the tides soon turned against their efforts. Similar decisions to let 'the economy' prevail were made everywhere.
Impressed by the 'powerful' solutions of the great victor, the USA, both the old and the new nations decided for an all-out modernization after its presumed example. In older nations, local life-styles in combination with more traditional systems of governance temporarily constituted a counter weight to the modernization drive of the government. But all this could only slow down the consequences of this totalitarian drive, which eroded the economy's embeddedness in the local ecological and socio-cultural realm. Where traditional systems of government were under more pressure still, and local life-styles more vulnerable still than in the West, the modernization craze proved its eroding qualities most openly. After half a century much of Africa is in ruins, a prey to dictators from within and to powers from without, more often than not in puzzling combinations. But then, priority was granted to a 'production plan' at the expense of the local socio-cultural and ecological realms.

Yet, at the start it was difficult to discern this dark end of the way. After the war there was widespread deprivation, and strong measures were required. Soon a secularized version of the Salvation Army's '*Soup, soap and salvation*' had its day. For who would quarrel with this sequence of priorities: food - health - education? But, of course, the real issue was the disregard of the local level, where the Salvation Army and similar organisations had always focussed its efforts. After half a century, we are back at exactly that level, and not only for Africa. We now emphasize the need to improve the local social justice systems and the use of local resources. The importance of the ability of people to live in harmony with their neighbours and the local ecology is being rediscovered. In a painful way we learned an old lesson: it is 'wisdom' that alone lays sustainable foundations for economic life. The 'strong' economy envisaged by the post-war modernization craze is an illusion, and leaves demolished local resources and communities behind.
Re-focussing on the ground-level of human life, where people interact, also implies re-embedding the economy in the local socio-cultural and ecological realms. Though it is still common to hear the verdict 'impossible!', that verdict ought to be reserved for the urbanization and deruralization that is at the core of post-war modernization. There is no socio-ecological future for our megacities and their devastated hinterlands. Yet, for half a century we were all so obsessed by modernization, that we were actually erasing the traces of other ways of life. We may no longer have the great faith of those post-war decades, but its consequences fill our horizons, so that it has become difficult to link up with tradition.

Yet, some of its modes of existence have proved their sustainability for ages already. Aspects of that viability emanate from the words of wisdom expressed in the ancient text in front of me.

The central target of the destructive thrust of modernization was (and is) the peasant and traditional agriculture. The massive government involvement with the agriculture/food sector in the industrial countries is illustrated by their production- and export-subventions, totaling some 350 billion dollars for 2004 alone. Peasants everywhere are exposed to such powerful pressure from the rich countries that they are being annihilated. But note this pressure is exerted by the government and by big agro-concerns, with their ‘industrial agriculture’ an artifact, its existence completely dependent on past and present government power. Even a cursory look at its ecological and social credentials teaches us that sustainability is out of the question here. Quite to the contrary, this ‘industrial agriculture’ has a devastating effect on the industrial countries themselves. Its concept of ‘productivity’ is debilitating, and paralyzes timely processes of re-evaluation and policy making.
It is urgent to dispel the sense of *fatum* that is connected with the ‘industrialization’ of agriculture and its supposed inevitability. That notion of urgency came to me when, in the late spring of 2000, Bob Goudzwaard and I had the privilege to speak with the council members of the CCA, a recently formed organisation of mostly small farmers in the Netherlands. The ability of those people was evidently as big as their mountain of problems. These farmers were able enough, but they faced a mountain of problems because of the government’s grim policies of ‘industrialization’ of agriculture, that since long had degenerated into an escape from reality. In less disabling circumstances these were resourceful farmers. Yet, how to live and labour when government and agro-concerns erode your very way of life and production process?
Still the resourcefulness of those farmers is a core element disproving the notion of *fatum*. Although the living reality of farmer and farming is in constant danger because of our all-out modernization policies, here before our eyes there was the core of this living reality: people surviving decades of policy making aimed at their eradication. Without any doubt this testifies to their solidity and viability. Jan Douwe’s ‘*De virtuele boer*’, as well as Bob’s explanations of the economic side of it all, were a great help to me to get a better idea of the many aspects of this viability. I could now refer already to two traditions that in some way had resisted half a century of all-out modernization policies: the tradition of the small farmers, and part of the academic tradition. Resistance offered by academia was remarkable indeed, considering the popular opinion that ‘science’ was at the heart of the modernization drive. But I had experience enough with the reality of tradition in e.g. chemistry to know that, ultimately, it would escape re-definition by either government or big industry (its tradition predated those present powers for centuries). On close analysis, the traditions of science would also be valuable in evaluating the massive ‘modernization project’.

When the research scheme was agreed upon with Jan Douwe and Bob, we realised it would be quite complex. Agriculture itself is complex indeed and has its connections with a lot of other fields. Yet the need to get the peasant/small farmer in focus again helped to retain a unity in the research projects that were to follow. As to their wide scope there was no escape: time and again previous research on specific questions proved to be fragmentary only. And so, always having been an avid reader, I yet had to shift to a higher gear to explore the vast amount of literature. Putting it all into historical perspective was the ‘thread of Ariadne’ in (re)ordering the huge amount of literature that became available in the course of the projects. Gradually it turned out how strongly the modernization paradigm influenced us all. Its exclusive tendency to concentrate on the present had dissolved historical memory.

We had become accustomed to viewing history in a one-dimensional way, with the opposition between tradition and modernity as its ‘essence’. We used tradition only as a dark background against which modernity stood out more clearly. Only a keen perusal of the professional literature could dispel this emphasis on the present and bring history back in. It became increasingly evident that our destructive 20th century was not amenable at all to a linear-evolutionary description. And yet such a description was at the heart of current accounts of agricultural development and policy. It became apparent that modern agricultural policy could not be termed ‘progressive’, neither in material respect, nor as to its treatment of the peasant/small farmer. Instead, it reflected in many ways the outright confusing character of the age, with its global and total wars. People in such devastating circumstances will cling to any life-buoy. The more so if that is one that looks as big as ‘modernization’.
The more the historical perspective was regained, the easier it was to focus on the real-life resources of agriculture and food production. Here again some scientific traditions proved helpful, not because of some supposed ‘superiority’, but first of all because of the ample paper trail that they had left behind. Of course, quite decisive was that government and big industry, although they were instrumental in the accelerated growth of many research disciplines in post-war decades, still had to deal with disciplines that not only predated them for centuries, but that also proved vital enough to maintain and develop their own standards. Consequently, government and big industry had simply not been not able to re-define all of academia and research, in spite of their all-out modernization project. There were standards left, in several disciplines, that were helpful in evaluating notions and practices that had become dominant under the modernization paradigm. For example, in comparing agricultural with industrial production. Gradually ‘industrial agriculture’ proved to be non-sustainable. Nowhere had it originated ‘from the soil’. Government and agro-industry had imposed it ‘forcefully’ instead.

These analyses were a help in regaining sight of the central position of farmer and (local) ecology in agriculture and food production. The non-sustainability of the modernization project became clear enough but, more importantly, so did the sustainability of farmer- and ecology-centred versions of agriculture. It became apparent that there is no substantial obstacle blocking the transition to such versions. Yet, the institutional restraints are strong indeed. For the government and its experts not only claimed to be guides towards the modernization of agriculture, but they also increasingly reformed agriculture to an activity ruled from centres of power and ‘expertise’. In that process the re-definition of laws, rules and standards played a prominent part. Yet, although laws and rules can easily be changed, and subsequently imposed on rural subjects by powerful governments, nature and ecology cannot thus be forced into ‘cooperation’.
It became increasingly apparent that the newly devised rules did not apply to nature and the ecology. Here only sympathetic cooperation will do, orders from ‘centres of power’ are of no avail. That is the reason that sustainability is not to be achieved, unless central responsibilities are re-located again to the ‘shop floor’, to the local farmer and ecology. For modern agricultural policy suffered from self-inflicted blindness, and assumed poverty where it did not exist. Its own narrow paradigm allowed only tunnel vision. It not only missed out on the rich resources that the farmer always had at his disposal, but also made them dis-functional (if it did not destroy them). To correct this abuse we do not need big budgets. What we need in our time, in which the modernization project proved a blind alley, is people who are dedicated to the plight of the farmer and the ecology. The sustainability of agriculture and food production now depends, not on supposed high-tech science and technology, but on people in government, research, and industry, who are ready to step out of the limelight themselves, to allow the farmer and the ecology to take the centre again.

Acknowledgements

If we consider the scope of this thesis, it is clear that part of its roots are in previous decades, and that many people contributed to it, in one way or another. Although such contributions can be hard to specify, there are some people whose influence can be defined more easily.

With deep respect I remember the distinguished science historian Reyer Hooykaas, whose approach to science as history is the backbone of this thesis. As to the years in which the project went through its consecutive stages, Bob Goudzwaard, economist of 'joyfull sufficiency', was always there with his counsel and encouragement. Jan Douwe van der Ploeg supervised the research on its long way to sociological maturity, and probed its non-standard approaches. Farmer Maarten Molenaar gave freely of his time in the many talks that helped me probe the history and practice of post-war agriculture. His friends from the CCA (christian farmers' society) were also a great help in dealing with the project in a down-to-earth fashion. Some people helped with their critical reflection on the way. Nanning-Jan Honingh was a great help in matters of nature and ecology throughout the years. And Wim de Hoop introduced me to a valuable opportunity to focus on crop N-nutrition as related to agricultural policy.

Discussions on various scientific subjects were always a welcome break, during the years in which the vast amount of literature to be collected and scrutinized required a near-monastic way of life. For years there were especially two groups of people whose participation in such discussions was a living reality. The first was the CEN network, with its wide public and its ecological focus. The second was Henk Jochemsen's Standing Committee on the Life Sciences, with its profound discussions of diverse scientific subjects.

The present research project, because of its comprehensive character, had its preparatory stages in previous decades. I remember many people with respect and affection, yet I want to mention two people in particular, because they opened up the future to me. Rien de Bruin took my first explorations into a historical approach to science fully seriously. Rien proved to me, as to many others, that truthfulness is an enduring source of life. Jaco Kraayeveld was also such a source. Your dedication saved the lives of at least two people: Andreas' and mine.

The benefit of the present report for a wider public is conditional on the usage of the English language as vehicle. The brittle, book-derived language of someone like the present author, does not meet the standards. Fully aware of the extent of the endeavour, Meindert and Debby Gast offered freely of their time and energy to scrutinize the text, both linguistically and didactically. It stands to reason that I followed their corrections and emendations. Of course, such faults as remain are mine only.

One person stood by me and encouraged me unequivocally throughout the preparatory stages of the project to the very end. Nastasha, my wife, weathered many a storm standing shoulder to shoulder with me. During the last decade she has allowed me ample scope to complete this project and accepted ungrudgingly its rather open-ended character. All of this provides us with a reason to celebrate and to sing a Slava Vishni Bogu with our children.

Contents

Research questions
and
Outline

Unwrapping

Re-analysing a long half-century that was under the sway of High Modernism means first of all painstakingly to unwrap that period from its multifarious stories that took the present for the unique standard in historiography. With its linearizing accounts always ready to position a subject on the imagined axis 'tradition-modernity', the present was sure to show up as progress compared with the past. Post-war agricultural policies even got projected from such linearized accounts of history, with 'agriculture striving for the goal of Modernity', as mediated by experts in government-related institutes.
For sure, doubts have increased about the results attained, yet the picture of recent history as the linear sequence from tradition to modernity allows no escape. If now, in our 'globalizing' world, instead of the certainty of 'progress', we note the combination of ecological and social despair of the many, and the mad scramble for power and money of the few, our Modern Man is inclined to see 'fatum' where he formerly thought 'progress'. Many a policy maker simply sees no 'real' alternative to the industrial agriculture that has been introduced.
Still the question of alternatives has been posed, ever since the subject of sustainability came on the agenda. And for sure, tripling food prices for the undernourished poor do not point to sustainability, but to economic and agricultural insanity instead. With our former passion-for-progress crumbling, is an ecological and economic jungle all we have left as a prospect?

As to food and agriculture, our policies are still firmly entrenched within High Modernism and its institutions. Its presumed certainty of progress brought the non-consideration, if not denial, of alternatives with it. As a result policy makers in the present stand perplexed before an avalanche of problems, unaware of the many possibilities that got discarded - and that yet could prove perfectly sensible. As hinted at, the 'certainty of progress', once it gets disproven by the facts of life, automatically turns into despair, unless we can get rid of its linearized accounts and regain many-dimensional history instead. So in *chapter 1* we will try the un-wrapping, because we need to regain space for approaches to food and agriculture that do not land us into despair.

Lost in the rupture, and found

That post-war agriculture in its High Modernist accounts received the epithet 'revolutionary' signifies, in effect, the break of post-war policies with 'traditional' agriculture and its diversity. A rich diversity of agro-ecologies and rural communities got exchanged for a presumed 'industrial agriculture' that from the start looked fairly monotonous. Its character was indeed 'revolutionary', considering it consisted first of all in this near-complete rupture with the (recent) past. At one big stroke the agro-ecological and communal diversity, that always had been essential to food production, got discarded, and the one government-induced 'industrial' version put in its place. With so many people nodding, instead of protesting, there evidently was a common ideology in the air that made it all seem 'rational'. As a result many questions are only now being formulated – as we do here. And an important question is, of course: what did the government's experts in fact know about the rich resources that they so confidently helped discarding? What if they designed policies from ignorance?

In the *chapters 2-4* we consider part of this complex question of the 'rupture', seeking to re-enter a space where the many alternatives for agriculture and food production, that we yet lost sight of, can become visible again. We follow a historic-exemplary approach, with socio-logical focus and natural scientific expedients, to rediscover (a) the 'ruralities' where, throughout history, food production was situated (b) the rich natural resources that peasant and small farmer there had at their disposal. Information from soil science and broader natural sciences, as acquired outside government-dominated circuits, will prove a great help.

The chances of tragedy

Especially in the decades that were most formative as to our agricultural policies, the 1950s and 1960s, mankind was still at one in its great expectations of 'science and technology'. Yet exactly because this was part of the common ideology, in all the enthusiasm it was no easy matter to look closely at one's own research or that of others. There were projections in the air (e.g. 'weather modification') about which people in more modest times would just have shrugged their shoulders, and that yet in those years could become part of government policy. And all primarily because, in the ideological atmosphere of the age, it all seemed self-evident. For the present it means, that we simply do not know what is virtual and what is real, in all of those impressive institutions that we built with such fervor, in those post-war decades. Government- and industry-bound institutional research experienced such an accelerated growth, that its experts soon outnumbered the combined number of scientists that ever had lived. And all were convinced that they were building a better world. This surely was not just power play: these were the days in which even most economists were broadly convinced of equity issues!

Yet it did not take long or some big, man-made problems showed up. And quite discouragingly, these were problems cáused by the new policies and the new technoscientific efforts. But these same policies had opened-up the economy for big actors, and they were steadily expanding their power in the agricultural and food sector. We entered some confusing decades, in which policies of governments shaded over into policies of transnationals. As it was, Western governments, as well as transnationals active in the agro-sector, had great stakes in the 'industrialization' of agriculture, as it got institutionalized in the 50s and 60s. Hardly a miracle that evaluations, though necessary, had a hard time.

With the postponement of evaluation, the probability of tragedy could only grow. For what can be mended when evaluation is timely, leaves us with only a *fait accompli* when it is post-poned. And that *fait accompli* can by then be the devaluation of a lifetime's labors. Added to that, for many actors power issues will soon loom large – what can only deepen the tragedy. For what if our proud constructs lack a foundation, and prove rather empty of contents?

The need for evaluation

In the course of the present investigations I got convinced that our long post-war half-century is indeed an Age of Tragedy. Not tragedy stemming from deprivation and violence, as people in the first half of the 20^{th} century had experienced it. Many of them saw their lifetime labors end in flame. Or they experienced worse even. But ours is the tragedy of first entering a presumed Kingdom of Man, and then seeing it crumble, because our constructs lack quality, after all. It is not only that we are facing problems that we know we can't solve, but that we realize they are of global extent, like the war- and safety-issues, or the food scarcity. Because they are global, we know we can't escape the confrontation. That then can bring us to evaluate, at last. With evaluations in the field of agricultural policy and research being most urgent: food scarcity is as real as the fact of our greatly decreased global food stocks.

Something went greatly wrong, and we better take some tenacious efforts in probing what it was, and where. What did we presume we had while it was not, and what did we discard that we in fact needed? One thing we presumed we had was the certainty of a high-yielding agriculture - but it was not. What we discarded was a food supply coming from the peasants and small farmers with their agro-ecologies and rural communities – and we did not even look what in fact we discarded. So in ch.4 the focus is especially on the peasant's/small farmer's (human and natural) resources, those we discarded, but that yet, from an approach that allows farmer & ecology once more to take center stage, can become available again. In ch.5 the focus is on evaluating that what we thought we had, both as to knowledge, and as to the presumed achievements of our industrial agriculture. Both ch.4 and ch.5 are major chapters, in which I concentrated quite a large amount of background literature.

Then in the chapters 6 till 9 we will take a closer look at this strange post-war hybrid of agricultural research and government.
From where the sudden, exclusive, focus at high-fertilizer breeding (and hybrids), in the US first of all? From where this idea of 'agriculture by design', an agriculture from the 'drawing boards' of distant experts in far-away institutes (ch.6)?
What are the roots of this assumption of industrial fertilizer's supremacy? Why did mainline research break away from the proven 'organic' practices and follow fertilizer's narrow trail instead? Where did it break away from careful soil research (ch.7)?
An 'agriculture from the drawing boards' is bound to deny the decisive contributions of soils, plants and farmers, and to posit a 'clear and distinct' world that obeys the expert's generalisations instead. But the earth and its soils are simply not like that, and we will take a close look at that fact in ch.8. The approach was completely mistaken, and we will review the disarray that fertilizer-intensive agriculture now is in. Because mainline agricultural research was, apparently, isolated from other disciplines, it did not perceive that it was a stranger to soil, plant and farmer. We will take a closer look at this isolation and its consequences, the loss of perspective.
Yet, how was it that this one-dimensional choice for high-fertilizer cropping, after its war-time introduction in the US, first found its way to Europe, then all over the globe? For sure, America's relative isolation ended with the war, and Marshall Plan Aid constructed massive bridges. But why did Europe soon 'round up' its own farmers (ch.9)? Is there not some connection with the war?
To give a start with answering that last question, we take in ch.1 already a short look at UK agriculture in wartime, while chapter 9 we probe somewhat deeper in the wartime US situation. Both indicate that indeed the war itself was a turning point, not the least because of wartime economic and power concentrations that were adverse to the small farmer working with local natural (and communal) resources. Then in ch.10 we look more closely still at the wartime situation in the Netherlands, and its aftermath.
Now Dutchmen after the war were quite like the citizens of other nations under Nazi occupation, in that they did all their best no more to think of those horrible years. But the predictable result of that attitude was, that they dragged along much more of the war than they imagined. Especially as to agriculture the burden of war was a heavy one. Sure, ruined farms there were many, but these were being repaired, even if often with great delay. But the real burden that was to last were the wartime power concentrations and occupation decrees that shaded over into post-war decades.
The last chapter, ch.11, is not a summary, but once more poses some central questions about the real-life value of post-war developments. It includes a positive note (as in ch.4): an indication of one of the natural resources that the peasant/small farmer has at his disposal, biological nitrogen fixation.

1. Sociological framework

and

Research viewpoints

Bibliographic note

An important aim of this thesis is to supply the reader with enough bibliographic information to allow him some independent exploration of the literature. Sometimes that is because the subject at hand is of rather central importance, yet, more often that is because in the course of my researches I discovered that some subject up till now had at best been covered very insufficiently. To facilitate research in such subjects, I took pains to make the literature more accessible.
That is especially so where the historical treatment of subject matter is wanting, or faulty. In those cases I considered it my obligation to help the reader in general, and the young researcher in particular, to explore a period of history that is easier for me to survey, given the span and course of my life. The reader will find most bibliographic information in condensed form in frames with smaller type distributed throughout the text. It stands to reason that references have been added only after I had personally considered their relevance to the reader.

About this chapter

An introductory chapter is the one that is written last, and up to a point this introduction conforms to that rule. Yet, the need for a historical and sociological framework was clear from the start. An extensive proposal for such a framework was the subject of incisive discussion after the first years of explorative research in the diverse subjects that pertain to the overall project. After that, research into the specific subjects could acquire its definite form and substance.

By then the general framework had definitely become visible – and the picture it offered was not a quiet one. Post-war urbanization/de-ruralization is a historically unique upheaval that is connected, in complex ways, with the preceding period of the 'Second Thirty Years War' (WW I – Interbellum – WW II). Peasants all over the globe have been evicted from their land, and again there are puzzling connections with the war and with e.g. pre-war colonial policies. Last but not least, research became a mega-enterprise as a consequence of the war, and it retained much of its war-like character because since then it has been directed by the centres-of-power towards pre-set goals.

Evidently, also in regard to science and research a critical stance was indispensable, but where to find an 'Archimedean point' in the midst of so much turmoil? No doubt a socio-logical approach was needed that implied a definite distance (≠ distantiation) from the present scene. Some presumed 'social science citation index' would be of little help, yet, there proved to be ample tools and materials around for a thoughtful sociological approach (cp. References).

Post-war technocracy is a historically unique project to rule all of nature and society from the bureaucratic centres (government and/or industry). Monolithic from the start, it soon dictated the reductionistic kind of S & T it deemed fit for its purpose, and took care to have it instit-utionalized, to the exclusion of approaches that are incompatible with its central aims. Its experts soon outnumbered the sum total of scientists who had gone before, its institutions acquired an impressive visibility, and its regulations covered the utmost corners of the country. With post-war agricultural policy technocratic to the core, an analysis cannot but start with a close look at the main components of this impressive technocratic system. We will take a historically informed look at the system's expert and his specific kind of rationality, as well as at the kind of science that is cultivated and used in its institutions.

The post-war system originates from a very specific, historical situation, so it stands to reason that we will pay attention to this broader history also, a history that we need anyway to arrive at the sympathetic kind of understanding of the post-war half-century that is a prerequisite for a true evaluation. In the course of those preliminary investigations we will see the farmer and the ecology come out of the shade in which post-war research and policy had jockeyed them away.

All-in, we will introduce a 'language' that, however imperfect, respects farmer and ecology as irreplaceable participants in the discussion about agriculture. The floor is open.

1.1. Which rationality?

As Karl Mannhein pointed out in 1933/35/40, both industrialization and bureaucratizing are founded on, and promote the progression of, *functional rationality*, meaning (I quote the 1949 reprint p.53; cp. the 1935 ed. §§ I.5 & I.6)

> '*a series of actions is organized in such a way that it leads to a previously defined goal, every element in this series of actions receiving a functional position and role*'.

This functional rationality, once it has become the dominant approach, 'by no means increases' (Mannheim l.c. §I.VI) *substantial rationality*, defined as

> '*an act of thought which reveals intelligent insight into the inter-relations of events in a given situation*'.

Quite to the contrary, functional rationality (l.c.)

> '*is, in its very nature, bound to deprive the average individual of thought, insight, and responsibility and to transfer these capacities to the individuals who direct the process of rationalization.*
> *The fact that in a functionally rationalized society the thinking out of a complex series of actions is confined to a few organizers, assures these men of a key position in society*'.

Post-war agricultural policies, as major post-war bureaucratic projects aiming at agriculture's supposed industrialization, were ruled by this 'functional rationality', and that rule included the efforts at evaluation of its policies. Any substantial evaluation (*sensu* Mannheim) would have required a substantial criticism of the policy (the 'product') itself, within the broad context of other approaches to agriculture. However, the policy started by declaring itself superior, that is, it started with the rejection of the frame of reference that it needed for its substantial evaluation.

As oil is running out we suddenly start realizing that this lack of substantial evaluation is also part and parcel of our transport policies and other core projects of post-war government. We simply rejected any alternatives to our policies based on the availability of cheap oil, even though long-term policy required their consideration and active exploration, then as now (cp. Schumacher 1964). Half a century of inadequate evaluation of 'cheap oil' then led to greatly magnified *path dependency* of our present transport and agricultural policies. We built impressive 'institutions' – which might be suspended in mid-air. The prominence accorded to industrial fertilizer is one of them.

Its large-scale introduction was in many ways a government project, with the government experts not thinking much of the farmer's committed labour on the land. With fertilizer presented as doing perfectly what the farmer could do only imperfectly, the demise of most farmers seemed 'self-evident'. The concentration of land in just a few hands followed, as did the distant processing and distribution of agricultural products.

If in-depth evaluation of the use of fertilizer reveals that it leads to disastrous consequences - long-term loss of soil fertility being one of them - we are thrown back to the very beginning. The moment that the massive introduction of fertilizer proves to be a problem, the whole system with its large-scale distributors, centralized processing and industrialized crop growing, will fail. At that point the farmer with his caring approach towards the local environment will become important again.

Yet, those institutions and economic powers in the sector of food and agriculture that owe their existence to our fertilizer-based growth policies will still derive their '*raison d'être*' from the supposed 'fertilizer power'. Functional rationality has been very effective in institutionalizing this 'power', this seductively simple concept that relieved both researcher and policy maker from a close consideration of the soil and of its care-taker, the farmer.

Mannheim's distinction of functional vs. substantial rationality was well known before and after the war, certainly in the Netherlands where he lectured in 1933 and where his '*Mensch und Gesell-schaft im Zeitalter des Umbaus*' was published (Leiden 1935) (cp. Woldring 1986 Ch.XV). We find a very clear exposition of the issues in Bouman's 1947/48 university lecture (Bouman 1948/67, esp. p. 206f.). With the English edition *Man and Society in an Age of Reconstruction* published in 1940 (6^{th} reprint in '49) and Mannheim the best-known sociologist of those years (Hughes 1977 p.418f.), we have ample reason to take a close look at post-war government projects.

> Mannheim's distinctions are essential if we want to scrutinize the greatly increased powers of bureaucracy, as it originated in Depression, war and Reconstruction. Max Weber gave an in-depth analysis of bureaucracy in his '*Wesen, Voraussetzungen und Entfaltung der bürokratischen Herrschaft*' (Weber …), and we have seen his gloomy predictions come true, not only in the Nazi bureau-cracy, but also in the exceptional growth of technocracy everywhere. It was Mannheim who gave us the concepts that we need to evaluate those developments, but as an elaboration of Weber's analyses (for a very good exposition see Mommsen 1989a, 1989b). Surely Weber already gave us some hints, e.g. when he wrote '*Die Bürokratie verbirgt ihr Wissen und Tun vor der Kritik, soweit sie irgend kann*', implying that S & T, when allying themselves with bureaucrcay in technocracy, change radically in character (also Bouman, l.c.).

Note that not only our agricultural policies are part of our post-war *growth economy*, but so are e.g. our transport policies. This 'growth' was essential to the central government project, yet, it was not just imposed by the government, but it was also as the expression of the *modernization ideology* which inspired us all. That combination makes us realize that in current research we are likely to get entangled in some closely woven nets. In spite of the fact that those nets were woven with threads that lacked substance - e.g. in a historical sense – they contributed substantially to the prevention of discussion and evaluation. Wehling (1992 S. 32, 31, 35) writes that *theories of modernity* - the 'sociology of the 60s' (Giddens) - developed

'*im Erfahrungshorizont eines scheinbar krisenfreien und 'harmonischen' ökonomischen Wachstums, das von einem stabilen politischen Konsens getragen schien. Der Annahme [war], daß damit lediglich an die 'normale' Entwicklung industrieller oder moderner Gesellschaften wiederangeknüpft worden sei, die zwischen 1914 und 1945 durch mehr oder weniger externe Störfaktoren lediglich unterbrochen worden sei …*'

'*Die Theorien der Modernisierung (und die verwandten Konzepte der Industriegesell-schaft) haben nach 1945 in den westlichen Ländern Funktionen der gesellschaftlichen Sinn-stiftung übernommen. … Wesentlicher Effekt der Theorien der Modernisierung und der Industriegesellschaft war es, nach zwei Weltkriegen, nach Nationalsozialismus und Stalinismus ein Bewußtsein von der Kontinuität und Normalität der "modernen", "industriegesellschaft-lichen" Entwicklung sowohl im (sozial-)wissenschaftlichen Diskurs als auch im Alltags-bewußtsein mitzubegründen und zu verstärken*'.

'*Nach dem Ende des Zweiten Weltkriegs konnte sich "im Widerspruch zu aller historischen Erfahrung in Europa ein primär an Entwicklungskontinuität orientiertes Zeitbewußt-sein durchsetzen" (Lutz). Möglich war dies einerseits vor dem Hintergrund eines mehr als 20 Jahre lang – zumindestens an der Oberfläche – stabilen, krisenfreie und kontinuierlichen Wirtschaftswachstums, an dem erstmals in der europäischen Geschichte die große Mehrheit der lohnarbeitenden Bevölkerung materiell beteiligt wurde. Auf der anderen Seite wurde, vor allem in Deutschland, aber auch in den anderen europäischen Ländern, eine auf Norm-alität und Kontinuität zielende kollektive Bewußtseinslage kräftig unterstützt durch die Sehnsucht nach der Verdrängung von Nationalsozialismus und Zweiten Weltkrieg, von Stalinismus und dem Abwurf der ersten Atombomben. Theoretische Normalisierungsangebote bedienten in* *dieser Situation die Verdrängungsbereitschaft und den Wunsch, zum 'business as usual' überzugehen, als sei nichts geschehen'.*

With 50 million dead and 50 million adrift, we understand the urge of Europeans in post-war years to 'make sense' by suggesting those horrible years of totalitarian destruction were outside 'normal history'. But likewise do we understand the futility of the effort.

In post-war decades an all-out effort to 'modernize' society was taken for the 'normal' course of history, with the war a deplorable break in this course. This all-out effort had the accelerated growth of government- and industry-related research as a core element. Yet, the choice for *functional rationality* isolated the effort from its human and ecological contexts, and the starting point in the denial of recent history was irrational. In that way, evaluation was well nigh impossible. This lack of evaluation next caused that the rise of the consumer society in Europe was taken as proof of 'modernization'. Wehling explains how (l.c. S.36):

> '*Das "Kontinuitätsparadigma" industriegesellschaftlicher Entwicklung half nach 1945 die Vergangenheit auszugrenzen, zu "beschweigen" und zu neutralisieren und neue Erfahrungen in Deutungsmuster einzuordnen, die gesellschaftliche Normalität versprachen. So konnte etwa die beispiellose Veränderung der allttäglichen Lebensformen und Konsummuster als Angleichung an die "Modernität" der USA gedeutet werden. Aus dieser historischen Konstellation erklärt sich … zum großen Teil die "alle Schichten durchdringende, vielfach naïve, noch echt fortschrittsgläubige Bejahung des industriellen Wandels mit allem, was er an sozialen Zumutungen, auch an damals schon erkennbarer ökologischer Belastung mit sich gebracht hat" (*Schwarz*)*'.

Before long the 'modernization theories' filled the horizons of social scientists everywhere. In books like '*Modernization: the dynamics of growth*' (Weiner (ed) 1966) it is admitted that the term '*is so loosely used*', yet, it is interpreted enthusiastically as signifying '*a comprehensive process of change which Europe and America once experienced and which is more than the sum of any small changes*' (l.c. Preface). 'Modernization' is extended to all spheres of personal and social existence, in an attempt to impose a full-blown, but flawed ideology on society.

1.2. (High) Modernity/modernization. Some aspects.

That imposition would have been in vain, of course, if it had not resonated with convictions in the population at large. Wehling points to some decisive aspects (l.c. S. 32),

> '*Auf der Ebene von 'Alltagstheorien' begründete das Kontinuitätsparadigma ein hohes Maß an Zukunftgewißheit und trug zur Entproblematisierung des gesellschaftlichen und technischen "Fortschritts" bei; mit der Annahme einer industriellen Eigenlogik legitimierte es zugleich Sachzwang-Argumente in der administrativ-politischen Praxis und bot einen scheinbar stabilen Orientierungsrahmen für flankierende Planungs- und Steuerungs-konzepte*'.

The welfare state offered greatly extended 'certainties', but it came with a price, the assignment of a central place to the 'expert' in administration and society. In Wehling's words (l.c.):

> '*Damit erwies die Annahme einer "Kontinuität im Wandel" sich auch als äußerst förderlich für die erfolgreiche Etablierung sozialwissenschaftlicher Verfahren der Erklärung, Prognose und Planung in den verschiendensten gesellschaftlichen Bereichen. Das Professionsinteresse von Soziologie, Politik- und Wirtschaftswissenschaften verschränkte sich mit ihren gesellschaftstheoretischen Grundannahmen. Vor diesem Hintergrund konntn sozialwissenschaftliche Erklärungsmuster oder Versatzstücke sozialwissenschaftlicher Theorien auch zu Elementen der Konstitution des Alltagsbewustseins werden.*

> *Im Horizont derartiger Normalitäts- und Kontinuitätsannahmen verlor die Frage nach den spezifischen, historischen Voraussetzungen der industriel-kapitalistischen Entwicklungsdynamik nach 1945 fast jede Bedeutung. Erst recht tauchte im Rahmen des Kontinuitätsparadigmas die Frage nach dem ökonomischen und vor allem ökologischen Grenzen dieser Dynamik nicht auf*'.

Symptomatic was the blind optimism with which the United Nations proclaimed the 1960s the 'Development Decade'. Wehling states (l.c. S.109):

'*Dabei ging man davon aus, daß "innerhalb eines Zeiraumes von zehn Jahren die grundlegenden Voraussetzungen für ein selbsttragendes wirtschaftliches Wachstum … in den Entwicklungsländern geschaffen werden könnten" (Fröbel a.o.). Als das Scheitern solcher Hoffnungen sich abzeichnete, wurden die 60er Jahre einfach in die erste Entwicklungs-dekade umbenannt (ebd.); die 90er Jahre sind inzwischen die vierte Entwicklungsdekade – ausgeblieben ist nur die "Entwicklung"*'. And Dube (1988 p.104, 106) summarizes:

'*In the decades following the end of the Second World War, the Third World has experienced several changes of mood, but in respect of economic growth and technological change – or, development generally – it has moved from euphoria to despair*'. … '*Paradoxic-ally, development had made the weaker sections of society more vulnerable*'.

Wehling explains (l.c. S.126):

'*Die 80er Jahre gelten auch in den Augen der Weltbank als "verlorenes Jahrzehnt" für die Mehrzahl der Menschen in der Dritten Welt (Weltbank 1990). Die ökonomische, soziale und ökologische Situation vieler Länder Afrikas, Asiens und Lateinamerikas hat sich im vergangenen Jahrzehnt [80er Jahre] teilweise dramatisch verschlechtert; wachsende Artmut und massive Zerstörungen der natürlichen Lebensgrundlagen stehen dabei in vielfältigen Wechselwirkungen. Ausgelöst und verscharft wurde diese Konstellation vor allem durch die Anfang der 80er Jahre aufgebrochene "Verschuld-ungskrise" vieler Staaten der Dritten Welt; sie ist aber vor allem das Resultat des Scheiterns der seit dem Ende des Zweiten Weltkriegs verfolgten Wachstums- und Industrialisierungsmodelle: Die gegenwärtigen Zuspitzungen in den Entwicklungs-ländern sind "weitgehend Folgeprobleme der modernisierungsorientierten Politik der 50er und 60er Jahre" (Hein & Mutter 1987; Fröbel a.o. 1986)*'.

Faced with those troublesome developments, social scientists try to 'modernize modernizat-ion'. Eisenstadt with his 'multiple modernities' (2001a & b) offers us valuable perspectives. Most important is his acknowledgement of the destructive potential at the core of modernity:

'*Contrary to the optimistic views of modernity as inevitable progress, the crystallizat-ions of modernities were constantly riven with internal conflict…. War and genocide were scarcely new phenomena in history. But they became radically transformed, intensified, generating specific modern modes of barbarism. … The Holocaust, which took place in the very centre of modernity, became a symbol of its negative, destructive potential, of the barbarism lurking at its very core*' (2001a, p.110)

Other authors have analysed this connection with terror and genocide both earlier and more thoroughly, e.g. Detlef Peukert, Omar Bartov, Anthony Giddens, and especially Zygmunt Bauman whose '*Modernity and the Holocaust*' became widely known. (Note that we will return to the subject in Ch.10). The post-war 'history' of the self-evident progress of modernity proved a flight from history. We indeed have ample reason to take a close look at this period of 'progress' as a whole (as we shall do repeatedly). More specifically, we can no longer refrain from asking the question if one of its core elements, that of deruralization and urbanization, might a dead end?

But notice that authors like Eisenstadt, who developed a far greater sensitivity to (recent) history than their optimistic colleagues from the 60s, still tend to uphold the ‘rationality’ of a core element of (High) Modernity: that of expert-led rationalization/industrialization. The ‘constructability’ of society has been in doubt for some decades now, but to doubt the ‘constructability’ of nature would surely indicate the end of our post-war era of (High) Modernity itself. Keeping an eye on exactly this issue is at the core of the present research.

We have good reasons for such vigilance. As indicated, our post-war policies all boiled down to the same thing, they bred impressive institutions, and soon there was a common ideology in the air that made it all seem self-evident. For half a century society seemed to accept a double negation, a negation of history and the limits of S & T. From that double negation we constructed a more comprehensive version of modernity, referred to as *High Modernity*.

Scott (1998 p.88) stresses that the ensuing ideology of authoritarian **High Modernism**, which is

> ‘*envisioning a sweeping, rational engineering of all aspects of social life in order to improve the human condition*’,

was specific neither to the right nor to the left, but was the dominating ideology all over the globe.
Walters & Haahr (2005 Ch.2), in line with Scott 1998, define **High Modernity** as
a set of ideas:
(1) a powerful faith in scientific and technical progress (2) the belief that expanded production could satisfy all human needs and (3) that the social order could be rationally- and scientifically-ordered
a social movement that affirms the knowledge, identity and prestige of a particular social stratum – planners, technocrats, engineers, architects and other experts
a form of governance that is linked with ambitious, large-scale projects – the building of dams and highways, the rationalization of whole cities and economies.

> The functional rationality of the governments’ projects implied their de-contextualized character, so their ‘forceful’ imposition brought the danger of neglect (or denial) of everything that was outside the projects’ scope. Cp. J.Elsom 2007 ‘*Missing the point: the rise of High Modernity and the decline of everything else*’.

Of necessity our research into this epoch is composed of strands so varied that our piece of history will reveal something of its true complexity. As in other eras there was no Iron Law that guided us to the present, nor an evolutionary course selecting the fittest, but history was once more contingent and complex. Once this contingency dawns upon us, the institutions that were the pride of our age of High Modernity loose their self-evident character, and we gain the freedom to scrutinize them.

But note: High Modernity was **our** ideology. Therefore, ours is a rather mixed situation: we increasingly sense that the period under study is something of the past, yet, we only just started making up our mind as to its High Modernism or the institutions embodying this ideology. Understanding this period is possible only if we can keep our own reactions and counter-reactions at bay, yet, the study of the period is an emotional endeavour because High Modernism with its institutions provided us with ‘basic certainties’ for such a long time. To say that the period under study is in need of understanding, not of reaction, would be a platitude if that period was more distant, with an ideology that touches us less than the modernization ideology. Disentangling the closely woven nets of High Modernity – our communal products - is also an exercise in disentangling our own emotions.

That is a primary reason that I have chosen for an account that highlights the *historical contingencies* that structured the post-war era. Those contingencies come to light if we are ready to do some tenacious historical research, not incidentally, but methodically. What we gain with that approach it is the freedom, both emotionally and conceptually, to consider what, in the present, derives chiefly from the post-war era's *dream of the age* (*sensu* Butterfield 1949), and what, maybe, are this era's true achievements.

Maybe the true achievements are far more modest, and far less numerous, than we prided ourselves upon. After all, what distinguished our Modernization Hero from his forebears was his **refusal to face tragedy**. And exactly that made him prone to meet it.

1.3. High Modernity – some more characteristics

Post-war High Modernity refused to learn lessons from history. That way it also missed the criticism of S & T that was extant already before World War II. Wehling reminds us of one of the primary critics of those years, Walter Benjamin, who a.o. stressed that (l.c. S.101/2):

> '*Die Beherrschung der Natur vollzog und vollzieht sich als sachlich verkleidete "rationale" Herrschaft über die arbeitenden Mensch. Gegen die 'rechts' wie 'links' vorherrschende Vorstellung von Natur**beherrschung** als dem "Sinn aller Technik" setzte Benjamin bereits in den zwanziger Jahren … ein anderes Bild und ein anderes Modell rationalen Umgangs mit der Natur: eine Idee von Technik nicht als Beherrschung der Natur, sondern als "Beherrschung vom Verhältnis von Natur und Menschheit"*'.

Others came with similar analyses, pointing to the fact that our 'power' over **nature** is limited indeed, but that we, more often than not, exert our power by limiting **people** to strictly dictated interactions with nature. Denying people access to forests or water is an obvious example of such 'power over nature' (e.g. diverting water away from the agricultural areas of the Indian reservations in pre-war US, or in the Andes in present-day Peru). But note that also apparently powerful technologies have strings attached that strictly prescribe behavior for e.g. the farmer.

Offering fertilizer-, pesticide- and and irrigation-dependent seeds is an example, because it in effect takes alternatives away (think of the factual prohibition of use of local varieties in most versions of breeders' law). 'Empowering' the farmer by providing him with N-fertilizer likewise implies denying him access to (free) biological nitrogen fixation (Winogradsky 1927). Indeed, so-called powerful technologies come with strings attached, and those strings allow the 'provider' to exert power, if not over nature, then still over the 'recipient', by greatly limiting his access to and development of natural or communal resources.

The role that High Modernity assigned to 'science' established the power relations indicated. It sounded innocent enough (Rostow 1960, in Wehling o.c.):

> '*Eine traditionelle Gesellschaft ist eine Gesellschaft, deren Struktur innerhalb begrenzten Produktionsmöglichkeiten entwickelt ist, die auf vornewtonscher Wissenschaft und Technik basiert sowie auf einem vornewtonschen Verhalten gegenüber der physikalischen Welt*'.

Rostow was ready to provide the aspiring 'modernist' with the 'newtonian S & T' from the US sources – with strings attached. As is well known, Latin American authors were among the first to detect the dependencies that were implicated. But note that Rostow c.s. postulated an S & T that, because it was supposedly valid everywhere, in some miraculous way was not dependent on time and location.

The image of technology that they projected was simply wrong, and the science they pushed was reductionistic to the core. The reductionist view of ‘Newtonian science’ was so extreme that even Newton himself would certainly refuse to subscribe to it. Yet, minds were under the spell of this ‘newtonianism’. Schultz, the well known American agricultural economist, was one of them who were willing to admit that ‘traditional’ agriculture had wrought great things with its limited means, but that it was unable to progress any further and that it was now up to ‘modern S & T’ to surpass them. So centrally developed ‘expertise’ was offered as superior to local knowledge, resources and experience. This implied the inferiority of the farmer’s expertise and the lack of perspective that working with local resources could offer. Neither of these two view-points was true to the life of soils, plants, and rural communities, yet, they were decisive in shifting the power to the government/industry with its experts.

High Modernity was attractive also because it promised to bring, with one big sweep, a better future to the weak. Its break with ‘tradition’ made sense especially where tradition implied the oppression of women and the poor. For oppression can be at the very core of a ‘traditional’ family, something that brings Ramachandra (2003) to his bitter remark: ‘*The romantic image of close-knit Third-World communities conceals the incestuous relationships and massive oppression, especially against women, that the typical ‘traditional’ family embodies*’.
Yet, after half a century we became ‘sadder and wiser’, for it is emphatically Modern Man who is the hero of High Modernity. He is praised for his ‘mobility’ and ‘flexibility’, and many feminists now point to the fact that it is especially women and children who are his victims. They are not just left to fend for themselves, but their mutual care and constant, close relations, though fundamental to society at large, is marginal to Modern Man. High Modernity did not bring the ‘liberty to care’, but once more constructed an economy and society ‘without a heart’. Many an ethicist stresses this very point and urges the introduction of a care-based ethics at the very core of the economy.

> But note that High Modernity’s loud praises for Modern Man can hardly hide that he is uprooted. More than anybody else Simone Weil is known for her incisive analysis of this aspect, and for her vivid description of the re-rooting tha we all need (the young farmer especially).

Quite generally, without fitting in with the local community and a proper attitude it is not possible for humans to grow. Only then can we grow grow in experience and make sense of life. Already Walter Benjamin pointed at this side of modernity (Wehling o.c. S.82 f):

> ‘*Erlebnisse fügen sich nicht mehr in Sinn- und Deutungskontinuitäten ein; sie bleiben zusammenhangslos. … Der Verkümmerung der Erfahrung, ihrer Verdrängung durch das Erlebnis, entspricht die Ablösung der Erzählung durch die Information. Die Information, eine Form der Mitteilung, die mit dem Hochkapitalismus aufkommt, befriedigt nicht nur ein anderes Bedürfniss als die Erzählung, sie konstruiert auch eine andere ‘Welt’ als diese: eine Welt trügerischer Nähe und Verfügbarkeit, die gleichwohl gegen die Erfahrung der ‘Informierten’ abgedichtet ist*’.

Indeed, High Modernity shows a spectacular growth of ‘information’, but it is quite generally at the expense of experience. It promises a ‘knowledge-based society’, yet, it makes it hardly possible for people to connect with the real world of people and plants.

A human being is only able to learn (in any real sense of the word) in close interaction with others and with the world around. Much of this was well known in the Interbellum, thanks to the labors of dialogical/personalist philosophers and educators like Buber, Berdjajew and Kohnstamm. Then after the war, High Modernism made us forget this 'dialogical' character of human knowledge and experience - until a counter movement set in.

> E.g. in linguistics has been focussing for years on 'usage-based language acquisition'. Books like Per Lindell's '*Rethinking language, mind and world dialogically*' (2009) show the way to the wider implementation of the dialogical approach in education and social science.

Yet, with government and big business the faith in the knowledge/information-based society is unbroken. They want power over 'knowledge', so their first the first step consists in severing local bonds and emphasizing that locals do not have the 'knowledge' that is at the core of High Modern-ity's projects. And of course, only after this bond with specific people and places is severed, 'knowledge' can be treated, parcel-like, as 'intellectual property'. But note that our proud products such as 'knowledge society' and 'intellectual property' make no sense in a world where real knowledge is experience-based.

1.4. Some more chief concepts

We continue with introducing some more of the main concepts and subjects of this research project, and its wider framework. Within that (historical) framework we will consider the *action settings* that make up post war agriculture & agricultural policy, with the transformations of the farmer's resources and environment central to the imposition of policy. The utter disregard for the *ruralities*, which had always been the integrative loci of the food production, led to their disintegration. This is characteristic for the *ecological regime* that got imposed. It was an imposition based on the *technocratic* conviction that was at the core of post-war government projects. When 'nature did strike back' and the technocrats proved increasingly at a loss in regard to real-life nature and ecologies, their technocratic social-economic order in combination with its impoverished ecology was yet in place, enabling economic actors and policy makers to pursue their own ends. We entered the age of Thatcherism and Reaganomics with its increasing financial-economic and ecological anarchy – which on the other hand was also the age of re-discovery of agro-ecosystems and ruralities.

Figuring prominently in this thesis are agricultural research and its *experts*. In spite of suggestions to the contrary, also they were/are greatly influenced by their specific framework, that of post war High Modernity. The link is the *bureaucracy*, which with its great expansion - in wartime and in post-war decades - also induced an accelerated growth of *centralized* research and *institutionalized* expertise. For half a century High Modernity was the driving post-war *ideology* all over the globe. Being an active project of governments everywhere, it had an army of experts at its disposal to spread its gospel of modernization (modernization theory, economic growth theory, development economy), ánd the powerful means to shape society according to its doctrines.
It had its *roots in complex ways in the war* - sufficient reason to wonder about its peacetime qualities. With war and *sustainability* at cross purposes, the issue of the sustainability of High Modernity's projects in general, and of its *'industrial' agriculture* more specifically, is a pressing one. With so much expertise bound up – largely from conviction – in the government's (and big enterprise's) centralizing projects, little attention was paid to the *small & local* aspects. They were effectively treated as unimportant, even as a hindrance to

progress. And yet in all of ecology and human life things really happen on a small and local scale.

> An obvious biophysical illustration is the complete dependence of life on *interfaces*. Organisms and soils all consist of *complex hierarchies of constituents* that with their *fractal structures* arrive at dazzling interface areas, especially in the (sub)microscopic realm where (ultimately) most flows of energy and materials on this earth are effected. Our high-energy high-flow industrial methods are complete strangers there, and we acknowledge that much by grudgingly sealing them off in carefully isolated structures. Yet post-war governments with one mind pursued High Modernity's centralizing big-power projects as modeled on those 'factory methods'.

As in all periods of history there is also a big element of tragedy in the long post-war half-century. Not just in the obvious discord between the inscription on the statue in front of the UN headquarters '*they shall beat their swords into plowshares*' and the grim reality of the armaments race and the increasing proliferation of small arms (Wuramantry 1987). But it is more generally due to this period's determination-from-conviction to open up a future of plenty for everybody and the means to achieve this – which are not compatible to man & ecology.
Here it is not *technology* that is the major problem. After all, as most of technology is concrete by nature, its real-time specialists are focusing on practical applications, acknowledging the limits of such applications at the same time. Only its imaginary-time specialists whose trade is technology's 'limitless applications' and who design grand projects unencumbered by social and ecological boundaries are a real problem. When economic policy took those imaginary-time technologists for the real-time ones, economists and policy makers at both sides of the Iron Curtain lost contact with the real life of people & plants.

And so we have to face the possibility that during this half-century a vast army of experts laboring in impressive research institutes was trying to stand the world on its head, in decisive aspects. We experienced that much when experts in government institutes proposed 'scientific fishing quota' which subsequently led to the collapse of fisheries. It was a painful way to learn that experts, who cannot accept the limits of their expertise, usually fail in achieving their fairy-tale objectives.
All of our bureaucracy-related experts were supposed to prepare the government (and big enterprise) in its projects-of-power. Wartime research was taken as proof of such research and expertise and spokesmen for S & T who were open about its weaknesses and limits were as a rule considered soft-headed by the bureaucracy. Unrealistic constructs, both in theory and in practice, could have the day. To give an example: now we are forced to think things over because oil is running out, we rather sheepishly have to admit the utterly wasteful character of our transport and building infrastructures (our cars with fuel efficiency below 2%, our high-rise buildings with an impossible energy intensity).

Tragedy is always a grave element in the lives of people and society. To accept that one's efforts were in vain, or even destructive, in spite of the fact that they were largely based on conviction, is a grave element of tragedy in personal life. Likewise, for societies to see their proud constructs crumble is tragic (both aspects: Heering 1961). Yet whereas in other ages the possibility of tragedy was acknowledged at both levels, our post-war age very soon rejected this idea ('*an insensitiviness to tragedy*' ruling the mind '*so that to believe in tragedy requires for us a strong effort of will*' – Oldham 1948 p.41).
When Tinbergen, the 'friendly technocrat', saw environmental problems mounting he was perplexed. When the rich countries killed off his beloved New International Economic Order

(McGinnis 1979), in spite of its endorsement by the UN, he was shattered, especially by the discovery that it was the technocratic approach itself that was responsible for the course that events could take (‘technology transfer’ the Achilles heel, e.g. Montassir 1980).

As to agricultural policies, no other approach than a technocratic one was even considered. This is truly puzzling: as historical agriculture had always been embedded in the local community, soil and ecology, the decision to re-design it henceforth from a distant center was literally an effort to turn the world upside-down. Yet, in post-war years the technocratic, centralized approach reigned supreme everywhere: the conviction on which all these efforts were based was strong and its institutionalization massive. As a result even the evaluation of some of its central suppositions is still lacking. Such evaluation, that of necessity has important natural science components, is an essential part of present research. It will illustrate that tragedy is a strong element of these post-war agricultural policies and agricultural research projects as well.

Post-war economics and economic policy are doubtless deeply tragic. Not only because Thatcherism and Reaganomics were able to overrun the equity-conscious policies of, say, people like Beveridge and Drees. Not even because the end of the supervision of international finance – with the end of Bretton Woods – initiated a turn towards a destructive global economy, which even for a generation that had submitted to the ‘need’ for vast investments in the armaments industries, was disgusting (sex industry investments, semi-slave labor in ‘special economic regions’, food speculation). The tragedy in a way goes deeper still, for it concerns the whole of post-war mainline economy and economic policy. J.K.Galbraith recently spoke of

> ‘*the nearly complete collapse of the prevailing economic theory*’, adding
> ‘*It is a collapse so complete, so pervasive, that the profession can only deny it by refusing to discuss theoretical questions in the first place*’.

No doubt the current Recession teaches us some painful lessons. Policy makers are at a loss, now their former trust in ‘growth’ policies, as advised and administered by mainstream economists, proves unfounded. For the first time in about half a century the leading position of mainstream economists – with their background in neoclassical economics - in policy making is questioned. Until very recently this leading position was safeguarded by what (also) Boland termed (1997 p.285) the ‘*imperviousness of classical economics to criticism*’.

For sure, there are other disciplines too that refuse to discuss their concepts and methods. Quite generally such an attitude signifies that they have come into a dead end and need ‘power politics’ to ascertain their ‘supremacy’. Part of that power play is professionals emphasizing that they (only) are the ‘neutral experts’ – a sad example of experts refusing accountability.

> Boland pondered this refusal, and intimates that there is ‘*a more insidious reason for the imperviousness of neoclassical economics*’. He explains: ‘*What kind of student is attracted to neoclassical economics? Clearly, anyone who decides that it is in their best interest to be selfish would find that neoclassical economics provides a powerful justification of their selfishness. This is not to say that everyone in the mainstream of economics is selfish, but only that it is all too easy to identify colleagues who are very skilled at using their neoclassical explanations to deflect challenges to their selfish pursuits*’. **Evidently there is no escape from those value issues that are part of concepts and methods at the very core of the sciences**. Simone Weil made her choice: ‘*La vraie définition de la science, c’est qu’elle est l’étude de la beauté du monde*’ (1949 p.329).

The tragedy of our post-war economic policies is increasingly coming to light. But note that tragedy is the end only under the Greek pantheon of willful gods. If we instead admit we are living under a 'moral economy' (*sensu* Scott c.s., Edelman 2005; see also Stackhouse et al. (eds) 1995, Lutz 1999, Ullrich 2001), there is also the possibility to reverse former wrong choices, in the case of agricultural policy those denying the needs and rights of peasant and ecology. If we do that, we will (re)gain sight of a reality unknown to our technocratic economy and economic policies, yet substantial enough for people everywhere to deal with. To give at least some indication of what that 'substantial reality' would be like is also part of the present research project.

At this point, this research project is, of necessity, linked with other research programs. That is the more important because much of agricultural research and policy has lived a life of comparative *isolation* for half a century (cp. Keller & Brummer 2002). An isolation that in a way was inevitable, considering that its 'modern' research & policy started from a *rupture* with 'traditional' agriculture and agricultural ecologies.
An approach within a wider frame of research, wider especially in a historical sense, will soon show that post-war High Modernity always was connected with such ruptures. One of the results was that it missed out on essential scientific heritage and developments. To recover those as well we need a balanced *historic approach*. Remember that 'a-historic science' is one of High Modernity's constructs: for science as a human endeavor the dictum of the late prof. Hooykaas (famous science historian) is valid, '*There are no sciences, only humanities*'. The many publications that science as a profession left behind will prove a great help in this historical approach to the social and natural sciences.

As indicated economic policy in the present is now bearing the burden of 'collapsed theory'. In a way it is a relief to note that. For agricultural economic policies are strangling agri-culturists everywhere and the consciousness that they are based on the wrong foundations sets us free to look for more substantial ones. Because Galbraith's judgment of mainline economics has been the concern of many of his colleagues for decades, this will also not be that difficult.
But note that post-war governments, from their assumption that the (political) center with its experts could 'essentially' regulate all of nature and society, had to shrink the world (conceptually and otherwise) until it looked fit for their regime. The deterioration of the landscape and the ecology was part of the deal.

1.5. Ruralities – regaining the focus

Agriculture after World War II experienced a far-stretching transformation ('*heroic*' sensu Bulgakow 1909), first in the West, but soon also elsewhere on the globe. Though generally presented as a triumph of agricultural research and technology, there nevertheless were profound losses at its core. Among these losses the accelerating marginalization of peasant/ small farmer and rural community, and the ongoing loss of agro-ecologies and biodiversity figured prominently. If we (as van der Ploeg did in 1997; cp. Ramakrishnan 2003) define *ruralities* as

> *the local communities cum anthropo-ecologies where the methods of the peasants/farmers were/are in tune with local, natural and community resources in shaping their diversity of farming systems*,

then the afore-mentioned transformation started with the denial of these ruralities and resulted in their ongoing decay and/or destruction.

Such denial has a history, from Frederic the Great's concentration of blacksmithing and other crafts in the towns – leaving villages debilitated – to Ceausescu's 'policy of systematization' (Deletant 1995) that included the bulldozing of small villages. It is a history closely related to the effort to increase centralized power and to subject the 'distant' rural populace directly to it. As Busch reminds us of Ceausescu's goals:

> '*by destroying villages and neighborhoods, [he] was able to destroy nearly all social networks other than those linking people to the state…*' (Busch 2000 p.78).

These historical links suffice to make us attentive at the political character of the post-war 'modernization' of agriculture. Ruralities in effect got dismantled as a result of conscious policies. In the Netherlands a chief element of these policies was not a direct assault but ongoing negative propaganda ('*there still are too many farmers*'). This was done to justify ruthless large-scale land consolidation policies that undermined the ruralities' resource base. In this way the government and later other powerful economic forces were enabled to wield centralized power.

The presumed triumphs of agricultural research & technology notwithstanding, we have perceived for some time now that something is amiss, due to the fact that landscapes did deteriorate and biodiversity did get lost. We toy with the idea to appoint the peasant/small farmer park-keeper of the old agricultural landscape (if it did not get lost completely in the mean-time), but as to agriculture and food production we deem his time past. Yet if we really think about it, that opinion again stems from the much-touted triumphs of industrial agriculture, and not from e.g. any thorough considerations of sustainability. Just consider the utterly wasteful character of our pesticide use (Pimentel 1992):

> '*the heavy use of pesticides, especially in developed countries, is having widespread impact on aquatic and terrestrial ecosystems. Worldwide an estimated 2.5 billion kg of pesticide is applied to agriculture. Yet, less than 0.1% of this pesticide reaches the target pests, with the remainder negatively affecting humans, livestock, and natural biota. Just in the U.S., it is estimated that pesticides cause $8 billion in damage to the environment and public health each year. Millions of wild birds, mammals, fishes, and beneficial natural enemies are destroyed because of the recommended use of pesticides in the U.S.*'

If we further take note of erosion and eutrophication, we see that this industrial agriculture is unsustainable, and is quite likely to remain that way.

As it is, after nearly half a century of neglect we started regaining the love for the old agricultural landscapes, and we really started to worry about the loss of biodiversity and local ecology. Yet if, up till now, we haven't connected those grave issues with the question '*whither with agriculture & food production?*' that is because we also love the thought that 'modern agriculture' is both more productive and more reliable than its predecessors. But that thought is again stemming from the presumed triumphs of industrial agriculture, as a moment's reflection will make us aware (e.g. Norgaard 1989, Brush 1989), and from our, only too human, preference for good tidings. In plain fact, we are confronted with the great loss of ruralities (as locus of agriculture of old) on the one hand, and with the inherent lack of sustainability of modern, industrial agriculture on the other. That is, if we are really serious we have to admit that here is no excuse for our self-congratulatory attitude, because it is quite conceivable that we are going to end up in a loss-loss situation.

For decades the common supposition that industrial agriculture was the ultimate ideal, and incomparably more progressive than its traditional counterparts, precluded applying any research outside its narrow confines. **With its research structure geared to specialized, centralized institutions, there were hardly any incentives for its experts to take a fresh look at the whole**. Quite to the contrary: during its post-war accelerated growth under strict government direction it became populated with a new wave of experts, each taking his pride in his part in the extension of the 'industrial' agricultural system. When this system was considered final, history also got rewritten from its point of view.

Just an example: the organic matter-centered practices of traditional agriculture were not analyzed as a source of information for further development of agriculture. Instead they were considered essentially on the basis of their potential to deliver our industrial nutrients, the components of our mineral fertilizers (as will be documented form Ch.3 on). Small wonder that we with our extravagant use of industrial fertilizer appeared as 'superior'. It did not dawn upon us that we were just flattering ourselves. As elsewhere in society
the 'ideology of progress', deemed self evident because of the 'certainty' of progress of science & technology, led us to believe that the latest product or development was bound to be the best.

But then, *society at large* lost its historical sense and re-conceptualized history as the linear path to Modernity. Therefore it became 'logical' to envision policies that would set us on this 'linear path'. Then within this a-historical frame of mind (and policy making) ruptures with the (recent) past were no longer regarded as a lack of prudence, but as a sure route to success. Agricultural policies, especially, started from the idea that a break with the past was inevitable. For that very reason they were regarded as the epitome of progress.
Very soon forceful policies setting agriculture on this path to Modernity were considered the hallmark of progressive policies everywhere on the globe. It became 'unthinkable' that they were in fact in need of substantial evaluation: the policy-related agricultural research and extension network was proud to be at the centre of all this Progress.

> Up to this day also first-rate authors like Duignan & Gann (1991) inadvertently work from this frame of reference. For example, they prove to be quite critical of the social engineering of post-war decades, e.g. in its housing projects. '*To give just one instance, "slum clearance" enthusiasts who rehoused the poor in high-rise apartment buildings, surrounded by green spaces, were invariably taken by surprise at the resultant rise in juvenile delinquency, crime, and drug abuse. Amongst other things, mothers living on the tenth floor of a huge block could no longer keep an eye on their youngsters getting into mischief down below, and granny had ceased to live round the corner, as in the old days*' (p.468). Yet, as to post-war European agriculture their account (p.519 f.), though lucid at times, is oddly restricted because they do not question its progress: '*Europe recovered and grew remarkably because the Marshall Plan promoted a rapid increase in agricultural productivity (helped by tractors, seeds and fertilizers), that in turn released labor for industry and established efficient investment program*s' (p.475). The 'standard picture' here precludes any substantial evaluation.

The net result is that up till recently the experts and policy makers were congratulating themselves with the progress attained, sure that post-war history proved their point. But things are changing. While Allen 2008 still represents the self-congratulatory re-casting of agricultural history that leaves no conceptual space for evaluation, Uekötter for years does real history already (e.g. 2006). Stone 2007 intimates about the self-congratulatory kind of historiography

> '*It sometimes seems as if the history of agriculture represents the last outpost of the Whig interpretation of the past*'.

This 'Whig interpretation' (*sensu* Butterfield 1931/1973) is a politically motivated manifestation of 'presentism', '*the imposition of our categories on the deeds and works of past agents* *who lacked such categories*' (Jardine 2003). Its relation with science as a profession is an ambivalent one (see Cunningham 1988 par.8 '"*Ideas" and the whiggish historian*').

Ashworth 2008 shows that this post-war, self-congratulatory, attitude expressed in historical studies of science, technology and industry, was closely linked to the political defense of growth policies, with Walt Rostow a prime example. But note that among historians at large, there now is scarcely anybody left who is defending the ethnocentrism of an Allen and a Rostow (cp. Jack Goody, *The theft of history*, Cambridge 2006). Ashworth 2008 points to the fact that such a present-day proponent of an industrial growth economy as Joel Mokyr realizes this and therefore in his '*The gifts of Athena: historical origins of the knowledge economy*' (Princeton 2004) he writes that the '*growth of knowledge … is far too important to be left to the historians of science*' (!) Mokyr's dictum epitomizes the a-historical, yet strongly political character of the concept of 'knowledge economy'. Rosalind Williams writes about this doubtful approach to history that it makes one

> '*feel like entering a wormhole that sucks us back to a time when technology meant progress, when the people who counted were a few great-man-inventors, when knowledge was defined as truth about nature 'out there', when the West ruled, and when the question 'what did women know, and when did they know it?' assumed that women were housewives…*' (Ashworth l.c.).

As indicated e.g. by the re-opening of the debate about crop yields in Medieval agriculture (e.g. Stone 2007), many an agricultural historian has definitely left the 'wormhole'. Some, without any doubt, were never in it (e.g. Joan Thirsk).

For nearly half a century we were quite content to live our lives on the presumed axis from tradition to Modernity, and we didn't think much of exploring other dimensions. We reduced the many-dimensional space of historical and contemporaneous, personal and communal life to just half an axis (only the 'modern' half of the axis tradition-Modernity was considered worthwhile). Yet, living on half an axis is not for fulfillment, and we gradually became curious again about real life in its multidimensional space. It also became conceivable that the peasant/small farmer-centered ruralities have resources and possibilities that our distant-expert-centered industrial agriculture is lacking. That then is the position from which the present research started.

1.6. Historically informed

With research no more fixed to the 'axis of progress', history could play its part again in every respect. All of us 'moderns' are prone to a condescending attitude towards previous generations. Yet, only when regaining a truly historical approach, we can do justice to them – and in a way to ourselves. I fully do agree with Butterfield (1949) writing

> '*The technique of historical study itself demands that we shall look upon each generation as, so to speak, an end in itself, a world of people existing in its own right*', and
> '[the historian] *turns the crude melodrama that some people see in life into a more moving kind of tragedy. In the last resort he sees human history as a pilgrimage of all mankind...*'.

If we can discern errors, it is neither helpful to lift them out of their context (problematic then as now), nor to pass them over lightly (which precludes mending them). An example can help to show what I mean.

Taking bottle-feeding as superior to breast-feeding was an error of the past half-century which had devastating consequences. Yet, we can only do justice to the people concerned if we understand it as part of '*the dream of the age*' (Butterfield, again), the dream of *the constructability of nature & society* (see van der Wal 1988 for a good analysis), a dream in post-war decades enveloping almost everyone due to a desperate need to escape from half a century of total war and deep depression. The optimism inherent to this dream led to the production of a 'miracle' baby formula whose actual benefit was grossly overestimated. This led in turn to the birth of those transnationals which subsequently so stubbornly promoted the use of this [objectively] inferior product. The desire to prolong their huge gains from this product was definitely part of this stubbornness, but we should realize that we all helped, from conviction, investing the formula with qualities that simply weren't there.

We are confronted here with a definite characteristic of the post-war decades. Faith in the constructability of nature & society got *institutionalized*, leading to adverse recommendations to young mothers about breastfeeding that were contrary to both traditional infant feeding practice and a lot of thorough medical research (see Lawrence 1989 also for a historical overview, Blewett et al. 2008 for immunological complexity of human milk).
So why and how could it yet be considered 'rational'? For one thing, the rise of a science of nutrition that emphasized measurement and technology, but stayed silent about the vast range of unknowns, made both mothers and clinicians trust dietary advice starting from measurements and calculations that had the semblance of objectivity, yet were hardly more than a cover-up of our vast lack of knowledge. But then, a 'science' no longer open about its limits was an essential part of the culture we all were breathing, a culture proudly speaking of its 'limitless S & T'. And the steep decrease in breast-feeding was part-and-parcel of post-war deruralisation/urbanization (Bader 1980), itself an expression of the faith in perfect substitutability of natural with technical resources.

The decline of breastfeeding became a hallmark of Modernity – and the consequences were disastrous. Yet, as intimated by the complex relationship with state-promoted urbanization, reversal was anything but easy. For in the meantime High Modernity had been institutionalized and mother and child as well as pediatrician were up against those economic and political powers that had great stakes in this post-war 'development'. A quote from Halfdan Mahler - for 15 years Director General of the World Health Organisation – can help us discern the predicament (Mahler 2002 p.3):

> '*The way an infant is fed can be a matter of life and death. Breastfeeding can save millions of lives. Breastfeeding seems such an innocent, peaceful matter that all of us should support and fight for its protection and promotion. But, believe me, it is not at all simple. There are raw economic and political nerves behind an apparently peaceful picture'.*
> *'You might not believe the kind of pressures we were under at WHO in the 1980s, and it is perhaps worse now*'.

Among the economic agents Nestlé became by far the best known, simply because other transnationals revealed far less about themselves. Still, also this one example is rather depressing (McGinnis 1979a). Because of the 'raw economic and political nerves' national breastfeeding polices and practices remain inadequate, also in Europe where there are considerable weaknesses in the protection, promotion and monitoring of breastfeeding (Nicoll et al. 2002).

Now the post war half century in similar ways and from the same conviction of *constructability* attributed qualities to an array of presumed results of science & technology that were as dubious as the 'superiority' of infant formula. Monod's 1949 introduction of his two 'constants' characterizing a microorganism is one of the better-known examples (Ferenci 1999 and, extensively, 2007). Widely hailed as definitive, it was propagated for decades in textbooks, until it became painfully clear that microorganisms themselves 'rule over' those qualities that Monod supposed to be constant. But until then, a 'discovery' born from the 'dream of the age' had for decades fortified convictions as to our abilities to quantitize nature's qualities and next turn them to our own ends. Consider laboratory microbiology with chemostat cultures – which in time proved not to attain a steady state at all, but e.g. display 'a sweep of spontaneous mutations' (Pullan et al. 2008 p.507).

Monod's 'constants' shifted attention from the microbe-environment interactions to the microbiologist with his presumedly superior knowledge. It also greatly influenced the role accorded to microbiology in agricultural research: confident that the soil microbes' behavior was rigidly fixed, inquisitiveness into their real-life contribution to plant growth was reduced strongly. Yet, researchers could not have been more mistaken than that. As is apparent from e.g. Ferenci's 2001 Minireview '*Hungry bacteria – definition and properties of a hungry state*', they were largely ignorant of microbial behavior, especially in soil. They missed about all of the versatility of (soil) microbes.

First a quote from Maharjan & Ferenci's 2005 'Metabolomic diversity in the species E. coli and its relationship to genetic population structure':
"*In this study, metabolomic profiling permitted a phylogenetic assessment of metabolic diversification amongst environmental, medical and laboratory strains of E. coli. Strikingly, no two E. coli isolates exhibited the same metabolite pool profile. Only 27% of detected metabolite spots in 2-dimensional high-performance thin layer chromatography were found in all strains, indicating the a relatively small core of metabolism is conserved across a species. …. These results suggest that great metabolic diversity, to the point of individuality, is likely to be characteristic of a bacterial species*".
Next a quote from Maharjan et al.'s 2006 'Clonal adaptive radiation in a constant environment':
"*We investigated the capacity for bacterial divergence with a chemostat culture of E.coli. A clonal population radiated into more than five phenotypic clusters within 26 days, with multiple variations in global regulation, metabolic strategies, surface properties, and nutrient permeability pathways. …. The multidirectional exploration of fitness space is an under-estimated ingredient to bacterial success even in unstructured environments*".
And Ferenci summarized in his lengthy 2008 Review:
"*Contrary to common belief, the chemostat environment is never in "steady state" with fixed bacterial characteristics usable for clean comparisons of physiology or regulatory states. Adding to the complexity, chemostat populations do not simply exhibit a succession of mutational sweeps leading to a dominant winner clone. Instead, within 100 generations large populations become heterogeneous and evolving bacteria adopt alternative, parallel fitness strategies. Transport physiology, metabolism and respiration, as well as growth yields, are highly diverse in chemostat-evolved bacteria*".

With such examples easily multiplied there is plenty of reason to be very careful with the widely hailed 'rationality' of our post-war half-century. Note especially that the examples given have important aspects in common: (1) 'nature did strike back' and our presumptions did in fact turn out to be incorrect, yet, (2) up to the present day people have been clinging to the old 'truths' in many textbook and protocols, not just from a flywheel effect, but because people had invested in them, emotionally and otherwise. Still **neither breast-feeding nor microbes are malleable in the way High Modernity envisaged** and so first of all

> '*it is crucial not to confuse social constructs or interpretations with their material products or referents'* (Murphy 2002 p.323).

Yet, that confusion seems central to post-war high-modernist projects. But note that nature is still there even when it is 'manipulated':

> *'When manipulated by applied science, nature is not socially constructed or abolished. Instead its rearranged processes are the very elements of social and technical constructions'.*

And that has far reaching consequences (l.c. p.329):

> *'Nature has its own constructive/destructive processes that retain their potentially autonomous force even when redeployed and included within human constructions. These processes have the capacity to burst asunder human constructions from within*'

Indeed baby's fall ill, and microbes in bioreactors refuse to follow textbook prescriptions.

It will be apparent by now why an informed approach to science and technology as relating to agriculture is part and parcel of the present researches. As indicated it will be a historically informed approach, as I learned it from my teacher Hooykaas, a science historian of the same standing as Butterfield was in the general field. With many a scientific discipline having its roots deep into history and carrying on a tradition in which texts are of paramount importance, a historical approach is both required and possible. Once thus re-historized, the sciences offer us great help in the historical description and evaluation of endeavors in which they got invested with more than a marginal role, agricultural R & D among them.
Used to this approach already, I soon became aware of the following points:

1. for more than half a century it is known already that industrial agriculture is unsustainable due to the erosion that accompanies it
2. the fact that plants are feeding on organic nitrogen compounds (not only on mineral N) was noticed from the 2nd half of the 19th century on
3. in 1927 the warning was sounded at the highest level that mineral N-fertilizer is thwarting biological N-fixation
4. from its inception 'industrial' agriculture (that is, the fertilizer industry) is known to contribute greatly to eutrophication, while
5. the latter's propensity to supply our drinking water with cyanobacterial toxins has been evident for decades already,
6. in its formative decades mainline agricultural research was characterized by a nearly total neglect of plant-microorganism synergisms (with strict symbioses a special case), resulting in dominant research approaches that made it practically unable to undo this neglect,
7. introduction of industrial agriculture as a result of this neglect was accompanied by an unbalanced use of industrial nutrients not only, but was also leading to a far stretching loss of access to/disclosure of natural nutrient resources,
8. with the neglect of mycorrhizae the most baffling example of neglect of symbioses, quite likely invalidating much if not most of half a century of 'advanced' breeding (mycorrhzae are fungal root symbionts, known for some 90% of terrestrial plants, that with their hyphae give an enormous extension of the root system).

Evidently, the peasant/small farmer-centered ruralities had some important resources that industrial agriculture was lacking. Worse still, industrial agriculture made those resources inaccessible, and interfered with the wider environment. No doubt the close consideration of ruralities was indicated, as was the close evaluation of industrial agriculture.

1.7. Agricultural policy & research as rupture

What showed up as the most puzzling characteristic of research pertaining to this industrial agriculture is its complete neglect of the traditional peasant and small farmer and the non-consideration of his practices. As indicated long ago (1926, §42) by scientist-philosopher Kohnstamm, such non-consideration of knowledge-from-experience and of the 'practical expert' means, that a research discipline makes a false start. Yet, industrial agriculture was introduced as a break with traditional farming, its promoters justifying it by suggesting it was science-based (as in the first post-war USDA Yearbook of Agriculture '*Science in Farming*').
Quite commonly this break was presented as the 'scientification' of agriculture (cp. van der Ploeg 1987 and 1999 for the phenomenon). But note this was a presentation of 'science' that was crucial to e.g. government bureaucracy in connection with its High-Modernist projects. In this same historical framework it was also the 'science' presented by mainline's 'science philosophy' – that then because of its alienation from real-life science was rejected by renowned authors (e.g. Kuhn 1977, MacIntyre 1977, Feyerabend 1978; also Putnam 2002).

> '*I believe that a full understanding of anything entails an understanding of its history, of its process of becoming. Historical relativism – the relativism that refers to the historically and personally contingent connectedness of all human actions and beliefs – is about as undeniable a fact of our existence as I can imagine. Contrariwise, the sought-for ahistorical and impersonal objectivity of some philosophers of science – the unconnectedness of knowledge claims to person or circumstance – is about as patently untenable a notion as I can imagine*' (Caneva 1998 p.330/331)

Another important signal that things were amiss is, that official agricultural research & policy in post-war decades displayed a puzzling isolation from e.g. contemporaneous soil science or analytical chemistry. Thus 'twice isolated' – from historical farmer and farming as well as from bordering research disciplines - this government-approved agricultural R & D developed its own idiom. This its own language not only displayed a great lack of concepts and methods in common with traditional agriculture, but also with several established research disciplines (with soil science and analytical chemistry two of those, as indicated).
Situated at the centre of the High-Modernist government project this specific agricultural R & D was well financed, and an impressive institutionalization occurred. Its accelerating post war expansion then shaped a true army of hard working, motivated young researchers and extension workers - who had learned a language that was not exactly helpful in discerning the peasant's/small farmer's expertise, or even in finding the information available from other research disciplines.

Things like this occurred in other fields too, for an accelerating growth of post war R & D occurred only there where it did fit in with government and/or industrial policy and its functional (vs. substantial) rationality (*sensu* Mannheim). Therefore, it is wise to take a close look before one would jump from the observation of impressive institutionalization to accepting the importance (or even reality) of the activities within those institutes (*vide* breastfeeding/food giants).
The complex of R & D activities that came to center on industrial fertilizer use offers us an example. What I learned here from some closer investigations was quite enough to bring the role of industrial fertilizer back to modest proportions:

- first of all nutrients in industrial fertilizer (e.g. ammonium and nitrate) proved to be only a small component out of the wide array of plant nutrient sources (e.g. organic-N compounds)

- then industrial fertilizer's relation to a true concept of soil fertility - with its great variety of chemical & biological & soil (micro)morphological aspects - proved awkward
- indicative of this, its regular use in industrial agriculture proved to thwart the symbioses that are essential for plant health and nutrition
- this together with other troublesome aspects – like its connections with eutrophication and greenhouse gas production - just underlines that this core element of post war agricultural research is in need of a big overhaul.

Significantly, post-war, government-approved, agricultural research & extension in its direct relation to the government bureaucracy hinged on a puzzling concept of the would-be **expert**. He now had become a figure who pretended discovering the essentials of farming from his research center, and who had the government engage him in that role, with the farmer required to follow his protocols (cp. van der Ploeg 1986 and 1999). Given the fact that in the immediate post-war years authors like Timmer had still warned against the unbalanced pretenses of such an 'expert' (Timmer 1947, 1949), the fact that the government not only promoted him to his central position, but also integrated him into its bureaucracy, is remarkable indeed. For surely, what we are confronted with here are not just new convictions, but also new legal and institutional structures. That is, the shift had a profoundly political character.

The new overall 'expert' structure – which related not only to agriculture, but also to infrastructure etc. - was empowering the government bureaucracy. Significantly, the specific symbiosis of bureaucracy and institutionalized expertise had its roots in the war (Greenberg 1967/1999, Lyons 1969). Next, also during post-war reconstruction government regulation was decisive in many areas, so there is likely more of a connection between the war economy and post-war decades than has been assumed up to now. We will take a short look at the UK because it is one of the few countries where some information on the subject proved extant.

When the leading historian Joan Thirsk writes about the UK that '*the regions have been forced by a dictatorial mainstream regime to serve the market through a national framework*' (Thirsk 1997 p.249) we sense that something else than democratic procedure must have shaped agricultural policy. And indeed even though the UK was a non-occupied country during World War II its War Agricultural Executive Committees had the task

> '*to implement policy on behalf of the Government and to link production on every farm with the general plan prepared by the Ministry of Agriculture*'
> (for this quote and the following see Menzies Kitchin 1951 p.241f.).

Preparations centering on machines and fertilizers had been made before the war already, but not with organic/traditional agriculture in mind. And yet of the Committees mentioned the

> '*powers, sanctioned by the Defense Regulations 1939, were, before the end of the war, extremely comprehensive and formidable. They could control in detail the cropping, stocking, cultivation and fertilizer program of any farm. They could dispossess recalcitrant or inefficient farmers. Further, they could exercise detailed administrative control of supplies through the rationing of fertilizers, feeding-stuffs, machinery and other agricultural requirements, and of labour by the supervision of manpower under the Military Service Acts*'.

The dispossession indicated was not a marginal phenomenon: it hit some 11.000 farmers. The minutes of the County War Agricultural Executive Committees were closed to the poublic for fifty years and only now have they been released (cp. yourarchives.nationalarchives.gov.uk on the web, 'Sources for British agriculture in the Second World War', under MAF 80). Yet, for most Counties the '*requisition, compensation and settlement registers relating to property taken over by the state during the Second World War ... do not survive*', that is, the registers that ought to be present under 'Work 50' and that '*give addresses of requisitioned premises; names and ddresses of claimants (usually owners) and agents/sollicitors; the government department which used the premises; dates of requisition, derequisition and claim for compensation; amount of claim and sum agreed*' (quote from www.nationalarchives.gov.uk, 'Land: Requisitioned land', Pt.5 'Second World War: records in the National Archives'). This is hardly conceivable, unless members of the Executive Committees and/or the government officials concerned thought it safer to remove the registers. Presently Brian Short, emeritus professor of historical geography in the university of Sussex, is leading the research into this aspect of history of the UK. For a first research guide see 'National Farm Surveys of England and Wales, 1940-1943'. For first research results see '*The front line of freedom: British farming in World War Two*' (2007).

To provide the information that the Committees needed

'*a farm-to-farm survey was made in 1940 and repeated in greater detail with the assistance of the advisory economists in 1941, when it became known as the National Farm Survey, or more popularly as the 'Second Doomsday*'.

'*it gave, among other things, details of: (a) the cropping and livestock on each farm; (b) information on the condition of buildings, fences, water supply and roads; (c) the quality and condition of the land; (d) whether the use of fertilizers had been adequate in the past; (e) the personal capacity of the farmer*'.

The 'advisory economists' were Oxford-based and looked down on traditional/organic agriculture, so the survey itself was already skewed towards industrial agriculture The advisory economists were quite generally pupils of the Oxford agricultural economist C.S.Orwin, who in his '*The future of farming*' (1930) introduced about all that was to characterize later government-directed development of agriculture. He stated that farmers remained locked 'over head-and-ears' in tradition, instead of capitalizing on the new-found knowledge of the chemist and biologist. Corn crops could be grown continuously, he stated, with mineral fertilizer and the land kept 'in good heart'; organics were not needed. Specialization in one or two products was the way to prosperity, diversified production systems were worse than useless. In short, Orwin c.s. pushed exactly all those changes that soon would ruin the human and ecological resource base of agriculture - so much about the 'standard of farming' that was achieved (Sheail 1995 p.186 f.) Technocratic development of agriculture next linked up up with this war-time change-over to 'industrial' agriculture (Palladino 1996).

'*This basic data enabled Committees to attack the problem of the indiviual farm with greater competence and to concentrate their energies on increasing the output of the less productive farms. To this end the activities of less efficient farmers were supervised in detail. They could be ordered to put into effect specific cropping, cultivation and manuring programmes, and if they refused to do so could be dispossessed and the land re-let to suitable tenants or farmed by the Committees themselves*'.

And Menzies Kitchin concludes without even a shade of doubt:

'*As a result of this supervision the general standard of farming improved, and by the end of the war the lowest pre-war grade of farmer had virtually disappeared*'.

It stands to reason that the farmers concerned interpreted things in quite another way, the more so because '*the rise of new ad hoc bodies … appointed from above and not in any sense responsible to any local community*' quite generally summoned strong resistance during the war (and initiated explorations into fully representative, local participation in and control of planning – cp. Beach 1998, also for the quote).

The Church-related Farmers' Right Association did all it could to fight or remedy '*eviction without the right to independent appeal*' that was such a '*contentious feature of wartime War Agricultural Committees' acivities*' (Short 2008 p.211), yet to no avail. After the war the FRA published its '*Living casualities: the dispossessed farmer*' (1946), a collection of true stories. Yet, in post-war years the prolongation of the rationing system was an important lever for the government to pursue 'dictatorial agricultural policy' in its agricultural planning.

> The Agriculture Act 1947 implemented the continuation of important parts of the wartime policies (files about its further details under MAF 142 in the National Archives). '*After the termination of hostilities, it was decided that some of the land requisitioned to increase food production should be retained for agricultural purposes, and under Part V of the Agriculture Act 1947 the Minister of Agriculture acquired powers to compel purchase of any requisit-ioned land which was not sold voluntarily to him, in order to maintain its full and efficient use for agriculture*' (National Archives, 'Land: requisitioned land', pt.4).

Wartime secrecy covered up decisions, including regulations and evictions, so matters were largely outside the jurisdiction of the courts and there was no revision of the wartime policies. As a result those farmers, who consistently worked with e.g. manure and rotations, were in deep trouble, during and after the war, if they survived at all. The centralized war agricultural economy, as followed by the directive economy of the post-war years, was completely on the Modernist side (e.g. Currie 1942) and of paramount importance in promot-ing the fertilizer industry in agriculture (cp. Conford 2002 §VI). As a result, the rather broad sympathy for traditional, organic modes of farming (Conford 1998, Conford 2002) was of no avail.

If we realize that agricultural policies in the Netherlands in wartime – as in many other occupied countries - were still more restrictive, we sense that the war not only harmed traditional modes of farming, but also empowered the central government bureaucracy in its imposition of 'industrial' modes. It apparently is worthwhile to ask if the 'industrialization' of agriculture, as a core project of the post-war High-Modernist government projects, is not *essentially* rooted in war (instead of resulting from presumed 'progress'). Of course the government's bureaucracy and experts provided a direct link to post-war decades.

1.8. Science and technology out of balance

Before we continue: we must be aware that the **expert** *always* runs the risk of being 'unbalanced'. Just take Feyerabend's (well documented, as usually) definition:

> '*An expert is a man…who has decided to achieve excellence….in a narrow field at the expense of a balanced development*' (Feyerabend 1999; the subject had F.'s attention a long time, e.g. Feyerabend 1986a in Feyerabend & Thomas 1986).

It is enough to remind us of the fact that to become a true expert one needs long years of apprenticeship, in which the things one thinks one knows can be brought into right perspective. For one's 'expert knowledge' derives its true value only from the wider field of experience that encompasses it, and needs an ongoing acquaintance with the practical and tacit knowledge of the people who know that wider field from close experience. In other words: **an expert is (only) he who had the time and opportunity to grow into balance, with a.o. his 'book knowledge' becoming embedded into practical wisdom**. Things can go greatly amiss if somebody thinks that 'new knowledge' can ever make him into an expert.

But then, the war and the post-war years were conducive to exactly such a 'growth into a lack of wisdom' of a new expert system. In a general way such years are always character-ized by their lack of balance - and the 20th century with its brutal wars certainly was more prone to it than former times. Then when an accelerated introduction and extension of new institutional research with a new expert system was organized, only great efforts could have prevented the introduction of a lack of balance and wisdom accelerating with it.
As it was, the new expert system chiefly brought a grotesque enlargement of male dominance, with a bureaucracy now intruding also into parts of society that formerly had been left alone. The development of the new expert system was reminiscent of male dominance in traditional government with its military 'core'. The result was an expert system convinced that it could 'rule from a distance', with an '***institutionalized separation between relationality and rationality***' (Lo 2005; cp. van der Wal 1988).
These were men who '*thought big, on a grand scale, without concern for the day-to-day amenities that make living enjoyable*' (Jezer 1982 p.180 specifically on post-war housing projects). Unable to see woman's position at the heart of farming (e.g. dairying), they were doing their utmost to rationalize agriculture by de-relationalizing it. Cutting the local bonds that had always characterized sustainable forms of farming, they worked hard at creating an agriculture lacking social and ecological roots.

It is likely that the post-war extension of welfare covered up this growing adverse male dominance for a time. At last there was a development that made a real difference with the previous half-century of war and deprivation, and a comprehensive one at that. Then there is no doubt that in the introduction of the welfare state true considerations of care also played a role (the UK had an exemplary role, see on it Harris 2004, and Brown 1995 esp. on Beveridge). And it is even true that the need for participatory democracy at the local level was emphasized at the very start of the planning endeavour - not only that for the welfare state (cp. Beach 1998). Remember that these were the years in which also many an economist was proponent of an active role for economic policy in matters of distribution of welfare (as to theory they did not think much of interpersonal utility comparisons then - Hennipman 1962 p.36f.).

As so much had apparently changed for the better it was difficult to discern that *relational care* was missing at the core of the government's projects, that were rationalizing-by-de-relationalizing. Before long welfare's centralized top-down construction and implementation was itself conducive to the erosion of the social functions of villages and neighborhoods. (As to neighborhoods this was greatly intensified by the technocratic character of e.g. its grand-scale housing projects. Cp. Heijboer 2006 for an example). For some decades, the growth in material prosperity covered up that the new 'security', provided by the government, was non-social (and non-ecological) at heart. In the meantime the message of Modernization made Modern Man (!), who had 'freed' himself from most of his 'traditional' bonds, the role model everywhere. That, of course, was hardly helpful in leaving social and ecological bonds intact, let alone in making them grow.
In retrospect we discern a tragic element in the rise (and fall) of the post-war welfare state - especially because this was a generation far more idealistic than our own. Where our problem is largely a lack of idealism, that generations' problem was its fervent faith in the manageability of nature and society from a distant center (Kwa 1991). A centre where the

government and the (male) expert were devising solutions that (because of their directivity) were de-relationalized at the core.

With regard to agriculture, the expert who claimed to be more than just a specialist assisting the farmer, was extant already before the war. Many a breeder, at international congresses, already then insists on his 'rights', while mostly not even mentioning that he makes a direct use of big collections of farmers' varieties for his pure-line breeding. Evidently, there and then the government is needed to maintain justice for the peasant/farmer. For (s)he did the bigger part of the work not only, but in those very years is still actively maintaining and modifying this fundamental agricultural capital! Indeed, by then, most governments are not yet ready to grant exclusive rights to either public or private breeders.
For although breeders were monopolizing 'scientific breeding' for themselves, they (with their varieties) were a dwindling minority compared with the farmers (and their landraces). Even in a country like the Netherlands, that prided itself with its progressive agricultural research and policy, there was only a small company of public and private breeders, and even of those most focussed at one or a few crop plants only (Broekema 1936).
And so the turning point is the years of war and occupation - years in which balanced policy was easily shattered anyway – in which the farmer saw his breeding rights suddenly abandoned and breeders' varieties prescribed. Then when after the war regulations and laws stemming from these unbalanced years were prolonged, the farmer lost the opportunity to recover his former breeding resources.
The new 'expert' lived from the denial of the farmers' breeding expertise, both institutionally and legally. His concepts and methods were impoverished accordingly: all he had left were those approaches in which he could wish-away both the farmer and the local environment…. The new breeding endeavour became an extremely unbalanced exercise.

Quite generally, post-war years hardly were in balance. Psychological reasons would have sufficed for that lack of balance, but now it became legalized and institutionalized as well. We see it reflected in the big government projects of post war decades aiming at a far stretching transformation of nature and society. Here the 'unbalanced' expert was central to the 'unbalanced' projects: the new expert system was part and parcel of post war *High Modernity* (Scott 1997) with its *brute force projects* (Josephson 2002).
Now note that 'big force' is not the same as 'brute force'. The latter entered the scene when and where a kind of technology got promoted that promised to 'transcend all bounds', that is, a kind of technology no more developing in constant & stepwise interaction with (local) environment and community, but imposing its grand designs from its pretense of superior knowledge. A superior knowledge that would allow fail-safe project design & execution.

Proverbial example is here the 'weather modification programs' of the 1960s that started from the conviction that *reductionist* research approaches in meteorology were at the brink of delivering all that was needed for weather modification (Doel & Harper 2006, Fleming 2006; cp. Badger 2006). It would not take long before more critical approaches to modeling, as combined with the recognition that for most systems chaotic behavior is quite normal and predictable behavior the exception, made us aware that these technocratic projects were mistaken to the core (for meteorology see Tennekes 1991 etc.).

But note that by then technocracy had been institutionalized everywhere and that impressive institutes based their *raison d'être* on their claim of superiority due to their centralized knowledge. For decades at a stretch *functional rationality* had dominated government- and industry-directed research and extension, and its de-contextualized constructs had been imposed on society everywhere. The lack of real-life contents of some of those constructs became widely known (e.g. formula feeding), but note that a similar lack of real-life validity had already popped up in a great many post-war constructs. Worse still, the example of Monod's 'constants' makes us realize that, at present, we don't even know which 'scientific attainments' of post-war research are vacuous.

To be able to evaluate, we first have to distinguish between the thoughtful use of the many *reductive methods* of science, and their usurpation for ideological purposes in *reductionism*.

1.9. Science and reduction/ism, introduction

'Reductionism' in science is not to be confused with 'reductive methods'. A well-trained scientist is always aware of the reductive character of his specialist methods. In e.g. (bio)chemistry it is essential to investigate and discuss the limits of the analytical methods used. In this context we think of e.g. distortions caused by a specific method and its presup-positons. Only then can the scientist proceed to integrate the results obtained with his reductive method(s) in the whole (cell, tissue, organ).
Most scientists are on their guard now, and practice something like 'Pragmatic Holism' (Edmonds 1996), for science is littered with opportunistic 'methods' that did not comply with the requirements: close investigation of limits and the awareness that the object investigated as a whole surpasses the fragmentary information obtainable with the methods chosen.

The enzymology of the 60s and 70s offers many examples, based on its assumption that enzyme 'purification' followed by experiments in dilute solution offers direct access to the function of enzymes in cells and tissues. For in reality enzymes are embedded in

> '*a complex and diverse particulate infrastructure in living cells … [that] encom-passes not only an extensive membranous reticulation but als a "ground substance" which is laced with a dense array of proteinaceous cytoskeletal elements. The protein density in association with these membranes and fibrous structures is akin to that in crystals'. … 'One immediate consequence of this extensive organization of enzymes in the cytoplasmic compartment (amd others) is that the classic, bulk-phase, scalar concept of concentration is no longer very helpful. Instead we may have to start thinking in terms of "local concentrations"'. … 'The key point is that the successively higher levels of the hierarchically organized, complex living cell are dependent … not so much on the **elements** at the lower levels, but on the nature and existence of boundary constraints*' (Mendes, Kell & Welch 1995).

Also in enzymology it is true that '*to understand the whole, you must look at the whole*'.
Specifically focussing on reaction kinetics, Schnell (2004) speaks of the

> '*fundamental difference between cytoplasmatic and test tube biochemical kinetics and thermodynamics*' and stresses that in the cytoplasm '*reactions follow a fractal-like kinetics. Consequently, the conventional equations for biochemical pathways fail to describe the reactions in in vivo conditions*'.

More broadly still, Goldberger (2006) emphasises about physiological systems:

> '*Physiological systems in health and disease display an extraordinary range of temporal behaviors and structural patterns that defy understanding based on linear constructs, reductionist strategies, and classical homeostasis*'.

For a useful tutorial review of (only) the influence of macromolecular ‘crowding’ in the cell (with its strongly enhanced ‘background interactions’) on biochemical reactions, a review limiting itself to ‘freshman’ thermodynamics, see Minton 2006.
As to new concepts, Kopelman 1989 reminds us that “*’Stir well’ is the most universally found instruction in chemical recipes. … In the absence of convective stirring there is still diffusive stirring. We call it ‘self-stirring’. However, under dimensional or topological constraints, self-stirring may be highly inefficient. Fractal spaces are ideal testing grounds for understirred reaction kinetics. The drastic and unexpected consequences of such ‘fractal reaction kinetics’ are demonstrated here*”. Cp. also other contributions to Avnir (ed) 1989. Schaeffer et al 1988 and Elias-Kohav et al. 1991 show that fractal roughness and porosity occur where growth or dissolution takes place far from equilibrium – in e.g. manufacturing methods of catalysts and in formation of many geological and especially soil materials, and sute enough in the cell. Méhauté 1991 shows how with the fractal geometry **history also makes its entry in mathematical analysis** (l.c. Ch.5). In his own words (§ 5.1.6): ‘*The convolution operator appears naturally when we consider approximations to a fractal interface; and the concept of convolution is the key to memory in physics – that is, to the understanding of physical systems whose behavior at a given instance depends on their previous behaviour up to that instant – they have “memory”’. ‘Thus if we wish to know the response of the system at time t we must know its behavior up to this time – its past history, in fact’. ‘One might say that the convol-ution [operator] makes of the mathematician something of an historian: to understand the present he must know of the past – and he cannot reverse the flow of time*’.

If present-day scientists have good reasons to adhere to ‘Pragmatic Holism’, what then is the source of the recurrent debates about ‘reductionism’ in science? Here a historical digression is helpful. ‘Reductionism’ in science is connected with Renée Descartes, who in his *Discourse* states the following four precepts of his method (cp. Sorell 1987):

> ‘*The* <u>*first*</u> *was never to accept anything as true if I did not have evident knowledge of its truth. That is, carefully to avoid precipitate conclusions and preconceptions, and to include nothing more in my judgements than what presented itself to my mind so clearly and distinctly that I had no occasion to doubt it.*
> *The* <u>*second*</u>*, to divide each of the difficulties I examined into as many parts as possible and as may be required in order to resolve them better.*
> *The* <u>*third*</u>*, to direct my thoughts in an orderly manner, by beginning with the simplest and most easily known objects in order to ascend little by little, step by step, to knowledge of the most complex, and by supposing some order even among objects that have no natural order of precedence.*
> *And the* <u>*last*</u>*, throughout to make enumerations so complete, and reviews so comprehensive, that I could be sure of leaving nothing out*’.

Note that the Cartesian program makes no sense **in a hierarchically structured nature, in which each level has its own set of ‘emerging properties’ which cannot as such be derived from the ‘parts’ that feed into it**. We will see (Ch.3, Ch.4) that soil also has such a hierarchical character. Moreover, most real-life systems display ‘deterministic chaos’ (see later) and just allow short-term predictability (at best).

As a matter of fact, Descartes soon met tough opposition, e.g. of Pascal (Pensées, 84):

> ‘*C’est ainsi que nous voyons que toutes les sciences sont infinies et l’étendue de leurs recherches: car qui doute que la géométrie, par exemple, a une infinité d’infinités de propositions à exposer? Elles sont aussi infinies dans la multitude et la délicatesse de leurs principes; car qui ne voit que ceux qu’on propose pour les derniers ne se soutiennent pas d’eux-mêmes, et qu’ils sont appuyés sur d’autres, qui, en ayant d’autres pour appui, ne souffrent jamais de dernier? Mais nous faisons des derniers*

qui paraissent à la raison comme on fait dans les choses matérielles, où nous appelons un point indivisible celui au-delà duquel nos sens n'aperçoivent plus rien, quoique divisible infiniment et par sa nature'.
'L'étendue visible du monde nous surpasse visiblement; mais comme c'est nous qui surpassons les petites choses, nous nous croyons plus capables de les posséder; et cependant il ne faut pas moins de capacité pour aller jusqu'au néant que jusqu'au tout; il la faut infinie pour l'un et l'autre, et il me semble que qui aurait compris les derniers principes des choses pourrait aussi arriver jusqu'à connaître l'infini'.

His final conclusion about Descartes' 'method' is (Pensées, 195):
'***Descartes inutile et incertain***'.

Note that on a practical level, Pascal, Gassendi, a.o. pointed to the clash of Descartes' physics with reality. But Pascal found fault especially with Descartes 'limitless' science, for he decided as part of his program to (Pensées, 193)
'*Écrire contre ceux qui approfondissent trop les sciences. Descartes*'.

Valuable introductions to Pascal's wider approach are Hooykaas 1939 and Hibbs 2005. Jones 2001 deals with the wider unity between Pascal on the Vacuum and his *Pensées.*
For Gassendi on Descartes see LoLordo 2005 & 2007. There is a side to Descartes' approach that is mostly passed over: the connections between his concept of true knowledge and the problem of suffering (van Ruler 2002 is a good introduction).
As to 'science', note that it was not so much physicists, as well as Hobbes who was an ardent supporter of Descartes. That suffices to demonstrate that Descartes' reductionist method was at least politically attractive from the start. Hobbes, then, met his opponent in Boyle, who appealed to Bacon to settle the issue.
Not even the advent of 'Newtonian mechanics' changed the scene, if only because the effort (Laplace's) '*to show the mechanical stability of the universe … [was] an anti-Newtonian effort*' (Ault 1974 p.8). Newton was quite right to deny the effort, and Rouvray (1997) explains why: '*Even without any impacts, the motion of the Earth itself appears unlikely to remain stable in the long-term. This is because the solar system is a dynamic system and one that exhibits clear evidence of chaotic behaviour. The existence of chaos in our solar system was first demonstrated in the last century [that is, end of 19th century] by Poincaré who showed that solutions to the relevant equations of motion generally failed to converge to a specific answer*'.
Note that Newton was no 'physicist' in the way the 19th century pictured him - by leaving his other works unpublished. Since 1998, the Newton Project has put an astonishing number of transcriptions of his theological, prophetic, alchemical, and historical writings on the internet (http://www.newtonproject.ic.ac.uk). They are estimated to run at least several million words (Frankenberry 2008, cp. also Manuel 1974). Note that their interpretation requires some solid historical-theological and similar knowledge, e.g. studying Newton's alleged 'Arianism' requires a thorough knowledge of the Greek Church Fathers that he studied closely (Pfizenmaier 1997).

Even in the deistic 18th century reductionism was still kept within bounds, e.g. by Kant. It is only with the 19th century that we witness a strong surge of reductionism (esp. in naturalism and determinism), which did not include first-rate scientists like Faraday and Maxwell, however.

1.10. Science and reduction/ism since 1800

It is characteristic of the last decades of the 19th century, as well as of our own post-war half-century, that they had such a short memory, and started attributing imaginary doctrines to 'science'. Chargaff in his '*Kant: schlechte Aussichten für einen Newton des Grashalms*' (Chargaff 1982 Kap.2) quotes Kant (from § 75 of the '*Kritik der Urteilskraft*'):

> '*Es ist nämlich ganz gewiss, dass wir die organisierten Wesen und deren innere Möglichkeit nach bloß mechanischen Principien der Natur nicht einmal zureichend kennen lernen, viel weniger uns erklären können; und zwar so gewiss, dass man dreist sagen kann: es ist für Menschen ungereimt, auch nur einen solchen Anschlag zu fassen, oder zu hoffen, daß noch etwa dereinst ein Newton aufstehen könne, der auch nur die Erzeugung eines Grashalms nach Naturgesetzen, die keine Absicht geordnet hat, begreiflich machen werde; sondern man muss diese Einsicht den Menschen schlechterdings absprechen*'.

Then Chargaff gives us two quotes from Schopenhauer (from '*Die Welt als Wille und Vorstellung,* I'):

> '*Mit Recht sagt daher Kant, es sei ungereimt, auf einen Newton des Grashalms zu hoffen, d.h. auf Denjeningen, der den Grashalm zurückführte auf Erscheinungen physischer und chemischer Kräfte, deren zufälliges Konkrement, also ein bloßes Naturspiel, er mithin wäre, in welchem keine eigenthümliche Idee erschiene, d.h. der Wille sich nicht auf einer höhern und besondern Stufe unmittelbar offenbarte; sondern eben nur so, wie in den Erscheinungen der unorganischen Natur, und zufällig in dieser Form'.*
>
> *'Denn in jedem Ding in der Natur ist etwas, davon kein Grund je angegeben werden kann, keine Erklärung möglich, keine Ursache weiter zu suchen ist: es ist die specifische Art seines Wirkens, d.h. eben die Art seines Daseyns, sein Wesen*'.

This should suffice to prove the point (for a broader approach see Ault 1974). When at the end of the 19th century, and in the decades after WW II, reductionism was made 'scientific', the ensuing 'science' became unreliable.

At this point it is important to note there is something inherently attractive in reductionism because of the apparent certainty that it offers. Descartes' 'method' had as a background the Thirty Years War which laid waste large parts of Europe and so was a time of much soul searching. When reductionism is reintroduced in later years, it retains its connection with men groping for certainty in the midst of shattering experiences.

Yet, this existential certainty is not to be found in science. That is because the sciences are historical through and through: the sciences are always **communal efforts** ('traditions') which, at least since Bacon, do not depend on e.g. an 'experimentum crucis' that would allow some a-historical recasting. What is convincing in the sciences is the 'whole story', as it originates in an historical setting and there convinces the majority of those familiar with the research fields covering the subject at hand. Kohnstamm 1926 already gave a lucid exposition of this social character of scientific 'laws' (Kohnstamm 1926, IIIA, Ch.2):

> '*A natural law is a result at which a researcher – after serious and solid consideration of as many as possible of the data at his disposal – arrives, on account of a decision to do, for the time being, as if things stand firm, about which he is sure that he does not know for sure that they stand firm, and in which decision he is followed by a sufficient number of his expert colleagues*'.

This close interaction of social and material aspects makes each specific science greatly dependent on its tradition. For this can easily become too rigid: Simone Weil speaks disparagingly of a 'stagnant village atmosphere'. Or it becomes too 'flexible', with 'interests' within industry, government, and/or the expert community re-defining concepts and methods.

> A re-evaluation of the historical records is incumbent on each member of the scientific community, every time he refers to a method or theory to prove a point, to probe the arguments at its introduction (or reformulation). Of course, a researcher (or his boss) can consider this procedure too cumbersome and try to make a short cut by hiding behind e.g. the institution of peer review to suggest 'all is well'.

'Reductionism' remains a sensitive subject exactly because of its 'existential overtones'. When it re-surfaces, in connection with determinism, in the second half of the 19th century, there are no connections with the scientific practice of e.g. a Faraday or a Maxwell. In this connection Jordi Cat arrives at some important conclusions, after studying Maxwell's use of **scientific metaphor** (Cat 2001 p.437):

> '*Maxwell's pattern of commitment to metaphorical and literal understanding of theoretical terms … is irregular both over time and within each separate discussion. This diversity and the distinction between understanding and truth-based explan-ation are compatible with a pluralism about what counts as understanding… What specifically counts as understanding is something of transient historical nature*'.

Note that explicit criticism of Maxwell's approach did not originate in the physics community, but with e.g. Carnap in the 1930s, that is, with the logical positivists. As to Maxwell's approach, we read (Cat 2001 p.398):

> '*the practice of science engages the scientist's intellect, imagination, body, material environment and culture; and, correspondingly, in the proper understanding of scientific practice, history, logic … philosophy of language and psychology … interact*'.

As to Faraday, the most remarkable experimental scientist of the 19th century, whose work is at the roots of electricity's present role in society, Elspeth Crawford's study is really useful. Faraday grappled, time and again, with the limits of his knowledge, in a state where concepts were still to be born. I quote Crawford (1985 p.222/3):

> '*Before he found the 'fact which introduced order', Faraday had to 'lose himself in the complexity of the world', at least for a time. This kind of thinking begins with 'negative capability', a state of mind which implies **strength**, not frailty, in its capacity to **tolerate complexity, disorder and even confusion**. … 'Negative capability' involves the ability to tolerate the threat to one's existing understanding*'.

Then she quotes Faraday himself (from his '*Lectures on mental education*'):

> '*Among those points of self-education which take up the form of **mental discipline**, there is one of great importance, and, moreover, difficult to deal with, because it involves an internal conflict, and equally touches our vanity and our ease. It consists in the **tendency to deceive ourselves** regarding all we wish for, and the necessity of **resistance to these desires**. … This education has for its first and last step **humility***'.

Then Crawford stresses (l.c. p.225/6):

> '*'Vanity', 'ease', … 'humility': Faraday's words all describe emotions. He knew how to fight prejudice and make good decisions. His problem in communicating his knowledge of 'how' he thought lay in the nature of the process itself. It could not be demonstrated on a laboratory bench or written down to be critically examined because the very nature of the process is that it abandons conscious assessment and*

because the feelings to which one is exposed are private. He stated it as clearly as he could: this self-schooling depends upon ***humility****.*
Because humility is a state of mind characterized by dependence or faith, …. the desire to use experience via reasoning to gain knowledge is replaced by a desire to see, accept and appreciate what is actually there. This open-mindedness can be observed. ….
We do not readily tolerate the disturbance which 'negative capability' brings. However, when we positively accept the feelings associated with dependence, new thoughts are possible. The responsibility for creativity belongs to the individual who accepts his own mental pain. Creativity is not the mechanism of a dummy pushed by society or by mysteriously chosen precepts and principles. … Faraday reached freedom from prejudice through his emotional courage'.

For more on Faraday's ground breaking work read the contributions to Gooding & James (eds) 1985 '*Faraday rediscovered*', as well as Gooding's extensive contribution '*Putting agency back into experiment*' in Pickering (ed) 1992, '*Science as practice and culture*'.

Maxwell and Faraday illustrate to what extent many a heated discussion about 'science' is wide off the mark. For sure, sweeping statements like '*everything can be reduced to physical entities*' are alien to this human endeavor, science. As a chemist I concur with Erwin Chargaff in refusing to '*measure the vapor pressure of the spirit*'. Chemical and physical methods are of a specific, limited character and so have a limited 'range'.
In fact, and that is hardly a secret, our activities as chemists or physicists are greatly dependent on our 'mental discipline'. Not at all unlike our love for our spouse or our children. In short, it is indeed quite likely that good chemistry or physics is dependent on 'humility'.
Historically speaking the sweeping statement '*everything can be reduced to…*' is a left-over from a sorry state of affairs at the end of the 19^{th} century, when for a time absurd opinions about e.g. the origins of language, music and song were vented.

The 'monism' of Haeckel c.s. falls under this same heading. Scientists like Lorenz or Bakhuis Rozeboom (phase theory) were not impressed. Of course a token adherence to 'physical monism' does not mean that the subject matter of a publication really depends on it (Stephan's 2002 valuable discussion is an example).

Who wants to understand this rise of reductionism in the course of the 19^{th} century has to delve into history, not call upon some presumed 'science'. Here I found Kohnstamm's 1926 analysis of the '*19th century dogmatism*' to be enlightening (1926 Book IIIA; on Kohnstamm see Hofstee 1973, Vermeer 1987).

As a freshman Kohnstamm read Büchner's 'Kraft und Stoff', expecting to find some sound arguments, but found none. Before long he found the naturalism and determinism of his age, which was dominating public discussions, completely unconvincing. He understood very well that this dogmatism did not stem from science proper (cp. his short autobiography, Kohnstamm 1934). Then, when he treated the subject in his 1908 inaugural address '*Determinism and science*', leading physicists of those years, i.c. Van der Waals and Lorentz, expressed their agreement (Langeveld et al. 1981 p.2/3; more on the subject in Kohnstamm 1918, 1921). Much later, right after World War II, Kohnstamm perceived that the subject had re-surfaced, and he dedicated his last book to it (Kohnstamm 1948). It did not prevent the likes of Monod (with his 'microbial constants') coming to the fore. 'Humility' was hardly a characteristic of those spokesmen of post-war S & T.

1.11. Post-war science and reduction/ism

In spite of the fact that the likes of Monod dominated the scene for some decades after WW II, the 20th century offers us wide-ranging studies, both inside and outside the academic world. They demonstrate that it is absolutely essential for science to rid itself of the tunnel vision and the inadequate methods of reductionism. Consider e.g. Heisenberg's 1958 account of physics, and to the academic discussion at large philosophers like Buber and Berdjajew, who were widely read. Only within the totalitarian systems of the age, Stalinism and Nazism, such contributions were labelled as 'unscientific'.
Then World War II caused a rupture, also in scientific discourse. Yet, because of the strong quantitative growth of institutionalized research in the decades after the war, it seemed for a time that 'science' had at last come of age. However, the manifest quantitative growth, as 'administered' by government and big industry, had peculiar qualitative characteristics.

> One of these characteristics was quite common: the assumption that it was possible to "fix" one aspect of a system without affecting the whole. Economists assumed that data could be taken as fixed except for the one aspect subjected to modelling. Agronomists assumed that yields were determined by one factor 'below optimum'. Even ecological or toxicological research was, more often than not, limited to one-organism research, which is the reason why most real-life interactions were missed (even those of pesticide, pest and predator). Mainstream agronomy missed out on the array of possibilities emerging from active three-party-interactions, from plant-soil-farmer interactions to tripartite symbioses like those of legumes, Rhizobiae and mycorrhizae (which are exceedingly common in nature and in tradit-ional agriculture – Borisov et al. 2007, Küster et al. 2007, Zhukov et al. 2009). Its exclusive preoccupation with industrial fertilizer arose from practical and theoretical poverty….

The wish to isolate problems from the process as a whole made researchers project phenom-ena into a lower dimension (Flatland) where they allowed a closed representation and so became 'manageable'. This wish was so strong that many followed Monod in *constructing* a world that lent itself to their 'rule' because this 'new world' had hardly any degrees of freedom left. But even if researchers were more modest, it still is a simple mathematical truth that an infinite number of (open) 'phenomena' in the higher dimension allow a specific (closed) projection (Frankl 1972). There is something bizarre in reductionism.

> An example was the 'molecular biology' of the Watson & Crick/Monod type ('despot DNA'). Its unsatisfactory character was exposed from the start, e.g. by Erwin Chargaff, the discoverer of base pairing in DNA. As always, also in regard to 'molecular biology' reality proves far richer than theory, and its dimensionality proves far higher than the Watson & Crick/Monod reductions allow. Cp. Burian 2007 p.304: '*theories built on structural molecular formulae (including nucleotide sequence) and structural features of molecules do not contain sufficient information to diagnose the functions of microRNAs. The dimensionality of the problem is greater than that of the body of knowledge going into nucleotide sequence and (secondary) structure of the relevant molecules*'.
> The 'enthronement' of DNA was largely an institutional endeavour (see e.g. Stokes 1982), and was completely dependent upon its presentation as a unchangeable ruler. It had to abdicate when Wang c.s., based on crystallographic analyses of oligonucleotid crystals, proved the reality of different DNA conformations. Since then we have 'dynamic DNA', with the dynamics evidently dependent on many interacting factors and not emanating from the DNA as an unflexible ruler (cp. e.g. Belmont, Constant & Demeunynck 2001 on '*Nucleic acid conformation diversity*').

But note that the molecular biological dogmatism of Watson, Crick and Monod was disputed from the start – cp. Polanyi's '*Life's irreducible structure*' (1968), and many contributions by Erwin Chargaff. Their contributions resonate in e.g. Matile 1975 and Feyerabend 1986. Feyerabend is incisive, as always (l.c. S.132):
'*[der Molekularbiologe] kriecht nicht im Gras herum und befasst sich mit grossen Dingen, wie Gänsen, er sitzt im Labor und sieht sich Photographien des Elektronenmikroskops an. Er entdeckt so Mikrostrukturen und er kann vielleicht auch angeben, wie diese Mikrostrukturen einen vorgegebenen Rahmen erfüllen – den Rahmen selber erfinden kann er nicht. Denn das hiesse doch, dass [er] einen wesentlichen Beitrag liefert zur Beantwortung von Fragen wie die folgenden: welche Arten von Vögeln gibt es?, wie verhalten sie sich?, wie singen sie?, wie antworten sie auf einander? – und das, ohne auch nur einen einzigen lebendigen Vogel gesehen zu haben; ein geradezu mirakulöser Fall präetablierter Harmonie. Wenn [er] aber die zu reduzierenden Verhältnisse nicht selber produzieren kann, dann braucht er die von ihm gelegentlich verachteten älteren biologischen Wissenschaften einfach, um weiterleben zu können, denn ohne sie hat er nichts zu reduzieren*'.

To be sure, the problem is not so much in the projection, as well as in its restoration to higher dimension. Chemical and physical methods are generally reductive, and known as such by the people handling them knowledgeably. The projection that is implied by their use is meaning-less, unless the researcher is 'at home' in the higher-dimensional space where the real phenomenon is situated, and knows how to interpret the impoverished knowledge that is gained with the reductive methods (cp. Feyerabend 1986).

Note that e.g. Kohnstamm 1926, M.Polanyi 1957, Heitler 1971 and Primas 1981 are perfectly clear that physical and biotic reality exceed our reductive methods. So it is puzzling that, quite generally, it is forgotten that after the war reductionistic approaches were indeed **rejected** at the highest intellectual level, and that some notable physicists pointed to the limited value of reductionist approaches. Indeed, there is no exaggeration in the statement that reductionism in science was **never** a viable option in post-war years. De Broglie 1946 and Heisenberg 1958 leave no doubt about it (Heisenberg even gives a systematic-historical treatment of the subject).

Heisenberg and Weiss are among those notable physicists, as are the famous quantum physicist Heitler and many of his colleagues at the University/ETH-Zürich. Feyerabend belonged to this group, as did his friend Hans Primas, professor of physical and theoretical chemistry in Zürich who a.o. is known for his theoretical contributions to NMR (Amann & Müller-Herold 2010 edit some famous lectures). The subject is far wider than we can deal with here. Refer to Heitler 1961/1966; Heitler 1971 (read his Vorwort); Fornallaz (ed) 1975; Feyerabend & Thomas (ed) 1986; Primas 1981, 1990a & b, 1994, 2002; and the celebratory volume Amann, Altspracher & Müller-Herold (eds) 1999.

Trewavas (1999 p.30) summarizes:

> '*Physics might be considered as dealing with the simplest of systems, and among all the sciences, should surely represent the success story of reductionism. This seems not to be true. Physics is moving into holistic territory. Indeed strong surprise is registered by these physicists [like the ones mentioned] that biology, which deals with the most complex systems known, has so strongly embraced reductionist attitudes where their application may be perhaps least useful; just as physicists are deciding the approach is of limited value*'.

Nowadays physicists wonder why it took so long for *chaos* and *complexity* to become attractive research subjects. After all, research in those subjects could have started in post-war decades already. Trewavas gives some indications why it did not, especially not in mainstream crop sciences (l.c. p.32):

'*It is the simplicity that appeals. Any real complexities can be simply ignored or set aside fo future investigation without any thought as to how they can be achieved. Concentration on one small area at a time can give the impression that, in some way, the whole will at last be understood, despite that there are an almost infinite number of small areas to be studies. By protecting the experimental system from any environmental perturbation and with control of all the parameters, the behavior can be investigated one step at a time, even if this bears little or no relation to the real circumstances under which plants grow*'.

And he continues:

'*A simple calculation indicates the likely environmental complexity involved. The plant environment can be separated in at least the different components. If ten different settings of each environmental parameter can be distinguished, then the possible number of real environments a plant may live and survive in is* 10^{10}. *Methodical means are, therefore, unlikely to investigate this situation adequately, but the attempted investigations to understand this situation do provide for career structures, publications, and all the other trappings of present-day biology*'.

Michael Polanyi's 1958 '*Personal knowledge*' could have given a re-start to research by offering an account that was clearly superior to the reductionist ones. The book had a very long period of gestation, and is unique in its in-depth overview of science and technology 'from the inside' (Scott & Moleski 2005 Ch.8). But as it was, the long 1960s were without parallel in their faith in 'limitless S & T', in e.g. enzymology as in agronomy. The careful approach of M.Polanyi c.s. did not appeal to the spokesmen of institutional science of the age.

After some decades we can look back and see what was of lasting value among the products of the 'long sixties'. Research that is of lasting importance has an eye for the broader connections. Enzymatic studies, for example, are not carried out in isolation but are part of wider investigations (cp. Chadwick & Ackrill 1994, Mendes et al. 1995). And in kinetic and mechanistic studies 'curve fitting' of data to some convenient mathematical function is of limited significance only (the number of measurements is always finite and so will always allow an infinite number of 'fitting functions'). Instead, these studies depend strongly on wider investigations which for example give positive clues as to chemical intermediates etc.
Summarizing: 'lasting' research always strives for integration. For anybody well-versed in chemical or physical analysis, his methods are clearly very specific (and therefore limited), and do not warrant any 'sweeping statements'. It is from the wider context that his results derive their real meaning.

As to the present, Edmonds (1996) is quite right when he characterizes the working philosophy of most scientists as '*Pragmatic Holism*'. Still, an outsider could get the wrong impression, because there are some heated controversies in which parties use partisan statements about 'reductionism' and/or 'complexity'. But after the dust has settled, it is still clear that non-reductionism and complexity are here to stay (cp. Strumia 2007).

A valuable publication by a leading scientist about reductionism is in Gould 2003; for a thorough philosophical discussion refer to Smith 1984. Connected with 'pragmatic holism' is the concept of 'emergence/emergent properties', yet, because of its many aspects I prefer to pass it over for the moment. Cp. '*Emergent properties*' in the Stanford Encyclopedia of Philosophy for a useful overview.

1.12. High Modernity and reductionist S & T

High Modernity's claim to superiority of the knowledge gathered in technocracy's institutions is of a piece with its 'principled reductionism' that entails the denial of the essential character of local knowledge and experience. Indeed, the post-war peasant/small farmer was disowned of his expertise, due to the crass assumption of the government's technocrats that the peasant's knowledge was of an inferior kind. The enlightened laws and rules issued by the government next took away his local resources and with that his subsistence base.

Consider one such a technocratic account, this one from post-war Yugoslavia (Klemencic 1964 S.45):

> '*..die Rationalisierung der Wirtschaftsführung im Walde hat aber zum Verbot des Weidens im Walde geführt. Allen dießen Maßnahmen und Problemen, die durch die rationelleren Standpunkte in der Verwertung der Gebirgsweiden bedingt sind, waren besonders die kleineren, durch den Krieg geschädigten landwirtschaftlichen Betriebe nicht gewachsen. …deshalb ist es verständlich, daß die Basis ihres Lebensunterhaltes verlorenging, sobald sie Ziegen nicht mehr halten könnten und das Weiden im Wald verboten wurde*'.

As governments everywhere the post-war Yugoslav government was aiming at accelerated industrialization. Moreover, from the conviction of its superior rationality, it had no respect for the ecologically sound character of most of the farming systems of its peasants. Note that it simply ousted the farmers from the woods, even though these had been common resources for ages and had been turned into viable 'agro-forests'. Note also that no distinction was made as to the **systems** the peasants used for their small ruminant grazing (for which see Papanastasis et al. 2009). Many of the peasants, in e.g. the Julian Alps, in Velebit and in Zumberak, who had first been victims of the Nazis, were subsequently robbed of their last resources by their own post-war government.

The government did this based on a technocratic 'wisdom' that prolonged and intensified the misconceptions of Prussian forestry, while it did not even take the effort to consider the local agro-ecologies, in spite of the fact that these had been viable for centuries (cp. Rajan 2006 for Prussian forestry and Vera 2000 for expositions of the many misconceptions). Convinced of its superior expert knowledge, the Yugoslav government imposed an *ecological regime* (Zierhofer et al. 2008) that had no place for the peasant and his ruralities.

What we meet here is **the greatly simplified ecological regime befitting technocracy**, where scientism & technicism have taken the place of science & technology. For if they are true to the standards of their own traditions, S & T are carefully exploring limits, not transgressing them (Kohnstamm 1926, Froehlich 1978). 'Limitless' S & T is a square circle: there is quite a jump from the use of concepts and methods in S & T in ways that are true to their always limited character, to a use that neglects that character and that allows (or even requires) the expert to suggest that his concepts and methods make him identify all that is 'really there' (Funtowicz & Ravetz 1990). In that case we are faced with bad S & T:

denying the limits of concepts and methods simply means I'm not doing what I ought to, if adhering to the standards of my scientific/technical tradition that prescribe active exploration of the limits. Instead I invest these human concepts and methods with super-human power, as of themselves able to delineate reality to the government's experts (cp. Winner 1980). Disinvesting them from their human origins and contents (criticism already in

Kohnstamm 1908, 1921, 1926), I am ideologizing them, making them fit at the same time to justify the government's brute force projects.

Indeed we see that High Modernity was in need of reductionist S & T.
From now on the expert did not probe some soil, environment, or community merely to help him decide what could be the next step in his care-full interaction with local phenomena and local actors (with their knowledge-from-experience). That 'old-fashioned' approach linked up with that of the peasant/farmer, in which 'to fail in a safe way' was a core element in his experimental designs (e.g. try-out of new crop varieties). Designs that were made, so much will be clear, not from fear but from the conviction that local agro-ecologies still had positive surprises in store ('emergent properties' *sensu* M.Polanyi).
But under High Modernity this local, practice-based experimentation was considered passée and the new expert assumed he could make great strides at once: he could 'take his probes' without any further interactions, then move the probes to his 'laboratory', and there use them to design the project. Thanks to his superior knowledge he considered this to be a fail-safe design, and its 'neutrality' the supreme justification for the government to exercise its power-from-the-center, allowing it also to forcefully impose the design.

Its imposition then framed the *actions setting* (Weichhart 2003) for future actors – as the Bureau of Reclamation's (BR) projects in the USA did increasingly for agriculture with its hydrological projects (Brun et al. 2006). In the US, this BR actions setting was the one in which Congress in 1982 could 'legally' hand American farming over to very big enterprise (l.c.). But with their *ecological regime* (Zierhofer 2008) in discord with real-life ecologies, such hydrological projects inevitably have peasants/small farmers among their victims in the present (e.g. Madaleno 2007), while the future prospects of their technological 'solutions' are dim for everybody (e.g. Aubriot 2006).

As noted it is quite decisive that the technocratic power-approach

(a) became embodied in politically strong institutions (for the centralized 'hydrological regime' in Mexico see Kauffer 2006, in the US Brun et al. 2006), yet,

(b) does not derive from *S & T as viable traditions.*

Quite decisively, it is only by care-full trial-and-error under specified local circumstances that progress is attained, be it by the peasant/small farmer or by the scientist/technologist. As civil engineering professor and historian Petroski (2006 p. 167, 185) reminds us:

> '*Basing any design….on successful models would seem logically to give designers an advantage: they can pick and choose the best features of effective existing designs. Unfortunately, what makes things work is often hard to articulate and harder to extract from the design as a whole. Things work because they work in a particular configuration, at a particular scale, and in a particular context and culture*'.
> '*Constantly pushing the limits of experience of any technology is fraught with danger. It is done responsibly only a step at a time, and with a reality check after each that the wobble in the step is not getting out of control*'.

Reductionism either in science or engineering has nothing to commend – reason for Petroski to reject computer model centered 'testing' (l.c. 108, 111, 188).

Here at the highest level we are reminded of Michael Polanyi's careful discourses about S & T in his 1958 '*Personal knowledge*': it is only by consciously adhering to their traditions that scientists and engineers can do good (professional) work (also Polanyi & Prosch 1975; Ravetz 1981 stresses that Kuhn's 'paradigm' of a science is near to its 'tradition'). The craft-like elements are essential to their disciplines as 'living traditions' and always are in need of hands-on transmission: initiation into the discipline's practices and the introduction to its wider literature require a master-pupil relation (not once but repeatedly). The 'tacit knowledge' that always is at the heart of S & T, as it is at the heart of the crafts of old, is of a relational character and so cannot be 'objectified' e.g. in texts.

> Cp. Delamont & Atkinson 2001 for a recent account of the hands-on transmission in science and Collins 2001 for a most illuminating example. Lenoir 1997 shows the decisive element of artisanal experiential knowledge, as integrated by Bosch, in the development of the Haber-Bosch process for ammonia synthesis.
> As to the non-objectifiable character of tacit knowledge see Tsoukas' 2005 exposition, and dismissal of common misconceptions.
> Note that intellectual enquiry parallels the practice of a craft and similarly always starts from a tradition and from the initiation into it by a master (MacIntyre 1990, 1981/84. For a specific aspect cp. Funtowicz & Ravetz 1990 Ch.4 '*Craft skills with numbers*').
> To depict crafts as 'static' and contrast them with a 'dynamic' S & T is a construct without foundation, seeing that the sciences themselves needed close contacts with the crafts to get 'dynamic' (e.g. Hooykaas 1958, 1961, 1963/1971).

Note in this connection that the recent re-emergence of wind energy has is origins in the crafts: only from this craft base a new breed of engineers could start upscaling the technology. Engineers who had claimed before that they could do without this base just arrived at 'grand designs' lacking the necessary connections to real life (Heymann 1996, 1998). Conversely, in those fields where engineers 'succeeded' in implementing their 'grand designs', e.g. in large-scale re-allotment works, biophysical reality itself was jeopardized, e.g. landscape and ecology deteriorated.

It is care-full, local experimentation that opens up new perspectives on bio-physical reality. Deterministic modeling limits the 'essentials' to those it thinks it can distinguish, and in effect excludes any newly emerging properties. Proclaiming 'power-from-a-distance' we have to suggest that we 'know it all' and there is nothing to explore anymore. Conversely, if the limits of a model are carefully exposed, it can still be a help in exploring the unknown.

We will see repeatedly that there is no substitute for **local** exploration of possibilities. Too many of the products of the post-war expert circuit 'are no longer available' (the sardonical expression is Chargaff's). Its law-like generalities have crumbled (examples see next §) and its precepts have proven less-than-fit for man, nature and society.
If anywhere, it is in agriculture that new possibilities can (only) be explored locally, as 'emergent properties' of the local rurality (we will repeatedly return to this decisive point, e.g. in Ch.3 and Ch.8). To be able to do it, one needs a 'craft-like' introduction to the diverse sides of this rurality. But more emphatically still than the crafts, 'traditional' agriculture always dynamically responded to changing circumstances, in e.g. developing new crops or farming methods, as Joan Thirsk proves with a wealth of examples in her '*Alternative agriculture*'.

One can state without exaggeration that agriculture, the crafts, as well as S & T must be tradition-conscious, or they will not last even if some impressive institutions still will be bearing their name for a while. For it is only from the resource base embodied in their traditions that we can probe the unexplored and shape our specific 'craft' on the way (central argument in MacIntyre's critique of modernity, Pinkard 2003). More specifically:

neither agriculture and the crafts, nor S & T, provide us with law-like precepts that would enable us to jump in our designs from a 'laboratory' to the 'outside reality' (in the process equating the real-life world with a laboratory test tube).

From the awareness of the decisive importance of local factors and interactions leading to 'emergent properties', scientists like Polanyi, Weiss, and Schumacher in post-war decades rejected the reductionist approach in science and policy. Evidently there is a connection between '*Life's irreducible structure*' (the title of Polanyi's 1968 lecture) and the way in which one can learn to explore it respectfully. For an explanation of the hierarchical and non-reducible character of '*The living system*' see Weiss 1969.
Note that not even electronic properties of a polycrystalline surface derive, by e.g. weighted linear addition, from those of the monocrystalline surfaces (cp. Visser 1993 Ch.7).

Where such a 'jump' was projected anyway for a time, e.g. in the construction of large dams or the determination of fishing quota, the results have been sobering, to say the least (for dams see e.g. McCully 2001, for fisheries Hutchings 2000, Jackson etal. 2001, Pilkey & Pilkey-Jarvis 2007 Ch.1). It is not only in traditional agriculture, but also in traditional (!) S & T that only small and gradual steps are acceptable. Think in this respect of the great care needed with upscaling, or even with transfer, of production techniques (Polanyi 1958 gives a wealth of examples).

In the field of agro-ecologies and the interactions of humans with them we find

> '*that the relationships are context dependent, that what works in one situation may not work on another, and we must become more attuned to this reality*' (Keller & Brummer 2002 p.269).

But then, already in far more simple systems in S & T we meet the fact that

> *as a rule properties do not, as such, derive from their 'parts'* (e.g. Gould 2003).

In regard to the analysis of agro-systems we are in need of '*Multi-scale integrated analysis*' (Giampietro 2004). The more so because in our long post-war half-century reductionism crashed completely ('*The crash of reductionism against the complexity of reality*', l.c. Ch.1).

1.13. The crash of reductionism against the soil

The expert's reductionistic approach of agriculture crashes right at the start, when it is applied to the soil. We will look at some soil chemical and mineralogical aspects, among them those that have shown up in the course of fundamental investigations of chemical soil pollution.

Pondering the enormous complexity of solid phases in soil, we understand why the reductive approach in **soil chemistry** was bound to fail. To say it bluntly: there is no in-situ technician in soil safeguarding equilibrium between soil solution and soil solid phases, let alone among the latter. So one cannot substitute the momentary soil solution for the complex system of solid phases (supposedly thanks to 'equilibrium constants' - Visser 1993 Ch.10).

Bohn (1992) is one of those who summarized the inadequacies of efforts to describe the aqueous solubility of soil solids by solubility products of pure substances, by adsorption equations, and by ion exchange equations. As to adorption and ion exchange equations, though they can contribute to the description of specific cases, they as a rule are hardly applicable outside the specific situation where they were introduced. And as to solubility products of pure substances, these are, contrary to what one would expect, hardly relevant for most soil solids. In Bohn's words:

'*Natural minerals with few exceptions are impure. Coprecipitation, isomorphous substitution, and intermediates between endmember minerals are the rule; pure endmembers are rare*'. A close scrutiny of the actual solids is mandatory, and they are only rarely ideal solid solution. Instead specific solid solutions deviate from ideality, each in its specific way (e.g. Fu-Yong et al. 1992). Already Wood (1976 p.25) warned:
'*The silicate solid solutions discussed in this paper all exhibit deviations from ideality. It is apparent, however, that the techniques used to characterise these thermodynamic properties do not allow activity coefficients to be determined much more accurately than +/- 10%*'. This should '*lead the reader to view with caution any attempts to apply complex solution models to poorly constrained experimental results*'.
Where near-surface sensitive techniques have been used, solid-solution by diffusion through the solid has shown up, as in Stipp et al. 1992. Currently e.g. the concepts of solid solution formation and cation exchange are used side by side, as in Shao et al. 2009.

Yet, 'sure' of the reductionist science approach, research in e.g. soil pollution started from the assumption of the existence of an 'equilibrium' between solid phases and solution, and for decades experimental design itself promoted the neglect of kinetic aspects. Pignatello, after many thorough investigations (e.g. Pignatello 1989, 1990), says about the study of (de)sorption of chemicals in soil (1995 p.128, 137):

> '*Of the perhaps tens of thousands of experiments, nearly all have been, or are still, carried out with equilibration times of 72h or less, most under 24h*'. And yet
> '*There is ample evidence that sorption has a fast component, with equilibration times in the order of hours, and a much slower component. … As discussed above, the fast component may be only a small percentage of the total*'.

Recently Pignatello c.s. (Sander et al. 2005) introduced their 'Thermodynamic Index of Irreversibility' TII

> '*for quantifying hysteresis in soils where natural organic matter dominates the sorption process. The TII is based on the difference in free energy between the real desorption state and the hypothetical fully reversible state. … it does not depend on a specific equilibrium model*'.

That is, each specific soil-compound combination needs its own specific investigation of the *distance* from equilibrium.

For nonequilibrium sorption of organic chemicals see Brusseau et al. 1992, for adsorption/desorption hysteresis as affected by organic matter see Kan et al. 1994 and Schrap et al. 1994. Zhuang et al. 2008 in a thorough treatment indicate that as a rule such hysteresis is a characteristic of a healthy soil's level of organic matter. The one-time introduction of the concept of an 'equilibrium soil', quite to the contrary, indicates once more that the experts concerned had no knowledge of a living soil.

Mingelgrin & Gerstl (1995) offer a 'destructive analysis' of the supposed partition constant K_{oc} (for sorption from solution to soil organic carbon). A reason for K_{oc}'s fictive character is that organic carbon in soil is chemically and physically (micro)heterogeneous and embedded in soil (micro)aggregates (Hundal & Thompson 2006, Chefetz & King 2009).

We need to consider the kinetics of (de)sorption: Pavlostathis & Mathavan 1992, Farrell & Reinhardt 1994, Beck et al. 1995. Equilibrium-based approaches in e.g. soil organic pollutant characterization are wide off the mark: cp. Isnard & Lambert 1989, Stephanatos 1991. Note that even in laboratory chemistry the consideration of both equilibrium thermodynamics and kinetics is obligatory.

Note that as a rule not only soil organic matter, but also soil minerals will have a complex consistency: soil feldspars for example show a complex porosity (Dultz et al. 2006). And even where such a complex constituency is not immediately apparent, (de)sorption is still not simple and shows at least a fast and a slow component. German c.s. (2007) who study the (de)sorption of Pb from goethite Garman c.s. (2007) show that the process takes years. Also in this context the ‘equilibrium approach’ failed in its reductionist aims, in spite of it being pursued for decades (Lindsay’s 1979 ‘*Chemical equilibria in soils*’ is a specimen). The chief reason: it was rashly applied outside its valid range (that range is, especially, high-temperature chemistry and metallurgy).

> Truly balanced investigations, e.g. of contaminant sorption and desorption, are bound to acknowledge the true complexity (Totsche et al. 2003 offers a good example; see also Jensen 1993 and Marioloacos 2000). But note that decades of convenient reductions have their institutional fly-wheel effects - in education, in professional practice, and in the ‘standards’ set by bureaucracies. E.g., the OECD guidelines *prescribe* exactly the discredited, short term, equilibrium batch approach (OECD 1997). So it is still common to come across those convenient ‘constants’ in publications (e.g. Farenhorst 2008). Also in publications that as to other aspects attain a high level of quality (e.g. Vinther et al. 2008).

Note that, quite generally, soils resist an ‘equilibrium treatment’ also for other reasons, e.g.
(**1**) preferential flow, with its variability in time and place, and
(**2**) the frequent occurrence of differences in wettability (likewise variable in terms of time and place).

*As to the **First***: Williams et al. (2003; cp. also Holt & Nicholl 2004) are among the many researchers expressing the irreducible complexity of real-life soils:

> ‘*The spatial variability, noted even at this limited block scale [of the experiments], suggests that simplified approaches to understanding water and chemical transport are unable adequately to describe field behavior*’.

*As to the **Second***: quite common are differences in water and chemical transport which develop, in originally homogenous soils, as a result of the occurrence of small-scale differences in wettability. When researchers came up with the first examples of the resultant ‘fingering flow’ in (apparently homogenous) sandy soils, they had their reports returned by the editors of leading journals, due to pure disbelief. We have since learned that it was not the field work, but the expectations of our reductionist theories that were fundamentally flawed (wettability in soil: Hurrass & Scaumann 2007 and esp. Ritsema & Dekker (eds) 2003).

So there are many reasons why soils are always ‘*four dimensional natural bodies … with the key characteristic of varying with place and time*’ (Sommer). Moreover, being hierarchically ordered bodies, they always require investigation at several levels to arrive at something like a valid picture of the whole (e.g. Logsdon, Perfect & Tarquis 2008). Even for e.g. only water flow in a soil there is no site-independent way of modeling, and in regard to upscaling we still face the same stalemate that we faced around 1990 (when Hillel stressed our predicament), because ‘*There are practically no data sets presently available that provide sufficient information to extensively validate existing upscaling approaches*’ (Vereecken 2008).

True soil research, in short, is hard work requiring near-unending patience. So when post-war, mainline agricultural research assumed the existence of a neat soil world, where supposedly equilibrium concentrations as determined in standard laboratory experiments ‘determine’ the nutrient availability to the plant and so lead straight to the advice given on industrial fertilizer supply, it was completely out of step with reality. For even on the a-biotic level a soil like this simply doesn't exist. Given the real-life complexities, someone doing soil research always needs to do some solid (micro)local research (Phillips 2002; see also Ch.8). On the biotic level, the 'synergisms' possible between plants, soil nutrients, and micro-organisms, as well as the creative possibil-ities for the peasant to enhance and modify them, are all-important.

Pondering such sobering facts, we understand that any 'laboratory approach' to agriculture was never commendable – and yet it was central to post-war government Modernization of agriculture. This approach is not based on any sound consideration of peasant practices or of careful soil science, but on the strong faith in direction-from-the-center.

> Many of the 'constants', 'standards' or 'protocols' of post-war applied research – pertaining to e.g. 'equilibrium' approaches – derive from the goals of the government or industry, and have been implemented by their bureaucracies. Human and ecological reality on a local level hardly played a part in this process. For some thorough treatments of the subject, including proposals of *viable* modes of 'science for policy', see Funtowics & Ravetz 1990, and several essays in Ravetz 1990.
> What is generally needed is a complex systems approach, acknowledging non-linearity of interactions (leading to e.g. chaotic behaviors), multiplicity of scales, and emergent (non-reductive) properties. There is a '*multiplicity of legitimate perspectives*' that follows from the different ways in which a subject is context-bound. For a succinct statement see Gallopín (ecologist), Funtowicz (mathematician), O'Connor (economist) and Ravetz (science historian/ philosopher) 2001.

What we are faced with is first of all the ideology of government and the government-related expert. Yet, resistance would have been very strong indeed if this faith had not been shared by major groups of the populace, many big farmers included. Contributing to this common faith was the fervent hope that the solution of the major needs of the country was imminent. Government and expert projected a neat world 100% compatible with their directions, a world meant to replace the historic world of the local and complex ruralities.

> In 1946 we find a powerful bureaucracy connected with the Department of Agriculture in the Netherlands. It originates from the Zuiderzee works, from government involvement with agriculture during the Depression, and from the centralistic policies of the war years. Its 'knowledge centralism' is enforced after the war with its '*Landbouw Cursus*' (*Course in Agriculture* – all of it on paper…) for personnel everywhere in the country.
> Visser 1948/49 is an example of its expert officials extending this centralism, with his lecture about productivity estimates from fertility characteristics. Though talking about 'polyfactor-analysis', he neither discusses its preconditions nor its limits, and his statistics are unclear. Without any discussion he starts from easily measurable characteristics, without mentioning studies in organic matter, soil structure, and soil microbiology. (Note Winogradsky, the soil micro-biologist, and Kubiena, the pedologist, were of international fame; cp. Waksman 1946, Kubiena 1948). When two 'chemical characteristics' prove rather futile (or even lead to contradictions) it does not urge him to carry out an analysis. There is no mention at all of methods developed and applied by farmers. In short, Visser is not '*sachlich*', but simply 'certain' that things can be measured and monitored from the centre.

The post-war years were characterized by fervent hopes to re-make nature and society. When that became the central government project everywhere, nature and society got approached as aspects of 'reality' lending themselves to analyses-from-a-distance first, and to grand designs next. The assumption that the bureaucratic expert could deliver the needed law-like precepts was at the core of this historically unique phenomenon of *the enormous centralization of power in the hands of the executive branch of the state*. Reductionist S & T, aiming at dissection & reconstruction, sounded victory for decades. Until it became apparent that its long-time critics were quite right, after all.

1.14. Not *fatum*, but petrification

The post-war unbalanced expert/bureaucracy was no *fatum* but consequence of our own choices. In post-war decades and up to the present there were the governments (e.g. in the Netherlands) mandating their officials to devise regulations, effectively removing them from the process of political deliberation (cp. Crince le Roy 1971). If we note that this included, in post-war years, that many war-time regulations were converted to laws without any parliamentary involvement (cp. here Ch.9 &10), we sense that the legal and institutional results in the present are in need of a close evaluation.

Now with respect to e.g. the management of environment and natural resources, government administration could certainly have been structured for management that was able and willing to adapt to information from stakeholders and open to social and ecological learning experiences. In the words of Feldman (2007 p.xi) such administration

> '*should be structured to permit their officials to learn from mistakes … and to adopt 'mid-course' policy corrections. This requires … not only institutional flexibility but also some sort of allowance for the interpretation, application, and implementation of laws, rules, regulations, and prior agreements – legal flexibility – in order to amend imperfect decisions and adjust to the peculiar demands of the situations and settings we find ourselves in*'.

Note that this kind of adaptive management fits in very well with the careful, step-by-step, trial-and-error approach of traditional crafts, agriculture and S & T. So if not for the queer choice for 'power', post-war society could and indeed would have taken a course maybe more humble and hopeful than the one we got.

And it was a choice, for Karl Mannheim himself had warned for the managerial elites with their functional rationality (Woldring 1986 p.300), not the least because of the **petrification** that follows from their rise to power (Mannheim 1965 p.167, 168):

> '*But unnecessary is the over-emphasis on the manipulative aspects of knowledge and the zeal with which institutions have come to train graduates for certification in the mastery of prescribed subjects in the prescribed interpretation.*
> *..Knowledge acquired without the searching effort becomes quickly obsolescent, and a civil service or a profession which depends on a personnel whose critical impulse is benumbed becomes rapidly inert and incapable of remaining attuned to changing circumstances*'.
> '*Large and well-entrenched organizations are usually able to assimilate and indoctrinate the newcomer and paralyze his will to dissent and innovate. It is in this sense that the large-scale organization is a factor of intellectual dessication*'.

And indeed, the blessings of our managerial elites' policies, as fed by a reductionist S & T, hardly benefitted our real world.

For sure the efforts to impose these policies on the real life of men & nature met with strong resistance, especially from the peasant/small farmer. But within the neat world projected by the government and its experts such resistance, which was at cross-purposes with the wider government aims of 'development', was attributed to e.g. the peasant's history of oppression. Consider American anthropologist Jack Potter (1971 p.362):

> '…*because of their exploited social position and long historical experience, most peasants tend to be cautious, non-cooperative, and highly suspicious of the motives of persons within their community as well as of people from the outside. Such characteristics make it difficult for them to supply the leadership and develop the kind of community spirit and cooperative efforts that seem to be required in most of the new social and economic organization advocated by rural development planners. It is not impossible to develop leadership and foster cooperative organizations among peasants, but planners must recognize that such programs go against the grain of most peasant societies and great effort is required to overcome this built-in social resistance*'.

Notice that it is exactly because the argument is made up of more-than-partial truths that its conclusion '*great effort is required to overcome…*' sounded so convincing. These were sensible experts who demonstrated that for the good of the cause the government had to take recourse to forceful ways of implementation…

Still, as it was, the failures of High Modernity's projects were already contained in the very application of reductionist S & T by distant experts. Because (also) post war agricultural R & D was dominated by such reductionist S & T, we had better take a close look at the possibility that several of its projects were indeed of a ruthless nature and were maiming agricultural reality (farmer included) instead of serving it.

1.15. Going for legibility, and losing touch on the way

Up till here it became apparent that the marginalization of the peasant/small farmer and the dissolution of ruralities can only be studied in connection with High Modernity's brute force projects. The heroic transformation of agriculture by governments and their expert systems was a core project, wrought from the proud conviction that they could bring about maximum progress of agriculture from their institutional and political centre.

Now in 1950 the global majority is still made up of peasants/small farmers. Soon the shift to 'farming from the government centre' amounts to an ongoing displacement of this great multitude. As it does to the dissolution of their ruralities - either by abandonment and decay, or by forceful transformation of the land into an 'industrial landscape' where farming was subject to outside direction in such a way that it prevents the farmer from using the local resources which is at the heart of ruralities (e.g. disabling of symbioses by high fertilizer gifts).

There is an inherent lack of logics in the usual emphasis on the productivity of the newly introduced industrial agriculture, taking into account that such a mass of people lost their livelihoods, while agriculture and food production lost its base in the rich variety of ruralities (incl. their agro-biodiversity) all over the globe. When the land deemed fit for food production is confined to relatively few areas 'allowing' a transformation to industrial agricultural production, where is the logic of boasting of the 'supreme productivity' of industrial agriculture?

For the enlightened officials and experts impatient to secure agricultural progress these areas thus transformed were much more attractive than the patchwork of ruralities where nature and tradition seemed to conspire in preventing the application of methods of production that had the 'rationality' of industry. Already before the war Stalin and his new experts had chosen a brute-force approach in bringing about the transformation of rurality-based agriculture to an 'industrial' version in line with direction from the centre. In the USA the war was the turning point in a complex way, when the Late New Deal rediscovery of ruralities got shelved and the technocratic approach was given right-of-way.

That the Netherlands followed the US as its big example in agriculture is astounding for a small country with (before WW II) plenty of own ruralities, but less so if we consider that for enlightened officials and experts the new polders, the Zuiderzee works, seemed fully to affirm the 'powers of technology'. Their near-equivalent in the US were the dams and large scale irrigation works of the Bureau of Reclamation (BR). Johnson, from his conceived success in having the BR construct a big dam in the Columbia River, with the accompanying irrigation works, would never more be in doubt about the powers of a 'limitless' S & T. For indeed the ecological devastation of the Columbia River basin took time to develop.
As it was, and in combination with the greatly extended powers of bureaucracy resulting from depression and war, technocracy in different disguises became rampant at both sides of the ocean. With the Dutch bureaucracy choosing for technocracy in agriculture, the Netherlands were soon to become a catalyst in redesigning European agriculture at large (Mansholt c.s.).

With technocracy – centralized design & development – thus victorious everywhere, who would still have the patience to take a close look at all those varieties of mixed farming, or at the dazzling variety of hill & mountain farming systems? Hardly a miracle that governments aiming at progress focused on the well-designable regions, where large tracts of leveled land promised great returns to mechanical agriculture, with the help of irrigation or drainage (e.g. Hardeman 1978). They were convinced that they needed those returns for their growing industrial labor force and/or for export purposes (revenues).
And so before long the same model was pushed everywhere, with e.g. land consolidation schemes specifically designed and executed to create industrial agricultural landscapes. When landscapes got thus visibly impoverished and rural communities started to decay, officials and experts stressed that this was an integral part of enhanced productivity. Yet, more than productivity, the promise that this modernization/industrialization held to the central government of the enhancement of the 'legibility' (Scott 1997) of rural regions, of their inhabitants, and of their economies, seems to have been decisive.

As it was industrial agriculture did prevail - but not due to a proven superiority over traditional agriculture with its ruralities. **A driving power was this deep faith in the applicability of the industrial approach also outside the factory walls**. As to agriculture this faith was negative at heart, because it started from the dissolution and rejection of ruralities. Mixed farming a.o., with its patchwork of fields in rotation, was branded un-economic and unproductive. Its careful practices and its natural resources were discarded and policies implemented to phase them out. In its stead came monoculture specialization with industry-based resources.

> Note in this connection that High Modernity, as it is distinguished by powerful projects emanating from the center, shows a remarkable connection with regimes with strong & unbalanced central powers, military regimes among them. Walt Rostow, author of '*The stages of economic growth*' and advisor to US presidents Kennedy and Johnson, even accorded an express role to the military in modernization in his State Department memor-andum '*The role of the military in the under-developed areas*' (see Park 2006). The US endorsement of military revolutions in post-war decades, some of them very bloody, merely illustrates the doctrine (Schmitz 1999; for the 1964 military coup in Brazil see his p.268f.).

Before long ongoing erosion and eutrophication indicated the serious loss of care: industrial agriculture attained its low-cost production by externalizing its costs. But part of faith in technocracy is the expectation that some extra research will surely lead to solutions. What with the benefit of hindsight was a ‘flight forward’, was considered a laudable effort of officials and experts to perfect industrial agriculture in post-war decades. Firmly entrenched in the government bureaucracy, and implemented with an increasing array of legal and financial measures, conviction was held for decades that this was the one & only modern approach. Traditional agricultures were considered fit for the dustbin of history.

The frame of mind that was dominating agricultural policy soon after WW II becomes clear from the following quotations (Miller 1957 pp.339, 340):

> ‘*Agriculture is destined to undergo vast changes in the years ahead. Achievements in the field of synthetic chemistry promise better utilization of farm products and the creation of artificial foods by purely chemical methods*’.

This one is enough to make a chemist (like I am) gasp for breath.

> ‘*The agricultural complement of the automatic factory may turn out to be either a chemical food factory or automatic, tractor-propelled tillage, cultivating, planting, and harvest equipment*’.

Without the slightest hesitation crop growing is equated with factory production.

> ‘*The “farmer” may become indistinguishable from the urban worker in personal oulook and in his way of life: If the family-size farm should disappear and industrialization of agriculture should extend to its technological conclusion, industry would come to signify a common set of productive operations for all that part of economic life which was once known as agriculture*’.

The government and its expert are ready for a complete make-over of agriculture!

As is the case with other big government projects, the huge costs of this make-over were considered of an infra-structural character opening the way to a progressive future - and so they never got allocated to this industrial agriculture. But then, the total make-over of the economy of the 1950s and 1960s as a whole, to one based on cheap oil and ‘needing’ huge infrastructural works, was not questioned either. This wider make-over then pushed agriculture further on the road to an ongoing scale enlargement and ‘industrialization’.

> **Note it was/is government & experts pushing towards a presumed future. The foundation of ‘industrial agriculture’ in the life of soil, plant and farmer (or in the ongoing supply of oil, as well) was/is about zero.**

When the directive post-war economy increasingly gave way to a market economy, the growing agro-concerns found all the infrastructure and legal & financial system in place that they needed to dominate the field of agriculture and food production cum processing cum distribution. The government, in depression & war & post-war reconstruction, had prepared a system of centralized regulations and central institutes, while disempowering peasant/small farmer and rural community. The multi-national, that qua organization & methods was far nearer to the government bureaucracy than the peasant/small farmer, could & did now step in. And so we ended up with a transnationals-dominated food sector with both huge public costs and huge environmental costs. The price was not paid by the multi-nationals, but was put on the plate of the struggling minority of farmers that was left. Farmers bore the brunt of it all: they got the hopeless task of reconciling a factory approach - that at best is safe within factory walls only - with a nature and environment that is at odds with it.

Only recently, after half a century, did many an expert start admitting that we need mixed farming and a revival of local agro-ecologies to make agriculture sustainable again. That is,

> **we need the re-installment of care practices and local expertise that once were at the core of living ruralities**.

But that local knowledge more often than not expired with the dissolution of the rurality where it was at home. Note that, for half a century, introducing the next generation to most of the traditional care-practices was possible only *outside* of government approved agri- and horticultural education and extension (that exclusively showed the way to bank-approved ways of farming). Government policy in the Netherlands, for example, brought an end to mixed farming (Commissie R.O.L. 1959) and pressed for destruction of fruit trees and traditional orchards (Hoofdcommissie Fruitteelt 1960). Half a century of 'powerful' government High Modernity was quite enough to demolish ruralities, community and ecology and all.

1.16. War & industrial production - and sustainability

But from the start convictions were very strong, especially within the central government but also among many industrialists (or their left wing equivalents) next, and among quite a few big farmers too. These convictions derived from a burning enthusiasm for (the powers of) industrial production. Capitalists and communists both expected that industrial production would open up the realm of plenty for man and society, and the rest of the world followed suit and was eager to catch up.

Long before, in the years around 1900, there had been some probing discussions comparing agricultural with industrial production (e.g. Bulgakow-Lenin). But these had subsided when main-line socialists and communists re-emphasized their faith in industrial production as the entrance to the new world (the whole lot of dissenters, many a religious socialist among them, seems to have been forgotten since). Here at the core of their convictions there were no differences with the capitalists – and all made the curious step to invest a specific human invention with the super-human power to open up the future…The roots of this faith went as deep as the 18th century (e.g. Goudzwaard 1976). Yet its maturation to national doctrine first, and then to global supremacy next, is of a more recent date.

First of all there is a close connection with the extreme war-like character of the first half of the 20th century. As Schieder (1958), Scott (1997) and others describe it, Rathenau's direction of war production in First World War Germany made a deep and lasting impression on many industrialists and politicians (e.g. Lenin). This in spite of Rathenau's own recantation after the war (e.g. in his 1920 '*Was wird werden?*'; but see Struve 1973).
Still the overwhelming presence of peasants/small farmers and of small craftsmen everywhere made governments hesitate to impose technocracy outright. The very substantial agrarian movements, not only in Central and Eastern Europe but also in e.g. Switzerland (where Ernst Laur was its leader, cp. Wikipedia, Laur 1939, and Laur, Howald & Abegg 1971), as well as on other continents, were also an important influence.
But when the US in World War II displayed its superior war production power, as compared to the quite impressive German one, governments everywhere got convinced by the example big industry had set. After the total war had destroyed so much that was valuable, the 'powerful solutions' that seemed to be within reach (to secure a fast reconstruction and further growth) proved irresistible.

And of course, two world wars in a row, interspersed with a depression of tragic depth, had brought great involvement of the government with the economy & public life. Both world wars also had greatly strengthened the position of big industry - from preferential investments by the government handsomely carrying over into peacetime, to the implementation of a spate of legal measures (standardization with all of its institutionalization among them). Then the Depression and especially World War II brought about a great shift of power to central government, as well as great growth of bureaucracies with their back-up of institutional expertise. Next after the war the very tangible needs of the day caused fast & powerful solutions to have great appeal, so it seemed logical to profit from the greatly enlarged powers of central government & big industry in converting the war aims & methods to peacetime ends.

Now the importance of strong, centralized research to the war effort was not lost on governments everywhere. When they noted the (partial) transformation of this research endeavor in the US to peacetime uses, this soon initiated an accelerated growth of institut-ionalized research all over the globe. But note: true to its origins in the war efforts, this was centralized research feeding central planning aiming at powerful designs. Hardly an accident that post war decades are characterized by such an abundance of 'brute force projects', with the all-out introduction of pesticides to e.g. 'wipe out the insect or fungal enemy' as an agricultural example.

Indeed, there is no doubt that e.g. herbicides and (organic chemical) pesticides had their direct roots in war research (Russell 2001). Still they could only come to the fore, after the war, because governments had chosen for the centrally devised, powerful means, and ratified others in using these specific products and approaches. Impatient now with the small & weak - even though these are characteristics of children and of plants – governments a.o. decided for powerful transformations of nature & society, and so were ready to re-define problems and solutions to fit this power-paradigm (Scott 1997 – cp. Edelman 2005 positioning Scott's various studies). They found the peasant/small farmer opposing them, who stressed the need for patience, care and gradual shifts, as essential to the life of soil, plants, and animals.

Government and its many allies who shared its convictions on 'powerful projects' soon got impatient with the resistance against these projects popping up everywhere. After all, it seemed all so irrational. For sure the officials and the experts were sensible people, agreeing with a sensible author like Schultz when he wrote (1968 pp.13, 14):

> '*We live in a period in which there is indeed an agricultural revolution. The scientific and technical knowledge in the West is so far ahead, in terms of what is theoretically possible, that what we see in more than half of the world is obsolete by a very wide margin*'. ... '*Thus, clearly, the new inputs to increase world food supplies must come from outside agriculture*'.

Note that this dominant opinion had no foundation in traditional agriculture – but then, it expressly rejected such a foundation. These were fateful years in which we find not just mainliners echoing Schultz, but even (if only for a few years) Nicholas Georgescu-Roegen, one of the best-informed economists of the age (Georgescu-Roegen 1969a p.525/6):

> '*The tables have turned: it is the turn of the town now to support the economy of the countryside: the 'manure' must now come from the industrial sector in the form of fertilizer*'.

After millennia of ruralities-based agriculture and food production, this all was discarded and governments and their experts emphasized that from now on only an 'ecological regime' with industrial foundation would do. They claimed they could relocate agriculture from the ruralities of old to a uniform factory-like 'environment' where industrial fertilizer dominated and where, ultimately, nearly all labor could be substituted by machines. The concentration of the nation's, and ultimately the earth's, inhabitants in (mega-)cities was part of the deal…

1.17. Forcing agriculture through a bottleneck

Post-war, mainline (agricultural) economics in both its capitalistic and communistic versions was oblivious to ruralities. With regard to capitalist versions of agricultural economy, the USA, which never was a peasant society, gave birth to a school of agricultural economics that knew nothing of the peasant's/small farmer's (rurality-based) resources. It had developed in conjunction with a kind of agricultural research that in the first decades of the 20th century had been especially supported by … the railroads (Amidon 2008).

> Farmers in most of the South denied the relevance of this kind of research, although '*State scientists and the agents of fertilizer companies recommended highly the adoption of mono-culture and fertilizers*' (Earle 1988 p.205). Farmers were well advised to persist in their denial, because planters in Georgia and South Carolina, who had accepted it, discovered after a few years that:
> '*what initially seemed a foolproof, riskless agrarian system devolved swiftly into economic and ecological crisis. The mysteries of soil chemistry and fertilizer-plant exchange had eluded planter and scientist alike*' (l.c. p.206). It is hardly surprising that farmers elsewhere were not in a hurry to quit their cotton-corn-cowpeas crop rotation, the Southern farmers' innovation of the 1830s and 1840s (as long as some Agricultural Experiment Stations at least assisted in its ongoing development).
> Note that the railroad companies were prime actors in starving the small farmer (cp. William Stead's late 19th century indictment of the 'robber barons' in his '*If Christ came to Chicago*').

When during the Late New Deal social scientific research uncovered some viable ruralities (even peasantries, e.g. the Amish) within the US, this research and its results were rather violently suppressed by the mid-war Congress (Ch.9). At the same occasion the plight of the US tenant/small farmer was covered up, due to the dismantling of the Farm Security Administration and the prolongation of semi-slavery in large-scale agriculture.

Note that as to the administration of agriculture there were some close similarities between the US and the UK during the war. In the US it was the Farm Bureau representing especially the big farmers – for years it was led by Alabama cotton planter Ed O'Neal – that received the power from key functionaries in the war administration and the '42 Congress, against the will of a Henry Wallace (Blumm 1973 p.17f.). It suddenly was (see Danbom 1995 p.230)

> '*as powerful in setting national agricultural policy as it was in administering it locally*'.

Before that, During the Late New Deal including the beginning of the war, the Farm Security Administration was doing all it could, with limited means, to protect both American and immigrant farm labor. But then in '43 (l.c.)

> '*the Farm Bureau got control over farm labor when it moved from the War Manpower Commission to the [reorganized] USDA*'.

And it was at the Bureau's insistence that this USDA was banned

> '*from using federal funds to establish housing standards, set wages and hours, or promote collective bargaining among farm workers. It was allowed to hold unwilling*

> *workers on the land by threatening them with the draft, however, a power it used effectively in the cotton South*'.

In plain fact, due to those decisions (1943), the semi-slave labor known from the South (Dorothy Lang's photographs!) got the chance to grow into a core characteristic of all of big American farming. Large-scale industrial agriculture got the low-pay, low-regulation, semi-skilled labor it 'needed' (to be 'economical' and 'competitive').

In the communist world, it was Marx who already opposed the peasant economies with their ruralities and denied them any and all substance. In Georgescu-Roegen's words (1969b p.63):

> '*no Classical economist has had so decisive a role as Marx in spreading among both the orthodox and the non-orthodox chapters of traditional economics the tenet that peasants do not even constitute a social class and, hence, it is senseless to speak of a peasant economy as a distinct analytical category*'.

This remained the dominant opinion among communists and mainline socialists who put their hopes exclusively in all-out industrialization, in spite of some fervent discussions – e.g. Bulgakov-Lenin – at the dawn of the 20th century. The example of Yugoslavia already taught us that post-war communist policies in the agrarian societies of Eastern Europe amounted to a complete rejection of their peasantries/ruralities (Mitrany 1951).

Suddenly a concept of an economy that ignored the existence of ruralities and of the peasant's/small farmer's resources was propelled to a leading role in government policy (especially after World War II). It only showed an interest in the typical industrial inputs and a large-scale agriculture. Due to its policy-related blindness to ruralities/small farmers it developed a disproportionate fascination with industrial fertilizer, and especially with the supposed triumph of industrial agriculture, the maize hybrid responsive to the application of large amounts of fertilizer.

As a matter of fact, this maize hybrid variety was only one option among very many, and a very narrow one at that. In the pre-war USA public breeding had supplied open-pollinated varieties as well as hybrids, with both adapted to rotation- and manure-based small-scale agriculture. Its neighboring countries in Latin America could boast of peasantries that for many centuries had developed open-pollinated corn varieties, used mostly in mixed types of agriculture and often with high yields (if water relations allowed; Truman 1989). Yet, post-war public breeding soon got tuned to the wishes of the large seed companies (Kloppenburg 1986) or of the government and its big central research institutes (as in the communist world). The peasant economies and their ruralities were declared irrelevant, and very soon all that agricultural policy and economics could 'see' was just the high-fertilizer corn hybrid.

As these corn hybrids now 'proved' that from now on agriculture was manageable from an expert & industrial center, the FAO also put great hopes upon it (for aspects of the narrow technoscientistic approach of FAO in those years see Ilcan & Phillips 2006). And so we see that **without any evaluation at all** the FAO starts immediately promoting it in Europe: from '47 on and first entirely as an extension of US results. FAO's initiative then receives great response also in the communist world (Hambidge 1955 p.75, p.222).

Best known in this respect became USSR's president Chrustchev's great expectations of high-fertilizer corn hybrids. Yet, paradoxically, when these hopes were dashed nearly nobody took a second look. Quite to the contrary, the 60s show a singular devotion to fertilizer-centered agriculture. For a time the FAO is the willing instrument of politics and the fertilizer industry. In the words of its representative Parker (1961/63 p.143):

> '*In most of the underdeveloped countries the importance of fertilizers is not recognized by the government, and inadequate preparations are being made for their use and production. To focus attention on this important aspect of agriculture and industrial development, FAO is initiating a fertilizer program under the Freedom from Hunger Campaign. It is financed by contributions from the fertilizer industry in Europe, North America, and Asia*'.
> '*The success of this program … can make an important contribution to the rapid increase in agricultural productivity which is so essential for the 'take-off' into economic growth and modernization*'.

This high-fertilizer corn hybrid/agriculture was evidently the heart of the post-war, High-Modernity government project. It was this **fascination-without-(substantial)-evaluation** with the supposed triumphs of industrial agriculture that for some decades led to the complete neglect of rotation- and manure/compost-based maintenance and upgrading of soil fertility. Agricultural policy and economics only focused on industrial supplies and did not spend a moment's thought on ruralities or the peasant's/small farmer's resources. Rurality-based approaches to soil fertility maintenance were simply discarded, as were other agricultural care-practices.

Policy-makers and economists confidently decided to stake agriculture on the success of industrial fertilizer. For, together with mechanization-based scale enlargement, this promised an agriculture and food production manageable from the expert-assisted policy center. To most governments this promise proved irresistible. When they started to implement it, they declared fertilizer-based agriculture superior from the start because they equated soil fertility with mineral nutrient ion concentration in the soil solution. During those decades we look in vain for any wider considerations allowing a true comparison with traditional care-practices in agriculture. The exclusive focus on fertilizer is a clear specimen of 'functional rationality' instead of 'substantive rationality'. Yet, some substantive evaluations were being made, by authors outside the government-directed research.

An important evaluation was contained in Barry Commoner's 1972 publication. He pointed out that the 1968 annual gift of nitrogen fertilizer in the US was up 648% from '49, yet the increase in yield/ha is not some 600% but a mere 77%. (Most of the fertilizer increase at that point was ending up in the environment, with eutrophication only too manifest). The *per capita* crop production in this same period increased with a mere 6%, and that was all invested in the increased beef production, so that the nutritional situation of the populace was not improved. The less so because consumption of industrially-prepared foods got completely out of balance with fresh food consumption.

Significantly, in this period harvested hectares in the US were down by 16% ánd very many small farmers got 'phased out'. We start realizing that the changes were hardly of a neutral character considering that these were also years of immense social upheavals (cp. Jezer 1982 p.155):

> '*Between World War II and 1970, rural America had an out-migration of 25 million people*'.

Before the war rural social upheaval had been huge, as became widely known from John Steinbeck's '*The Grapes of Wrath*', as well as from Dorothy Lang's photographs. High officials from the New Deal administration admitted that up to the war policies aimed at the protection of the tenant and the small farmer had failed. Indeed during the Late New Deal serious efforts were made (initiated by Henry Wallace as Secretary of Agriculture) to remedy the situation, only to find a premature end when the '42 Congress thwarted them all. As it was,

> '*Between 1933 and 1941, even despite the hard times, farm population dropped from 32 to 30 million. At the end of the war it was down to 24.4 million, or 17.5% of the national population*' (l.c.).

This proves that war with its semi-dictatorial powers to government caused far greater changes in the position of tenant and small farmer than the Depression. Demobilization temporarily brought a slight reversal, yet next (l.c.)

> '*The 1950 farm population was 23 million, or 15.3% of the population; ten years later it was down to some 15.6 million or 8.7%. … But a large proportion of the dispossessed were poor, black Southern sharecroppers*'.

True as that may be, poor white tenants and family farmers in the South had been no less victims of the Depression, the war, and post-war decade policies. As it was (l.c.).

> '*Between 1940 and 1960, 90% of the agricultural population of the Mississippi Delta left the region because mechanization, racism, and lack of job opportunities in other field. By 1960, there were more people crowded into urban slums than there were living on farms*'.

Notice that in those same years mechanization, abandonment of rotations with legumes, and an increase of fertilizer application initiated the Mississippi Delta's ecological destruction, as exemplified by e.g. the suffocation of increasing parts of the Gulf of Mexico (Schmidt 1998; refs. Allred et al. 2007). A growth in agricultural production that was for 'luxury diets' only was achieved at the expense of great environmental deterioration which itself was the result of the dismissal of the expertise and care of the displaced population. Already then there was a loss-loss situation for all to see: crowds of people in slums without prospects, as the corollary of the government denying them the right to take care of the land. And yet, before long the military regime in Brazil after the (US assisted) coup in 1964 would duplicate this sad example of induced upheaval, once more with about 25 million displaced peasants crowding into slums as a result.

If we remember that the EU Common Agricultural Policy beginning 1960s still had to be shaped officially, and that most 'developing countries' had already significant experience with slum formation due to displacement of rural population, we start wondering if not the long 1960s were one of the most opinionated and thoughtless decades in history, with the way it approached 'development' part of its opinionatedness (1st and 2nd 'Development Decade' of the UN). Without any doubt this period's unbridled faith in a S & T 'without limits' prevented any evaluation of its ruthless projects.

What we see in post-war decades is indeed an 'agricultural revolution', but hardly a 'Green' one, because **with its subjugation of the farmer at the heart of his practices to the powers of central actors it is political in essence**.

This subjugation was achieved specifically by means of the forced change-over to a few centrally bred crop varieties. Soil fertility and ecological restraints were grossly neglected and some very specific results of centralized high-fertilizer breeding were now announced to constitute the core of agriculture. But these were certainly no 'miracle varieties':

(1) The total biomass per plant did not increase (photosynthesis was not enhanced), but only the harvest index (reducing the yield and use of other plant parts, as well as organic matter supply to the soil)

(2) Irrigation became the norm, but of course abundant growth due to water sufficiency is hardly ‘modern’
(3) Increasing fertilizer applications and increasing planting/sowing density were closely linked in post-war breeding, allowing ‘disengagament’ from the local soil and its partial replacement by its ‘laboratory equivalent’, the industrial fertilizer solution (but without regard to any physiological or ecological consequences)
(4) Pesticides had to cover up the weaknesses of the system.

The thrust of this centralized breeding was aimed at varieties responsive to large fertilizer gifts (and irrigation), high planting/sowing density, machine cultivation and harvesting. It comes down to copy-catting big-industry: product design and management at the center and semi-skilled labor on the floor. In plain fact there was no agricultural need for the change-over to these few varieties, because nature abounds with low-input ‘ideotypes’ of plant species that can reach high yields with a continuous supply of low-concentration nutrients (Janssens et al 1990).

1.18. High Modernity’s power approach

Nature was expected to subject itself to this centralized power-approach, and on the basis of this ‘certainty’ the government expert prided himself on promoting a quantum leap beyond the skilled labor-intensive and local ecology-dependent agricultural practices of old. But note that this approach, with its prescription of centrally devised varieties, led to a precipitous loss of biodiversity, and subsequently, due to its destruction of landscapes and ecologies, to far wider losses still. Hardly unexpected, if we consider the early indictment (e.g. Whittaker 1946) of the USA’s record of ecological destruction (Greenberg 1998, Tucker 2000, Josephson 2002). Indeed in the past decades we did not see nature submitting itself to the central directives, but indicating serious de-stabilization by all of this ‘power agriculture’.

It is fair to say that peasants and small farmers everywhere were less than enthusiastic about the ‘power play’ of all of those new agricultural policies. Yet, that their resistance was largely based on their knowledge of the local ecology did not dawn on our ‘enlightened’ governments and its experts.

When confronted with the evasion and other forms of denial of their policies by the peasant/ small farmer, government and its experts by being largely oblivious to the ecological foundations of agriculture were unable to understand the resistances that were put up. Soon they started emphasizing – like Stalin before the war – that the powerful transformations envisioned would surely be realized if only man himself was willing to be transformed and to ‘become modern’ (Latham 2000). The **ideology** (Boudon 1989, Goudzwaard et al. 2007) had grown mature and had entered the phase of forced conversions.

Convinced that the future belonged to their High Modernity, the government’s experts agreed with the sensible Schultz that all that did not fit in had to be relegated to the dustbin of history. We were in the high days of this highly peculiar **post-war half-century that knew no history. All of history & society now was compressed onto this one axis ‘traditional-modern’**, with the traditional not considered as a ‘fund’ of information relevant for the present too, but essentially as the dark precursor of radiant Modernity. Those people who, like the rural sociologist Hofstee in the Netherlands, still had some emotional link with agriculture, worked hard to construct a Modern Farmer as the rural equivalent of Modern Man. In their opinion only this figure would make a chance to prevent the complete aborption of agriculture by large-scale, industry-like enterprise (Hofstee 1962).

What was in effect an utter denial of history got imposed everywhere, yet, it was only rarely experienced as an imposition because the perpetrator and the larger public shared this same conviction of progress. Both sides of the Iron Curtain caused (their) historiography to demonstrate the 'progress of science & technology'. They were equals in regard to the technocratic faith that they had in common and the faith in industrial agriculture that was at its core, and so they both back-projected presumed progress in the same way. With such powerful actors agreeing, who would deflect? And so for nearly half a century the world was one in its faith in High Modernity - especially in its application to agriculture.

This unity was bought at a great price: that of depriving justice from all that did not conform to this trust in power, that is, from all that was small, gentle and local. It had no concepts for the shrewd and creative practices of the peasant and the craftsman: its concepts and methods 'knew' the big and centralized approach only. The government and its experts spoke a language that was so much at odds with the small and local life of man and nature that, looking back, one would be inclined to call it comical, were it not for the tragic consequences that ensued. Just some examples:

- they knew next to nothing of natural resources of great value to peasant and agriculture like mycorrhizae
- they were completely care-less, to say the least, with agro-biodiversity, e.g. with farmers' varieties
- mineral-N fertilizer in their opinion could do no wrong, even when in fact it made crops strongly susceptible to diseases (e.g. rusts)
- and at the same time the approach of main-line experts to biological nitrogen fixation for decades chiefly resulted in a deep underestimate of its importance.

Determined that only centralized research and industrial resources could be part of the modern approach to agriculture, all research that did not fit into this paradigm was rigorously excluded from agricultural research for decades. A (comparative) trickle of research in soil microbiology and of research in nutrients in natural ecological systems survived, but even this trickle existed in a government-induced disjunction from agro-technological and from agricultural nutrient research.
As a result, the fiction could hold sway for decades that well-structured soils have sufficient machine bearing capacities – and that plants are limited to the mineral nutrients that industry has in the offering. Until soil structural decay caused by industrial agriculture disproved the fiction - and both wild and domesticated plants proved to be able to utilize the full array of organic nutrients available to them (e.g. organic nitrogen compounds).

But note that the origins of it all are in post-war decades – decades that, on the whole, were quite earnest, and less greedy and corrupt than our own. As always in history, when looking back we see people taking something like the '*dream of their age*' for full reality, though it is hard to understand how they could thus exchange dream and reality (Butterfield 1949). Now the post-war era induced both immense losses of the human and ecological resource base, and displaced ever growing parts of the populace from their home base. Enough to make us wonder how people that serious could arrive at such results. There is more than an element of tragedy in it all…

One element is undeniably the great distortion in thought and practice brought about by half a century of cruel war and deprivation. War approaches are not for long-term sustainability, yet in important ways we acted as if they were. The immense psychological desire to make sense of some aspects, at least, quite likely initiated great distortions in perception. In regard to memories that pertained to wartime attitudes and practices the post-war years showed outright myth formation all over Europe (Judt 2005 Ch.2). Yet, the culmination of this distortion of perception was in the total disregard for the small and local life of people & plants, and the introduction of big-power approaches with great hopes for the future instead. Here we take the US as an example, but note that each country had its own version of the story, and that a 'grand unification' occurred when the US was conceived as the shining example by nations everywhere.

The war saw US president Roosevelt, who had been proud to be 'Dr New Deal' before, change to 'Dr Win the War'. Focusing on big-industrial power to fuel the war effort, he allowed a great many New Deal initiatives to dwindle. While the Late New Deal had made a start in rediscovering the viability of people- and location-centered initiatives, Roosevelt himself now was turning back to 'power approaches'. The common war effort brought a re-unification of the American nation that he could only dream of before the war. Yet, he next allowed attention to shift from people to the industrial apparatus, with its back-up of greatly enhanced industrial research. With the benefit of hindsight this shift was hardly commendable, yet the enormous attraction of it is clear. Just read White's (1996 p.58) account:

> '*When president Roosevelt asked the nation's manufacturers in 1940 to turn out 50.000 aircraft a year, they balked, questioning their ability to procure supplies and machine tools. In the mid-1930s the industry had produced a little more than 1000 planes a year. In 1944 Roosevelt announced the rise in aircraft production: it had reached his expectation, and doubled it…*'

Then he adds Roosevelt's own account to the industrialists who had held central positions in the war administration:

> "*American labor and management have turned out airplanes at the rate of 109.000 a year; tanks – 57.000 a year; combat vessels – 573 a year; landing vessels, to get troops ashore – 31.000 a year; cargo ships – 19 million tons a year … ; and small arms ammunition – oh, I can't understand it, I don't believe you can either – 23 billion rounds a year*".

American productivity had been quite startling – but for a very puzzling kind of 'market', one that only had troublesome connections to the peace-time economies of the USA, Europe and the rest of the world. In a complex way, the war effort granted a re-start to stagnating US industries, but then one that was as indifferent to the 'human scale' (including human health, community viability, ecological sustainability), similar to the whole of the industrial war effort. Note also that earlier Rathenau, the great architect of the World War I German economy, in his 1920 '*Was wird werden?*' had pointed to the great social, economic and ecological destruction caused by this comprehensive war effort. And yet Roosevelt, like other non-dictatorial politicians before him, was also overwhelmed by this 'display of power'.

Of decisive importance for the post-war situation was the appointment of a great many big-industry & finance tycoons to central positions in the war administration. For that, inevitably, caused their view to become dominant.

Even if many of those 'dollar-a-year-men' – who were not on the government's payroll but were assigned for the task by their corporations - were moved by real war-motivated idealism, their perceptions and affiliations were still those of American big-industry and finance men. With the focus on mass production for the war, other plans were soon deemed irrelevant, if not dangerous. The discontinuation of Henry Wallace as vice-president indicates the final stage of a process that not even Eleanor Roosevelt was able to stop. To the contrary, the First

Lady's perception of people and places, though of considerable influence during the New Deal, did no longer play a part in government policy either.

It was of course not only a tunnel vision that emerged from wartime, for the institutional changes were quite immense. Just consider the balance between small and big enterprise. For the US Jezer (1982 p.25/26) indicates:

> '*When the mobilization began in 1940, the hundred largest American corporations were responsible for about 30% of the country's manufactured products. Within three years this share had jumped to 70%. At the other end of the spectrum, half a million small businesses went out of business during the war, and countless others were in such precarious positions after the war that it was easy for the larger corporations, rolling in money [from war production], to buy them out and further consolidate their economic power*'.

Note that this was not just a US development, for war had brought similar forced concentrations everywhere, with especially the Nazi economic concentrations in occupied Europe following closely those in Germany itself (Bettelheim 1946 is one of the few publications on this embarrassingly under-researched subject).

A distinct element of the concentrations indicated, was the sudden growth of centralized research in a select number of disciplines. That selective growth was not just an American phenomenon either: similar policies in World War I Germany and next more specifically in Nazi Germany had predated it. But it was especially the US' example as the unique victor that was to impress governments everywhere. And it was not just the victor's 'research' that made such an impression, but the example of **centralized research feeding technocracy**. For **other kinds of research did not partake in the accelerating post-war growth** - if they were tolerated at all **–** and here again there was at least an analogy with Nazi Germany.

Still it is pertinent to note that in post-war years the economic and research concentrations indicated got endorsed *in spite of* the express elaborations of alternatives (e.g. by renowned authors like Rosentock-Huessy). One should especially not make the mistake to confuse the growth of big industry with the tendency towards industrialization that was quite general in those years. Industrialization policies did not need to focus on cwntralized big-industry approaches. Indeed, for several years alternative policies were still considered, but then they were overtaken by those big-industry policies, and that is a change that is in need of its own historical explanations. Here, as in the case of agricultural policies, there is not some 'historical necessity' (as such rejected by Huizinga and other historians) but there is an array of possibilities from which humans make their often-puzzling choices.

Significantly, in the first post-war years the attention for the subject of industrialization was, at least partially, feeding on pre-war evaluations of Fordism and Taylorism which had been highly critical of those 'industrial management' approaches (as Rosenstock-Huessy's; another example is Burberg 1929). Remember that Rathenau himself after World War I had inspired many people with his revitalization of guild-like concepts as a means to shape industrial production and the labor process, and with the democratization of the government in ways liberalism could and would not (de Koning 1930; Bouman 1936; Mommsen 1989 S.13f. gives a short account; e.g. Joll 1981 is more critical). And there were others: in the Interbellum in England and elsewhere 'guild socialism' inspired a considerable number of people.

Rathenau was opposed by the majority of big employers; Carl von Siemens chose a truly centralistic path instead (cp. Shearer 1997). His *Reichskuratorium für Wirtschaftlichkeit* was founded in 1921 and was financed by the Reich from ’25 on. The subordination of two important ‘war children’, the *Normenauschuss der Deutschen Industrie* NDI and the *Ausschuss für wirtschaftliche Fertigung*, to the RKW then made the elaboration of labor-centered norms rather more difficult. And yet studies in that direction continued, e.g. those of Burberk (‘Leiter der Arbeitsgemeinschaft für Industriereform’, Burberk 1929). Still Siemens took care to exclude labor’s influence on the development of the production process. Rathenau was murdered in ’22, while Siemens was appointed honorary director of the renewed RKW in 1939 under Nazi leadership, which showed the choices made at the political level. But one cannot possibly conclude from this ‘historical fact’ to the viability and sustainability of the modes of production chosen by Siemens and propagated by the Nazis …

With centralized, high-flow production at the core of High Modernist projects after WW II, in due time also the ‘industrialization’ of agriculture seemed perfectly ‘normal’. Yet, that view was far from general before the war. So what, in fact, brought the change?

1.19. Productivism

Immediately after World War II, many a well thought-out book was published probing the desolation that the war years had brought, spiritual/cultural desolation first of all. The authors all were public personalities, e.g. in the Netherlands the renowned historian Huizinga (Huizinga 1945 and 1946a,b as sequels to Huizinga 1935) as well as authors widely known on account of their role in The Resistance (e.g. H.Roland-Holst 1946 and professors Banning 1945, Kraemer 1945, Pompe 1945). They did not only look at the past but at the future as well, e.g. outlining conditions for truly innovating policies. Soon their common fate was consistent neglect.

Europe as a whole in those years sought to escape the probing questions by ‘myth formation’ (Judt 2005 Ch.2) and by a singular focus on ‘production’ and ‘rationalization’: **productivism** as e.g. condition for Marshall Plan Aid. Jacques Ellul, vocal critic of technocracy in the past half-century (e.g. Ellul 1990; see Vanderburg 1987), pointed to the vacuum of positive convict-ions at the heart of European society in 1948, a vacuum that made it unable (Ellul 1948 p.55)

> ‘*to control the technical instruments which it is being induced to employ*’.

Induced, e.g. by Marshall Aid as administered by a select group of people in (connection with) government administration. Note especially that

> ‘*Even if there is no authoritarian doctrine of the state, we are forced to admit that the power of the state is perpetually growing through the development of its administration; that its sphere of action is continually expanding*’ (l.c. p.53)

In both this expansion of administration and action the war played a primary role, so Ellul was probably far more to the point than his readers envisaged when he stated that (l.c. p.55) ‘*European society is entirely built upon a war basis*’. Consider here also the great number of ‘experienced’ officials who by 1948 were in key economic positions again, yet had acquired much of their experience in their collaboration with the Nazis.

The prospect for man, especially European man, is dim, says Ellul, due to (l.c. p.56)

> ‘*the grave fact that in reality there is no further question of measuring and basing the new civilization on the real man, as he actually is. Today man is subordinated to <u>things</u> and the coming society is a society made for these things and not for man, conceived in terms of things and not of man*’.

And he insists, contrary to common assumptions (l.c. p.54):

> '*if we wish to make a serious analysis of our economic difficulties and of the break-down of humanistic civilization, we must bear in mind that one of its essential causes is this assumption that "production must come first". … But if the law of production be allowed to take precedence of all other values – which is characteristic of our own time – it is a catastrophe…*'.

Ellul then obsreves (l.c. p.58):

> '*This kind of society, which tends to be constituted by a mere combination of forces, and thus denudes man of significance, is totalitarian, even if no explicitly totalitarian doctrine is invoked*'.

And he warns (p.56) that everywhere great efforts are made

> '*to create artificially the ideal type of man to fit into this society, i.e., to create the thing with which society can do what it likes*'.

As to Europe he notes (l.c. p.57):

> '*This tendency to create artificially an average type of man for the service of the state is one of the deepest signs of Europe's decadence …*'.

Note that Ellul did not exaggerate. For if we read all the modernization lore, which in deadly earnest, was soon implemented by government-related experts and which played such a decisive role in shaping (and justifying) brute force projects, we realize what massive efforts were indeed being made to produce this Modern Man. It is a sorry fellow: uprooted from the social and the ecology he is without defense towards the great politico-economic powers of the times, a slave parading as lord.
We see him parade first in the USA – where e.g. Arthur Miller portrayed his tragic fate in his 1948 '*Death of a salesman*'. After about 600+ performances of this play on Broadway alone it was impossible not to feel sorry for this tragic figure, yet government and its experts managed to do this. For this was its true ideology – as soon was proven in the all-out effort to spread its gospel all over the globe. And as state ideologies ever, it had strings of brute power attached to it.

World War II had been an extraordinary experience for the Americans. Hailed as liberators as none before – and they certainly liberated Europe from a fathomless evil – they soon linked their role as liberators with their specific powers, just like other 'liberators' had done before. The US have their own portion of problematic history, and is faced with a lot of social problems in the present, but there is no doubt that the US people had been united in the war effort, also at the Home Front. I quote Lingeman (1990 p.132):

> '*War unleashed and mobilized the powerful energy of American production; it gave workingmen jobs and made them part of a knightly quest whose goal was the killing of the hydra-headed Fascist monster. There was a hum and throb of industry everywhere in the land; with the big war production centers in smoke and flame and the clang of machinery around the clock, seven days of the week, munitions were being forged. In overwhelming force, industry bent all its efforts toward the single goal of more production – and still more. … Gone was the stagnation, the business conservatism, the time and motion study men, the cost cutting, the plowing under of crops, the millions of idle workers of Depression times; unleashed was American productive and technological genius*'.

Curiously a very specific technology got associated with all of this (l.c. p.128, 129):

> '*For the first time in American history, American manufacture was operating at full blast. There was no worry about overproduction: Production – and production alone – was all that mattered*'.

'*Mass production had other advantages besides its capability of turning out goods in large quantities. For one thing, assembly-line operations could be broken down into simple machine or hand tasks. This meant not only standardization of parts and efficiency of operation, but also the widespread use of semiskilled labor, so that men could be quickly trained and take their places on the assembly line without needing a long period of apprenticeship or schooling*'.

There was little opportunity for thoughtful reflection in post-war Europe, and the same is true of post-war USA. There the margins were small if one did not want to have one's efforts dubbed 'un-American', with troublesome consequences especially in the McCarthy era (Sexton 1991). Note that also any cooperative or 'guild-like' experiments could get branded 'communist'. True persecution was mostly prevented – by the Methodist Bishops declaring themselves openly against it, by the unions stubbornly taking cases to court, etc. – but

any re-start of the economy, as compared with its wartime narrow focus on mass production ruled by centres-of-power, was prevented.

But that was due to a mix of power play and idealism which, as a phenomenon, was hardly unique. The way in which American politics and government clung to the war-induced unification, which now became the hall-mark of 'this Great Nation', is not very different from the claim of the Yugoslav communists that the birth of the nation was due to their partisan fighting of the Nazis. And just like Tito's choice for the development of heavy industry was a mix of ideology and politics, so the American economic policy choices had such a mixed character too.

One should realize that big-industry productivism was decidedly not considered the only, or even the main, option appearing in thoughtful publications on science and technology in post-war years (like Loen 1948 and Dippel 1952/53). It is Dippel who already then (l.c. p.190 f.) points to the common tendency to love luxury and to evade real needs. He warns that by prolonging such an attitude we lose the ability to discern '*where technology passes into perverted technology*', with as a result a kind of 'automatic' development of technology that yet is not true to its essence. True control derives from the discernment of real needs, so the loss of that discernment '*feeds back auto-catalytically in the tempestuousness of technological development and the arbitrariness of its direction*'. And Dippel indicts (l.c. p.468):

'*If tomorrow our political or commercial or social planners command this technical trade & industry to produce atom bombs, poison gasses, morphine hypodermic syringes for the mass market, or electronic-photoelectric reacting, plastic toy-dogs with inbuilt barking and continuous tail-wagging – then these 'organized people' indeed will do it, and that with enthusiasm. … perfectly, efficient, expert*'.

And he sees the new expert with his 'functional rationality' - as distinct from 'substantial rationality' (Karl Mannheim) - as decisive in the growth of a system which as a whole makes no sense. Add to his analysis e.g. Galbraith's (1958) and we realize that these were not just some 'evolutionary developments': we are now confronted with **choices** made in the past.

Yet, accounts suggesting some 'evolutionary development' (preferably landing us in the best of all possible worlds) were soon popping up everywhere. But such accounts originated from projecting the present onto the past – High Modernity's usual kind of 'historiography'. It is part of the self-suggestion of progress that is the hall-mark of post-war culture, a suggestion not just based on power politics, but to a large extent on common ideology. To illustrate the impoverishment it caused, consider the following example.

Of course pre-war USA knew ‘entertainment’, but even that had its screams from the heart, like the blues’ ‘*Baby please come home*’. And entertainment certainly had not silenced the stern sounds of the Songs of Work and Protest, or the moving sound of ‘*Never was it true this side of Jordan*’ and ‘*Lord I’m bearing heavy burdens*’. Then the war initiated a profound change: after the war and especially in the 50s all of those ‘controversial’ sounds were not longer welcome. Shallow ‘entertainment’ won the day, e.g. radio stations even filtering out the Laments from the repertoire of Spirituals, just focusing on the ‘happy type’. Similarly, popular radio programs that had formerly given a lively picture America’s ethnic population groups – Jews, Hispanics, Blacks – was now replaced by tv-series depicting a ‘typical American’ family without any real social bonds or affiliations but ‘happy’ in their suburban existence (Jezer 1982 p. f.).

Turning itself into a shining beacon to the nations, the USA was forced to re-write its history and recast its culture – something that could only be achieved with the full consent of the majority of its population. As to that consent, ideologies are never just ‘power play’: there is always a convincing aspect at their core (Goudzwaard et al. 2007). It is especially that aspect that is able to motivate people to ‘convert’ to the ideology – only to experience its grizzly aspects later.

> Things would have been different if in the US the ‘big bosses’ had been screened for e.g, their economic Nazi affiliations. For it was not only Henry Ford who had such affiliations. But the US Justice Department’s vigorous investigations of such affiliations under Attorney General Thurman Arnold ground to a halt when Congress gave the big business-controlled War Production Board a say in these cases (Lingeman 1990 p.344). When we note that e.g. John Foster Dulles and Nelson Rockefeller were involved, we sense that history would have taken a different course if justice had had its way (Henry Wallace dubbed Nelson’s affiliations ‘treason!’).
> But then, the same is true for Europe, where in e.g. the Netherlands and in Germany prosecution of e.g. high judges and big bosses – prosecution because of wartime practices - was shelved (e.g.that against the IGF bosses). Politicians just declared cases closed, even when prosecutors had done a solid job already.

As it was, what in regard to its economy was a peculiar historical incident, now was promoted as the road to ‘progress’ and ‘development’ - promises that mass production of commodities will never be able to fulfill. Yet, part of this history is also that, when after the war people saw the cheap-oil based GNP rise sharply, they were quite happy to equate it with ‘growth’ and ‘development’, with technology the motor of this ‘growth’.

Then both the modernization theory and the growth theory got their eloquent experts in university and government bureaucracy. Until time found them out.

> At this point it is well to ponder two provocative theses:
>
> 1. the post-war focus, in Europe and elsewhere, on ‘Productivism’ after the example of American big industry,
> was a historical phenomenon that had no recognizable connection with development and progress in the true sense of the word.
> As its all-out central design and direction are especially a heritage from the war, the value of ‘Productivism’ for a peacetime economy was doubtful at best.
>
> 2. the ‘Modernization’ of man and society which was promoted by governments with all the means at their disposal,and which was aimed at at the ‘Modern Man’ who was ‘flexible’ in his

dealings with people and places, is merely the mirror image of the central design & direction indicated, with its totalitarian slant.
This 'Modern Man' is an uprooted fellow who is defenseless in the face of the big central powers.

1.20. High Modernity's economy

If we return to agriculture, we see that Productivism had definitely been imposed upon it, already during the war. Both in England and on the Continent wartime direction of agriculture had a totalitarian character, with central designs ruthlessly imposed upon it. In the UK the planning of resource allocation was the result of intensive and creative discussion between different parties at planning level, but as to its planning of agricultural production it all was top-down policy which rejected approaches outside its own narrow, productivist paradigm.

It is hardly surpising that as to agriculture centralized, specialized research also had its roots in the war and in the post-war years. Indeed centralized direction was so dominant that soon after the war most farmer- and locality-centered research was increasingly considered obsolete. In the US the Agricultural Experiment Stations, for example, with their close affiliation with the farmer and the region, were increasingly redirected to partake in centralized research benefitting the big breeders. In post-war USA the Department of Agriculture indeed had the power to effect such redirection (as was the case in many other countries, e.g. the Netherlands). Specialized research aiming at the 'industrialization' of agriculture was considered the one and only modern option, and that in itself caused farmer- and locality-centered research to be considered outdated.

As indicated the changing focus in agricultural policy and research was embedded in a wider change-over to technocracy (as broadly conceived). Though the years of war and recon-struction were bound to be directive as to food, energy and materials (Chester (ed) 1951), they were not exclusively 'technocratic'. Note e.g. that the equalizing effects of these directive policies were clearly positive when, in spite of war, it proved possible to provide a big part of the English populace with a better diet than it had known before the war. In a way the war encouraged politicians to extend earlier, partial, efforts to a comprehensive social security scheme. Especially Beveridge, whose 1942 '*Social insurance and allied services*', and particularly his 1944 '*Full employment and a free society*'. Had a far reaching influence (Brown 1995, Harris 2004).
In the same vein at least part of post-war economy had a positive focus. The war taught some positive lessons (Chester (ed) 1951, Winch 1969 Ch.12), often interpreted as the turn to a Keynesian economic policy. That it was initially not just a turn to technocracy is evident, because the majority of European economists became convinced that distributional aspects were a 'legal' part of economic policy (Hennipman 1962 p.32 f.). On the whole there was an idealism in the air, also among economists, that the present age misses badly.

Yet, post-war economic policies had technocratic overtones from the very start. Especially where policies were assimilated by technocratic bureaucracies their evaluation became 'legally problematic', for neither bottom-up input strategies, nor independent evaluations, were laid down in law. When next the cheap-oil economy of the 50s and 60s brought an unprecedented material prosperity, politicians felt no urge to evaluate and economists got over-confident about their models and growth theories (e.g. de Roos & Schouten 1960). Their world shrunk to one in which 'technology' was the motor of the economy (e.g. Heertje 1973;

see ter Borg-Nedervoort for a critical discussion). But note that the connections with real-life technology were tenuous at best (cp. Georgescu-Roegen 1965 – in id. 1976 - and also Bürgenmeier 1992).

Nature & ecology got ‘shrunk out’, or more to the point, economists simply passed it over (in accordance with the previous custom of many an author, e.g. Kleerekoper 1948). Even Hennipman 1962 in his extensive survey ‘*Goals and criteria*’ of post-war economics and economic policy is ‘not of this earth’ at all.
Interestingly Hennipman is eclectic in the authors he quotes, and doesn’t even refer to towering figures in the more recent field of economics. He is completely silent, for example, about Schmoller and the Historical School in Germany, in spite of the fact that its thorough explorations in the ‘embeddedness’ of economics are of primary importance for the goal he states in his publication (cp. Hodgson 2001, Grimmer-Solem 2003). He is equally silent about K.Polanyi c.s. (worthy heirs to the Historical School) who by then had published quite a few studies on the (dis)embeddedness of the economy (esp. Polanyi et al. (eds) 1957; on the concept of the (dis)embedded economy see Stanfield 1990).
Polanyi was definitely a widely known author due to his publication ‘*The Great Transformation*’ (Polanyi 1944/1957; Baum 1995 Ch.1 gives a good discussion). So the most one can say of this curious eclecticism is that those years of ‘evident progress’ did not really lend themselves to intellectual inquiry, or to urging people to be critical of sweeping ideas - and economists were ordinary people then as now.

But as a result of all of this unwarranted self-confidence the ‘engines of production’ got heated up more and more without a thought about e.g. energy, or waste. Before long the growth of environmental problems (plus energy problems) could no longer be denied. Some economists by then started to take a serious look at the situation (also summarizing earlier researches: Goudzwaard 1970 and 1974, Hueting 1974, Georgescu-Roegen 1976). So what made main-line economy turn inwards upon itself (even proposing ‘economic perpetuum mobiles’)? What especially caused its impotence with regard to the analysis of broader ecological questions?
No doubt, specific problems were diligently analysed, e.g. problems involving air and water pollution by Huetink (1974), and their costs pondered. Yet, when scholars like Huetink probed deeper, and explained that the economy is embedded in the ecology, **the ecology dictating the limits and possibilities of the economy,** mainline economists reacted by marginalizing them (e.g. Huetink was fired from the Dutch Central Bureau of Statistics). The mainliners’ paradigm remained technocratic at heart: in the standard economic accounts the economy is *not viewed as embedded in* the ecology. Nature and ecology within that paradigm play an *arbitrary* role in the economy, not an *essential* one.

Within the technocratic paradigm it is ‘unthinkable’ that, given some extra research, the technological substitution of e.g. natural resources would fail. But in fact it is the very methods of accounting that hinder a true perception:

> ‘*only the financial aspect of capital is taken into account, neglecting the fact of physical diversity. Yet the interface between the economy and the environment specifically concerns this physical aspect, which is not recorded anywhere*’
> (Bürgenmeier 1992 p.159)

It is wise, of course, to consider that both mainliners and non-mainliners were equally fallible, human beings. An important part of their problem was that pre-war start of the revision of economic theory often had no follow-up in post-war years (e.g. ‘just price’ discussions). After the war most economists reverted to the old, closed type of theories, conceivably modeled after the natural sciences (e.g. Kleerekoper 1948 Ch.1), but soon extended in a Keynesian way. Then the problems of the time – full employment and welfare - diverted attention from the fundamentals to the assistance of government. Next with Productivism the strategy from ’48 on, and development economics taking shape in its wake, economists soon were completely enthralled by the High-Modernity projects of government and industry.

As a result the hard working experts were quite perplexed when e.g. environmental problems occurred. Just realize that the **experts had to receive the ‘signals’** transmitted by those problems, and were supposed to investigate them, **within the paradigm that ruled their institutions, and that was a paradigm that had no language for it (it was out of contact with the earth and its caretakers**). This lack of an economic language expressing the essential character of nature and the ecology made investigation a formidable task. Huetink, for example, saw that the ’54 decision to take resource depletion in the Standard National Accounts as income was a failure. Yet, in e.g. his handling of water and air pollution he more often than not had recourse only to researches from within the reductionist paradigm, focusing on technological solutions and not on the ecological context. The more remarkable that he eventually arrived at discerning the economy’s all-out embeddedness in the ecology.

With nature and ecology missing from their (economic) picture and ‘technology’ promoted in the leading role it is hardly strange that the paradigm of mainline economists was more of a hindrance than of an asset ‘*to provide political actors with the economic knowledge they need to tackle the problem of sustainability*’ (Deblonde 2001 p.197). For indeed ‘*the content of an ecologically successful economics should regard institutional reasons for the present unmanageability of the ecological performance of industrial economies*’ (id. p.203).
As it is, main-line economy in fact contributed to the causation of the present problems, e.g. due to its unreserved endorsement of the implementation of ‘factory methods’ everywhere. Then as now it apparently inhabits a ‘world’ that allows for such implementation, one that e.g. allows the application of time studies borrowed from industry in the management of nursing homes and schools - in spite of the fact that labor in those institutions is not machine-centered but human-relation-centered. As this example shows, it even confuses machine-throughput with labor-productivity (for the difference see already Dippel 1952/53).

Now note that many present-day economists endorse financial practices that post-war economists would have rejected with indignation (e.g. investments in sex industry or specul-ation with food). Tinbergen deserves full credit for the idealism that made him labor so hard for e.g. the New International Economic Order (from at least Tinbergen 1962 on). Quite probably he also formulated some theoretical notions that our generation neglected to its own peril (e.g. Tinbergen 1967b). But note that with all his idealism he was without defense when the rich countries killed his NIOE-concept in spite of the fact that it had been endorsed by the UN itself (beginning 70s). For it was exactly because the poor were eager for the kind of ‘development’ and ‘growth’ they saw with the rich, that the poor countries were powerless in their dealings with the rich countries – which occupied the directive centers.

But then, Tinbergen himself, just like nearly all of his fellow experts, was a technocrat (e.g. 1967a p.232, 1971 p.414). Still being a ‘friendly technocrat’ he was greatly troubled by the develop-ments indicated. But it was only late in life that he started to intimate that something was wrong with the technocratic system itself, with its excessive concentration of power in the center. Can we find what made him, and other economists of his generation, unable to intimate the incompatibility of this technocratic system with the social and natural ecologies in which our economy and industry are embedded?

1.21. Not of this earth

As a matter of course I will not endeavour to give anything like a comprehensive answer in this paper, but will focus instead expressly on the ***lack of contact with nature and real life*** which is typical of standard economics (for an approach focusing on the arts see Klamer 1993). In fact others have done a thorough job already in the analysis of current economic doctrine, with a thoroughness that is outside my competence (e.g. Lutz 1999, Hodgson 2001, Ulrich 2001). It is quite perplexing for a non-economist to discover that a great many authors had painstakingly dismissed all the doctrines and theories that are central to mainstream economics (Hodgson 2001 places the discussion in a remarkable way in its historical context, while Bürgenmeier 1992 is 'gründlich' indeed). Galbraith merely gave a summary when he recently (2000) spoke of '*the nearly complete collapse of the prevailing economic theory*' and added

> '*It is a collapse so complete, so pervasive, that the profession can only deny it by refusing to discuss theoretical questions in the first place*'.

My own point is especially that economists of quite different persuasion largely followed the neoclassical economists with their

> '*paradigm or theoretical perspective where the idea is to imitate physics and other natural sciences as well as possible – more precisely the state of these disciplines at the time in the 1860s when the neoclassical paradigm was first articulated – with respect to epistemology, equilibrium thinking and an ideal of mathematical modeling*' (Söderbaum 1997 p.117)

Mirowski's '*More heat than light*' (1989) is the best known critical analysis of this paradigm and its background. My treatment of the subject has similarities with his, yet, in spite of its limited size is wider in scope. The neo-classical paradigm, with its equilibrium approach and determinism, was criticized by physicists even at the end of the 19th century, and became wholly unconformable to natural science since then.

It was especially mechanics that became the ideal of scientific modeling and predictability. At this point Tinbergen agrees with the late 19th century economist Jevons, whose 'quantitative approach', which took its inspiration from mechanics, is still highly regarded by mainline economists (e.g. Ekelund & Hébert 1990; cp. Dobb 1973 for a more extensive discussion). A quote from the preface to the second edition of his '*The theory of political economy*' (see Collison Black's introduction to the 1970 Pelican edition):

> '*but as all the physical sciences have their basis more or less obviously in the general principles of mechanics, so all branches and divisions of economic science must be pervaded by certain general principles. It is to the investigation of such general principles – to the tracing out of the mechanics of self-interest and utility, that this essay has been devoted*'.

Of course, it was the popular association of 'mechanics' with 'determinism' that determined Jevons' choice, not a closer consideration of the sciences of his day. As to these sciences, by the mid-19th-century the research into electricity & magnetism and into chemistry of Michael Faraday had become quite famous, and the connection with mechanics was about zero.

In the course of the years several economists expressed their doubts about this fascination with 'equilibrium', of Jevons and his heirs. Still, as a rule they did not find fault with Tinbergen and Jevons taking e.g. the pendulum as a perfect example of scientific determination (for Tinbergen's physics background see also Jolink 1992). Yet, exactly in this respect these two – and all who followed them in their belief in science as a closed system - could not have been more fundamentally wrong. For as described by e.g. Lighthill 1986 the pendulum is easily induced to chaotic behavior - in the physical sense of behavior which has only a strictly limited predictability in time (cp. Jackson 1992). That is, predictable behavior *sensu strictu* is even for the pendulum 'exceptional', whereas chaotic behavior is rather 'normal'.

Note that this example loses nothing of its strength on account of the fact that Jevons and Tinbergen were not aware of this chaotic behavior:
(1)
the example is general indeed and many more 'regular' systems are in fact chaotic, e.g. in physical geography (Dauphiné 1995; it is true even for the tides, Terra 2005). Jevons' and Tinbergen's pendulum as well as their concept of 'mechanics' is an artifact that gives us no information on real life and natural systems. Their use of 'mechanics' does not inform us about physics, but about physicalism (Smith 1984)
(2)
non-determinism in physics had since long been emphasized,
e.g. J.C.Maxwell in the 1870s recognized that much of contemporary physics was focusing at very stable systems only and therefore was not valid outside their boundaries (physicists busy with '*isolated systems where all conditions could be carefully controlled*', Peat 2002 p.138). As Maxwell wrote, **in 1874**, in Nature (cp. Stanley 2008 p.478):

> '***In unstable systems, like antecedents do not produce like consequences, and as our knowledge is never more than an approximation to the truth, the calculation of what will take place in such a system is impossible to us***'

(3)
the famous physicist-philosopher Kohnstamm (on whom e.g. Hofstee 1973 and Vermeer 1987) explained the subject starting in his 1908 inaugural address '*Determinism and science*' (with leading physicists like van der Waals and Lorentz expressing their agreement, Langeveld et al. (eds) 1981 p.8/9). Time and again he treated the subject, up to the publication of his book '*Free will and determinism*' (1947, subtitle '*a mathematical-physical and epistemological exposition for … non-physicists*'), a few years before his death
(4)
after WW II famous physicists like Louis de Broglie (1946) and Werner Heisenberg (1958) offered expositions similar to those of Kohnstamm. Werner Heisenberg in particular treated the subject extensively and used original sources. Wiin-Nielsen (1999 p.38) summarized:

> '*During the last century we have gradually learned and tested that almost all non-linear systems show limited predictability. The nonlinear equations that can be solved in a closed mathematical form are very few and very simple*'.

Tinbergen c.s. were miles apart from crass liberals like Jevons. But why then did they link up with Jevons' kind of theoretical economics, and not with the theories of the Historical School which had been truly empirical and far more realistic? Part of the answer is in the great desire to give 'scientific guidance' of a supposedly neutral kind to policy and industry.

When Amartya Sen at the 1986 Royal Society/British Academy symposium '*Predictability in science and society*' was wrestling with Lighthill's presentation of the pendulum's chaos, we sense it was this subject of 'neutral' policy advice that made him stick with equilibrium-based approaches. For Tinbergen, Sen, and others, giving up on such approaches had the flavor of surrendering the economy to anarchy. It was indeed tragic when their own approach contributed to the removal of policy barriers for the greedy economists who next overpowered their more idealistic kind….

Now if we move on from physics to the living earth the distance between the economic concept and reality becomes truly unbridgeable. In Ayres' words (1993 p.169):

> '*the climate-biosphere system is an extremely complex, nonlinear collection of feedback loops which has kept the earth's climate, oceans, and atmosphere relatively stable for the last few billions of years, in a state that is very far from thermodynamic equilibrium. … Geochemists and planetologists generally agree that the equilibrium state would be one in which the atmosphere would be about 60 times as dense as the present one and would consist mainly of carbon dioxide (and water vapour) with no free oxygen or nitrogen. … Because of the greenhouse effect, surface temperature would be much hotter than the earth is today (probably around 300 ^{0}C) with relatively little liquid water. … An earth in thermodynamic equilibrium within the solar system, absent life processes, would resemble hot Venus more than the cool green planet we now enjoy*'.

And Funtowicz & Ravetz explain (1997 p. 799):

> '*So life turns out to have its own thermodynamic structure, but it is very different from the temporary, anomalous phenomenon imagined by the science that took its inspiration from th steam engine. The popular term 'edge of chaos' well expresses the contradictions involved in sustaining the special conditions enabling its existence. The sun is a necessary condition for life, but not at all sufficient; its rays can destroy as well as nourish, destroying quality more easily than creating it. These insights were used by James Lovelock when he realized, from the presence of special trace gasses in the upper atmosphere, that our planet is an unstable system, far from equilibrium*'.

Again, the earth as a whole, as well as every life phenomenon on the earth, is very far removed from thermodynamic equilibrium. Such open systems have increasingly become the subject of research (e.g. Kleidon & Lorentz (eds) 2005). Mainline economy's fascination with equilibrium is a fascination with death, and Rees (2006 p.154) is right when he stresses that '*economists' policy models are* ***totally abstracted from biophysical reality'*** (emphasis his).

1.22. Valuations

More specifically, complete divorce from biophysical reality appertains also to standard economic policy's (monetary) ways of value assignment, as many authors have demonstrated (e.g. Deblonde 2001). In Sciubba's words (2005 p.29):

> '*There is growing concern that the acknowledged shortcomings in the treatment of environmental problems by the conventional monetary theory of value stem from basic faults in the value-assignment paradigm of the latter*'.

In an age in which the food situation is becoming increasingly precarious the undervaluation of natural resources by conventional economics is baffling. A notorious example is mangrove valuation. Arbuto-Oropeza et al. (2008) note

> '*The extreme undervaluation of the benefits generated by mangroves for fisheries versus the projected benefits of coastal development and aquaculture*'

and then summarize their own research - as based on material data such as fish landings – with the words

> '*The ten-year discounted value of one hectare of [mangrove] fringe is >300 times the official cost set by the Mexican government*'.

Evidently the conventional paradigm is blindfolding its adherents – as to natural resources in agriculture as well as to those in fisheries.

That is a prime reason why Sciubba and others work with methods of valuation that have immediate links with life. They for example use 'exergy content' for valuation, exergy being a measure of **both energy quantity and quality** (Dewulf & van Langenhove 2006). As Sciubba explains (l.c. p.27f.):

> '*economic systems are ecosystems that function only because of the energy and material fluxes that sustain human activities. All agricultural, industrial, and economic activities can only exist as long as they exploit (use) biophysical resources taken from a reservoir of finite mass capacity but of practically infinite exergy capacity. From this alternative point of view, it clearly appears that exergetic content, and not capital, is the correct measure for the worth of a commodity or a service, and that the monetary price ought to reflect this new measure of resource consumption*'.

Similarly Brown & Ulgiati (1999 p.491) warn

> '*It may be time to question the reality created by humans that results from their utility theory of value*' [with its monetarization]

and ask (l.c. p.493)

> '*What is the economic paradigm that will help humanity to develop necessary symbiotic interfaces with the biosphere? We believe that it is not a human-centered valuing paradigm based on the flows of money, but a biophysical paradigm, based on the flows of energy that drive and sustain all biosphere processes*'.

And their proposal is to use the (energy/exergy-related) concept of emergy in 'emergy accounting' (l.c. p.488),

> '*a technique of quantitative analysis which determines the values of non-monied and monied resources, services, and commodities in common units of the solar energy it took to make them (called solar emergy)*', adding (p.493)
> '*We have little difficulty in recognizing that the more effort we put into something, the more valuable it is. … Yes. Emergy is a biosphere value, it is the energy the biosphere invests in its goods and services (including the goods and services of society). The more that is invested, the greater the value*'.

As to mainstream economics they explain (l.c. p.492)

> '*We are not suggesting that humans are unimportant, instead we are saying that neoclassical economics (and its reliance on human utility values) has no place in the policy debates surrounding resource allocation and preservation of the biosphere. … Human preference cannot value ecological processes or environmental resources since these processes are outside the so-called economic sphere*'.

The ways in which in the biosphere e.g. solar energy is used to sustain the earth and life upon it – think of reproduction and maturation, and of restoration and upgrading – are only very partially known to us, yet we (our economists included) are completely dependent on them. But it is certain that in our low-energy world these life-processes, including the biological weathering processes driving (!) atmosphere and weather, are all processes at fractal, reactive surfaces/phase boundaries, with both organisms and ecosystems harboring a near-endless surface and hierarchy of such boundaries. As Gorbushina & Krumbein (2005 p.72, 73) indicate for soils and rocks:

> '*Exact fractal dimensions are difficult to calculate at present, but we can assume that the area of some parts of the calcareous Alps or the Mediterranean and Namibian limestone hills, in which biogenic weathering prevails, has an active surface of a minimum of* 10^8 *km*^{2} *instead of the topographically derived* 10^4 *km*^{2} *in a simple Mercator projection.*'
> '*From these fractal considerations, we deduce that the reactive terrestrial surface area for microbial wear down systems in soils and rocks for exchange processes with*

atmospheric carbon dioxide and other (also organic nutrient- and energy-rich) compounds and gasses could be by an order of magnitude of at least 10.000 times larger than that of the oceans'.

At the boundaries within the ecosystem, as well as at the boundaries within the organism, our high-energy, industrial means are completely incompatible. In other words: with our post-war energy-intensive growth mania we stood the world on its head. Its productivism, for example, was at cross-purposes with e.g. true energy efficiency:

'*The shorter the time span within one wants the process to be completed, the more energy that is irreversibly dissipated. This inefficiency not only increases considerably the need for fuel beyond the minimum energy requirements; it also increases the amount of material waste generated far beyond the thermodynamic necessity*'. (Baumgärtner & de Swaan Arons 2003 p.120).

Much of it was known right from the start of our Systems of National Account (SNAs), that live from the assumption that we can monetarize everything that really contributes to the economy. When Stone c.s. elaborated the SNAs in their sequels to Keynes' 1940 pamphlet '*How to pay for the war?*' (cp. Stone 1951), the limited value of that way of accounting were perfectly clear already. In this context the first-hand account of Robinson (1951 p. 40) might be definitive:

'*in the planning of the British war economy the national income calculations had a very important but in some senses a limited function. They were of absolutely first importance in relation to budgetary, savings and consumption policy and to the essential task of preventing inflation. But they did not play a central part, either then, or later, in the actual planning of the war effort. That was done almost wholly in terms of the physical resources, and to an increasing extent as the war went on it was done in terms of manpower*'.

Indeed the need for true materials flow accounting became clear enough when in '47 politicians did not respond to the physical accounts provided to them (with express warning of fuel shortages) and a fuel crisis indeed followed (Chick 1998 p.6f.). Yet, it was from '47 on that the move was made to methods of accounting that were exclusively monetary. This was due especially to the fact that economists like Stone c.s. did not consider the limits (l.c. p.10f.).

It makes sense that nowadays energy and/or materials flow accounting is taking centre stage again (Daniels & Moore 2002). It is this physical flow accounting that e.g. shows that extending the Total Material Consumption of the industrial countries to a world population of 9 billion people would result in resource extraction orders-of-magnitude higher than the present value – for which we have already reason enough to doubt its sustainability (Bringezu et al. 2003 p.58). Note that energetic metabolism accounting has come of age also on the national level with studies like those of Haberl (2001a,b). We are quite sure we also need it on a micro-level when e.g. analysis on farm level shows that (Tellarioni & Caporali 2000 p.119)

'*the market … discourages recycling while rewarding imports from the outside, thus undermining the very basis of agro-ecosystem sustainability*'.

Sociologists York, Rosa & Dietz (2003), who are familiar with physical science, used the most widely known of these accounting methods, the 'ecological footprint', to evaluate common claims of e.g. modernization theorists (see Wackernagel & Rees 1996 for a superbly popularized account of 'footprints'; Chambers et al. 2000 extended the material and Rees 2006 gave a more technical account). **None of the claims from mainstream economics and most policy makers as to 'sustainable policy/growth' passed the test**. In other words, we do indeed need this 'physical accounting' to separate the wheat from the chaff.

One of those accounting methods uses 'solar emergy contents' for measurement. It makes immediately transparant that labor that carefully enhances these contents - both labor at the product growth level and labor at the level of the system sustaining that growth - is objectively highly valuable. Its value does not depend on 'preferences', neither those of individuals nor those of governments or TNCs. That makes us aware of the fact that we indeed need such standards to help us valuate the peasant's/small farmer's labor (and the labor of his ancestors!). Conversely, monetary valuation according to the 'preferences' (market and otherwise) of the moment leads us completely astray – as it did in the past half century. As biomass production for e.g. biofuels is a hot item we now have an extra reason not to fall once more in this trap (cp. Haberl & Erb 2006).

Since the post-war 'productivity enhancement' and 'growth policies' have a significant background in industrial war-related production, it stands to reason that post-war accounting includes all the limitations of that background. It is not surprising that 'production maximisation' became the chief focus (with even resource depletion considered income). In the words of Stahel & Jackson (1993 p.279 etc.),

> '*a brief consideration of the dynamics of the production-consumption economy reveals that profitability structures are quite generally determined by the maximization of throughput at the point of sale*'.

And they add (l.c. p285):

> '*Under the existing model, economic growth can only be achieved by higher production volumes. In saturated markets, this means shortening the effective utilization period of products, thus speeding up replacement. This phenomenon, often attributed to the 'affluent society' or described as 'conspicuous consumption', is really nothing more than planned waste production and environmental degradation'*.

Again, with its background in 'limitless' production for war this is hardly a miracle.

1.23. The joyless economy

When this 'overheated industrial production' was claimed as the core of economic prosperity and growth, sales promotion by raising 'preferences' for superfluous and wasteful products became standard (for its pre-war history cp. Marchand 1985). Jezer (1982 p.127) cites Dutton's '*Adventures in big business*' (1958):

> '*Americans had to be 'sold' new habits, new ways of viewing life, new ambitions. Unceasing change and improvement in automobiles, kitchen appliances, foods, clothing, and in countless items adding to home comforts had to be matched by increasing change and improvement in people's desires. The home-owner had to be made to aspire a better home; the two-car family had to become an ordinary occurrence*'.

And he quotes motivational researcher (in the field of advertising) Dichter:

> '*One of the basic problems of this prosperity is to give people the sanction and justification to enjoy it and to demonstrate that the hedonistic approach to life is a moral one, not an immoral one*'.

Reading Bauman 2002 describing the sad end of this track - of suggesting that consumption is for life fulfillment - we realize that the net effect of the (our) economy, as it was shaped by all those 'experts', was dehumanizing indeed (well described by McLaren & Torchninsky 2009). In a profound way their construct makes no economic sense either (cp. Power 2000 for an extended philosophical analysis). But then, the widely known non-mainstream economist Galbraith had publicly warned for this senseless turn of the economy in his 1958 '*The affluent society*' (sarcastic enough to attract attention). The Dutch researcher Dippel had warned even earlier for it, in his in-depth treatment of technological possibilities (Dippel 1952/53). Yet mainstream economists everywhere expressed their polite doubts and, neglecting alternatives (e.g. Rosenstock-Huessy's), continued promoting 'economic growth'.

> In the Netherlands Pen (1959) and Hennipman (1962) were among those economists. Note that this common misdirection of the economy was hardly a fatum. Galbraith and others pointed in other, inherently more valuable, directions, but these roads were not taken. Dippel's voice was silenced by the socialist party bosses, and 'unlimited growth' was chosen as the party doctrine (cp. Dippel 1966, van Veen et al. (eds) 1973). End of the 60s the Dutch monthly '*Science and society*' even refused to publish Dippel's (invited) lecture in which he once more gave an in-depth cultural-philosophical discourse on technology (cp. van Veen et al (eds) 1973 Ch.10). Clearly it was due to an ideological choice that man- and ecology-compatible eco-nomic 'growth models' were passed over. But it was people like Dippel who were conversant with e.g. Rosenstock-Huessy's approach, which was built on nearly half a century of experience and study.

Rosenstock-Huessy was an authority already in the field of labor participation & education when he came to the US as a refugee from Nazi Germany. In the US of the 30s he countered the slogan (that was inspired by despair) '*A citizen is a man who is profitably employed*' with '*A citizen is somebody who can found a city anew*' (that is, after its decay or destruction). It was he who explained about industrial enterprises (1957 S.192 f.):

> '*Sie wurden gedacht, als bestünden sie aus 1000 Arbeitern und 100 Drehbänken usw. usw. Aber der Betrieb besteht aus denen, die ihn bei der Zerstörung wieder in Gang setzen können. Alle andern sind an diese angelehnt*' (see l.c. for remarkable examples from post-war Germany).

Central to his concepts were the teams embodying true 'industrial fertility' (l.c. p.197):

> '*Dort ist der alte Betrieb fruchtbar und Nachkommenbetrieb, wo sich die Scheidung von Kapital und Arbeit rechnerisch nicht mehr durchführen läßt, weil alle Arbeitskräfte sich zu Mitarbeitern bei den Neugründungen der Industrie eignen; denn damit sind sie zu dem Range des kostbarsten Kapitals aufgestiegen.* ***Sie sind unbezahlbar geworden****. Kapital und Arbeit tauschen ihre Plätze, sooft die Frucht der Arbeit, die filialfähige Gruppe, als die höchste Dividende eines Betriebs fühlbar wird'.*

Rosenstock-Huessy and Dippel were faced with 'ideological repression'. Their growth models, though man-and-ecology compatible, lost out to the inherently non-sensical concept of 'unlimited growth'. Note that the track that was chosen was also anti-technical to the core. Its all-out waste, for example, is in no way defensible (Schumacher's emphasis). Quite to the contrary, human- and ecology-conscious technology has since long focused on the design of durable goods as well as on an array of methods for product life extension (in conjunction with component life extension; Stahel & Jackson 1993). What results is a 'service economy' that is technically, ecologically and socio-economically fully feasible: disincentives derive primarily from economic policy, institutional barriers and cultural & psychological obstacles.

The technical requirements of resource sustainability have been well mapped by e.g. Sirkin & ten Houten (1994), which helped them to develop their '*Cascade chain method*'. Once more it is a 'service economy' concept (as with Stahel & Jackson 1993). In all of those concepts care-based labor is at the core (as it is at Rosenstock-Huessey's earlier growth model). That is of central importance to our subject, because in a farmer- and ecology-centered sustainable agriculture we have this same central position for care-based labor. Such a sustainable agriculture is not a ‘sidetrack’ for society, but is at one with responsible, human- and ecology-conscious technology. Yet, to be able to discern what happens in such a responsible agriculture and technology, we need different accounting methods than the ones currently underlying policy and presented by mainstream economics.

> Here the **TLC-factor** is decisive. Schumacher in concert with creative technologists designed small-scale industries where mainline pretended only large-scale industries were profitable. Some examples (Schumacher 1975): chipboard factories for 6,5 ton/day instead of 1000/day (’*eine riesige und wahnsinnig teure Anlage*!’), likewise small-scale sugar processing and cement factories. Significantly he emphasized: ‘*Alle unsere Arbeiten sowohl in der Land-wirtschaft wie auch in der Industrie haben einen Faktor hervorgehoben, der energetisch gesprochen hoch effizient ist… Ich nenne ihn den TLC-Faktor, “tender loving care”*’.

1.24. *Sächlich*?

As indicated, our wasteful economy could only grow to its present proportions when warnings like those of Galbraith were ignored. The 50s were crucial because it was then that man was turned into a ‘consumer’ and the economy was ‘founded’ on his suggesibility. The consciousness of the close connection between real needs and care grew dim (Reader 2005, Wiggins 2005), as did the importance of that connection for economic policy. Pretty soon this led to the present situation in which

> ‘*High costs are placed on the pursuit of certain fundamental needs while low costs are placed on the pursuit of consumption preferences*’ (Power 2000 p.277).

Such confusion, that in the past would have been a signal of growing injustice, was now declared essential for ‘economic growth’.

Of course the post war years, with their obvious need for government intervention in society, needed some ‘system of accounting’. That could very well have been one with qualitative policy goals undergirded with material, energy and financial accounting, all subject to the whole and open about all that was not known or non-measurable. Yet, techno-cratic and accounting claims soon were of the same cut, both not only pretending to oversee the whole, but even to be able to quantify it at will. After some years of transition the UN-endorsed Systems of National Account were the only ones that were left (in the communist world similar systems had the day). With these SNAs in place governments felt sure they were informed about the crucial aspects of the economy, including agriculture. But in plain fact the SNAs, with their very limited conceptual framework, and their lack of foundation in bio-physical reality, made everybody blind to non-monetarized, fundamental aspects.

That was an important reason why governments pursuing their agricultural policies were no longer able to discern that *ruralities*:

> (a) *embody essential natural and anthropo-ecological capital*
> (b) *display an economy that has reproduction and care at its center*, with
> (c) *points a) and b) more essential to agriculture and to the economy at large than money can ever be.*

Presuming that they were better informed than ever, governments everywhere began to shape their agricultural & food systems according to the signals received from their economic institutes, and in so doing missed out on most that was essential. Indeed, both historical research (Thirsk 1997) and the rediscovery of e.g. agroforestry (Cairns (ed) 2007) and other ‘traditional’ farming systems have made it abundantly clear that agricultural progress has always depended on the peasant/small farmer being free actively to explore new possibilities and to disclose a rich palette of natural and human resources.

For one thing the ideological attachment – for it was no less than that – to 'total quantitation' deprived the people involved of the qualities that any quantitation was supposed to serve. There are some close cultural and philosophical connections between the loss of essential qualities and the ideological veneration of quantitation (Kohnstamm 1926, 1942) and we indeed see them at work in post war decades. Also in the neo-positivistic accounts of science and technology that soon became obvious.

> As an example we met Monod's construction (1949) of 'characteristic constants' of micro-organisms. The point is not, of course, that certain 'constants' were postulated, but that everybody subsequently took them for granted. They were used in the design of methods (e.g. 'chemostat culture') that then, ultimately, proved to be empty of contents….
> A similar example of a scientific discipline where reductionist convictions led into a dead end is that part of molecular biology which took the Watson-Crick-Monod model of the DNA for life's super-ruler. This model was doubtful all the way (cp. e.g. Wells 2008) and crashed definitively recently, when it no longer could be denied that DNA is a cog - no less and no more than that – in the machinery of heredity. Cp. Stotz (2006 p.542):
> '*Networks of genome regulation made up of cis-regulatory sequences, trans-acting factors and environmental signals causally specify the physical structure of a gene and the range of its products through the activation, the selective use, and, more radically, the creation of nucleotide sequence information*'. Cp. also Moss 2003 and especially Gould 2003.

In spite of the fact that authors like Jacques Ellul had already warned for the technocratic stance (with its reductionism and neo-positivism) at an early stage, it grew stronger in the course of time. It had its roots before and in the war, yet, due to the Depression the number of people having serious doubts about its big industry-centered concept of economy and society increased significantly. Such doubts had been expressed even earlier, e.g. by the well-known economist Tawney at the IMC meeting in Jerusalem 1927 (Tawney 1927 p.168):

> '*Modern industrialism rests on the concentration of economic power on a scale unknown in previous ages, and the disposition of those who wield it, except in so far as they are restrained by custom or law, is to regard their fellow-men as instruments for the realization of economic purposes. But to the Christian, human beings are not instruments but brothers, and whether the method by which compulsion is exercised upon them is forced labor or a wage contract under which they are nominally free but in fact subject to duress, he will seek to replace it by forms of industrial organization which may make co-operation a reality*'.

And indeed we can notice a.o. an in-depth discussion of the 'just price' concept in the field of economics in those pre-war years (e.g. in the then international journal 'Stockholm'), as well as a public exposition of the need for the re-embedding of economics by academic economists like De Vries in the Netherlands (from his 1935 '*Regeling of vrijheid*' on, cp. Polak 1948). Altogether it is clear that the turn to technocracy after the war implied the neglect of important aspects of those pre-war discussions (refer again to Hennipman's queer eclecticism). But note that, once such neglect was institutionalized, the monolithic character of technocracy hardly allowed any discussion – until it was forced by 'outsiders' to do so anyway, because of the problems ensuing from technocracy in e.g. the fields of energy and ecology.

Also in regard to its reductionism the turn to technocracy could only be made by passing over the previous discussions about reductionism, (neo)positivism, and naturalism. For these had been the subject of incisive criticism in the years before and just after the war (e.g. Charmet

1946. In the Netherlands earlier already: systematic criticism by philosopher de Sopper and by physicist/ philosopher/social scientist Kohnstamm). In those years personalism was still a living philosophy, with e.g. Buber and Berdjajew highly respected proponents.

Before the war there had also been an international discussion probing the qualities of good research (e.g. Grünbaum 1925 and Kohnstamm 1929 esp.§22). It was closely related to the research on ‘Wissenssoziologie’ as initiated especially by Mannheim and Scheler. Grünwald 1934 gave a valuable overview that became rather well known. The high level of this pre-war research is also reflected in Hofstra’s 1937 ‘*De sociale aspecten van kennis en wetenschap*’ (‘*The social aspects of knowledge and science*’).

With many first-rate social scientists like Mannheim becoming refugees, the Nazi regime and World War II initiated a rupture also in research on ‘Wissensoziologie’. The Nazi regime first, next the war elsewhere too, initiated an immense amount of research aiming at ‘power’ over any subject that was supposed to be important for the war effort. Yet, even in post-war Germany, Scheler and Mannheim still had their ‘pupils’, and they offered us some important publications in post-war years. An author that should be mentioned in this context, is H-E. Hengstenberg, whose succinct definition of ‘Sachlichkeit’ (1957 S.9, 12) was well received:

> ‘***Unter Sachlichkeit verstehen wir jene Haltung, die sich einem Gegenstande um seiner selbst willen zuwendet, ohne Rücksicht auf einen Nutzen***’ (emphasis J.V.)
> ‘*Der Mensch ist des Zweckentbundenen Interesses fähig. Der Beweggrund seiner Zuwendung zu den Dingen geht über alles hinaus, was sich unter dem Gesichtspunkt einer Dienlichkeit für ein Subjekt formulieren läßt. Sachlichkeit ist* ***Konspirieren mit dem Gegenstande****, Mitvollzug seiner ihm selbst eigenen sinnhaften Struktur*’.

In fact, Mannheim, Scheler and Hengstenberg, partook in an age-old discussion, cp. Pascal’s “*l’amour et la raison n’est qu’une même chose*” (in his ‘*Discours sur les passions de l’amour*’). Many a scholar already had pointed to the need for empathic attention to the subject of research and discredited the suggestion that some ‘power approach’ would do. After the war physicochemist/philosopher Michael Polanyi and physicist/philosopher Loen further refine such empathic, non-reductionist approaches to science and research (Loen 1948, 1963, 1965, 1973; Loen graduated in 1927 with De Sopper).
But note that institutional post-war research with its accelerated growth had its origins in important ways in the war economy. Dominated by ‘functional rationality’ (*sensu* Mannheim) it was not even ‘utilitarian’, for as Hengstenberg warns (l.c. S.29), it

> ‘*es muß gesagt werden, daß ein jedes utilitäire Zuwenden zu Objekten nur dann sittlich einwandfrei ist, wenn es zugleich durch ein sachliches Verhalten, das aus unserer tieferen Wesensschicht kommt, überformt ist*’.

Research dominated by ‘functional rationality’ is not interested in ‘Konspirieren mit dem Gegenstande’, but only in reaching the predetermined goals as quickly and ‘efficiently’ as possible. Neither ‘sachlich’ nor ‘utilitäir’, it mostly implies a short-cut that is ‘sachlich’ impossible (!) and therefore is leading to aborted results. Yet, during the post-war half-century society was increasingly dominated by this puzzling kind of research & design.
Note that its roots are in the restricted kind of industrial research in which, because of the narrow focus of the production process that is confined within the factory walls, human (incl. social) and ecological effects are easily ‘wished away’. A narrow kind of technological research results, which is not convincing to the thoughtful technologist.

Christians (1989) opts instead for **sufficient design**. '*The notion of sufficiency in design is meant to supplant the narrow concept of technical, financial, and marketing efficiency so often used as guides for the development of design specifications. Even from the practical standpoint of product liability, these efficiency criteria … are not adequate for proper specification of a tool or product which must operate in everyday situations. In this light, appliances, for example, would be designed in terms of their dependability, disposability, and reusability, …*'. For related, ecologically conscious, 'sociotechnological design' refer to van Eijnatten (red) 1996, Vanderburg 2000.

1.25. Growth of technocracy, not of technology

Still, under the post-war regime of functional rationality this 'industrialization' of life and society was the highest goal. Economists and policy makers took the aborted technology for the real one. Vanderburg (1987 p.118 f.) as a philosopher of technology sketches the post-war course of events in a way that we will repeatedly find substantiated. Post-war society was shaped increasingly in accordance with this technocratic ideology, in four stages:

> '*The first stage comprises the study of some area of human life for a particular purpose. The results of the study are used in the next stage to build some kind of model that can range from a precise mathematical theory to one that is largely qualitative. In the third stage the model is examined to determine what happens when its parameters are altered in order to discover when it functions optimally. The technical operation concludes with the reorganization of the area of human life studied originally, to achieve the highest efficiency and rationality demonstrated possible by the model. It is by means of this pattern of events that modern society seek to improve the productivity of a plant, the running of a large office or hospital, the effectiveness of classroom instruction, …. As a result the technical operation deeply permeates the fabric of individual and collective life*'.

This 'technical operation' and 'technical way of life' are fundamentally a-technical (Vanderburg 2000). Whatever it is applied to

> '*is separated from its social and natural contexts. It is then improved on the basis of criteria which make no reference to the way it fitted into and will fit into these environments. Efficiency, cost-effectiveness, cost-benefit measures are all ratios which compare outputs with inputs to* ***internally*** *optimize some activity or process without any reference to how any improvements are going to fit into the socio-cultural matrix of a society or into the ecosystem*'.
>
> '*The technical way of life, therefore, produces a variety of tensions within the socio-cultural fabric of a society as well as straining the balance found within the ecosystem. The technical way of life is in sharp contrast to the 'rationality' of traditional societies based on custom and tradition, which embodied a variety of values able to adapt any part of the socio-cultural matrix to new circumstances without losing sight of the integrity of the whole. With varying degrees of success, earlier cultures had a 'rationality' embodying values related to both the internal and external functioning of any aspect of their way of life*'.

Due to the ongoing application of this 'technical operation'

> '*the traditional socio-cultural fabric is largely replaced by a system of inter-dependent techniques forming the new framework for society.*
>
> *The technical operation also separates knowing from doing, the knowers from the doers, and externalizes the control over a technicized activity. As the diversity of*

> *interdependent technicized activities grows within the socio-cultural fabric of a society, control over these networks of activities tends to be centralized in ever-larger institutions, such as the transnational corporation and the modern state*'.

But with the neglect of man and ecology built into the approach, the adverse effects soon mounted, with as a result

> '*a growing need for the regulation of an ever-growing range of activities. The separation of the regulatory and control functions from the activities themselves makes this regulatory apparatus much less effective than the traditional ones, which were largely built into these activities themselves. It is also much more costly to run and maintain*'.

Vandenburg summarizes:

> '*By means of a variety of fundamental assumptions or myths about the nature of society, our past, present, and future, and what constitutes genuine human well-being, we have pretended and continue to pretend that our striving for micro-level rationality and efficiency will translate into improvements on the level of the whole. In living systems, however, where the properties of the whole cannot be derived from those of the constituent elements, this is not the case. As a result, many of our advances on the micro-level are undercut by massive problems on the macro-level*'.

Note that this system of 'technique' is definitely not 'sachlich' (*sensu* Hengstenberg) because

(a) it puts its models in place of the 'Sache' and

(b) is reductionistic to the core.

We will see time and again that a reductionist approach is of very limited applicability even in the a-biotic sciences, let alone outside them. Based on this 'inside knowledge' Ravetz and others dubbed a discipline that yet was built from the propositions indicated a '*GiGo science*': '*Garbage in, Garbage out*'. Note that the reductions that are 'needed' for the construction of this kind of models are not 'sachlich': they derive from the modeler's goals and are then imposed on reality. In other words, reality is 'shrunk' to a neat system that then as a model allows manipulation by the professional. It follows that the results of his modeling cannot be 'sachlich' either. When the industrial or governmental bureaucracy imposes them onto reality anyway, it is bound to use (brute) power to effect this.

As indicated, we did not just stumble upon this bureaucratic process: there surely was an awareness of the need for 'Sachlichkeit' in post-war years. Yet in those years the number of scholars that was aware of the discussions about the subject soon dwindled in proportion to the new generation of researchers and experts who exclusively took pride in their role in technocracy. Quite likely disillusioned by the world passed on to them by their parents, as well as entertaining great hopes to open up the future with (their) S & T, the new experts neglected the work of the previous generation. And even where they did not neglect it, the institutions they worked for hardly left any room to use it.

Then very soon the fashionable a-historic presentation of science resulted in the 'linearization' of its history. Earlier research was only regarded as valuable if it has somehow contributed to and confirmed the current dominant trends. Also 'science' had to conform to High Modernism (at both sides of the Iron Curtain).

With (mainline) agricultural research at the core of the government's post-war High Modernity projects, 'linearization' became fashionable in agricultural history too. History was interpreted as the time 'leading up to' industrial fertilizer and fertilizer-responsive varieties, while the key-figure in agriculture, the peasant/small farmer, was virtually eliminated from history, as were his practices, only to be re-discovered recently.

1.26. Back to basics

As to particulars, Power (2000 l.c.) stressed a major thrust of the High Modernist project:

> '*We have fashioned our larger social and economic institutions to facilitate rootlessness'. … 'Those seeking to maintain a commitment to place and people are forced to pay a high price'* .

And indeed this 'rootless society' attaches no value to the soil and to the one cultivating it. It has no place for the key-figure that is firmly 'rooted' in the local soil, ecology and community, the peasant/small farmer.
This development had its roots in pre-war years and has been analysed by Simone Weil (Weil 1949/1990, English ed. 1952/1987). She noted (English ed. p.83)

> '*In all matters connected with things of the mind, the peasants have been brutally uprooted by conditions in the modern world*'

and subsequently presented her proposals for a true re-rooting of the young peasants (a.o. by providing an education that in all aspects gives them knowledge of and love for rural life).
About Weil's book T.S.Elliott in his 1952 foreword to '*The need for roots*' wrote:

> '*This book belongs in that category of prolegomena to politics which politicians seldom read, and which most of them would be unlikely to understand or to know how to apply. Such books do not influence the contemporary conduct of affairs: for the men and women already engaged in this career and commited to the jargon of the market-place, they always come too late. This is one of those books which ought to be studied by the young before their leisure has been lost and their capacity for thought destroyed in the life of the hustings and the legislative assembly; books the effect of which, we can hope, will become apparent in the attitude of mind of another generation*'.

Apparently by then things were taking a course already that caused great doubts in Elliott – as it did in Ellul and many others.

Part of it was, as indicated, the return to reductionism in science as related to policy.
We see it in full swing in the *GIGO-sciences* (Ravetz 1990 p.196):

> '*the policy-relevant disciplines dependent on mathematical models where the uncertainties in the inputs must be suppressed lest the outputs become indeterminate. Such GIGO-sciences (Garbage In, Garbage Out) have a role in statecraft analogous to that of classical astrology*'.

Economic modeling soon became one of those disciplines. Since the **models were presented as allowing an all-inclusive analysis of economic reality**, there was no choice but to leave our turbulent reality and, using bold assumptions and gross simplifications, construct systems that are 'well behaved' and deterministic. **Only by greatly shrinking the system while 'modeling' it, economists can maintain its predictability** (cp. Breslau & Yonay 1999). That is bad enough in itself - but it becomes disastrous when the models are 'feeding' policy.

In their application to agricultural policy, the limitation of accounts and models to monetary flows has as its chief effect that *economically* central requirements to agriculture are deleted completely. Soil husbandry, natural resource building, and maintenance of a true agro-ecology, for example, are central to sustainable agriculture and food production, but do not 'produce' anything that appears in the accounts or the models (as in e.g. Abert 1969). This is not just because it is impossible to monetarize them, but more embarrassing still, because standard agricultural economy has no notion that they are essential to agricultural production.

Remember that post-war agricultural policies actively discouraged or even abolished them, due to an economic approach that only allowed industrial inputs.

Economists are not alone in the shrinking trick indicated. In many a policy-related discipline modeling is made 'possible' by similarly shrinking the system until it obeys the modeler's wishes (or those of his superiors). Everywhere we meet this same 'projective modeling': from energy forecasting to nutrient modeling in agriculture (cp. Pilkey & Pilkey-Jarvis 2007). That process becomes positively dangerous where its 'shrunken reality' is imposed by the government at the level of daily life, e.g. at farm level.

As to distortionary influence, the Systems of National Account (SNAs) have done more than any other instrument to blind everybody as to non-monetarized or non-monetarizable revenues and costs (for early criticism see Goudswaard 1970). Missing out on some of the most central and essential aspects of the economy, and yet claiming they gave reliable guidance, they gave occasion for e.g. free rider behavior of economic agents (incl. governments). It dawned only slowly that **SNAs were unable to spot the externalization of costs or other grave distortions of the economy because they were actively distorting the picture themselves** (e.g. accepting resource depletion as income). Gradually it became apparent they are defective also at a more fundamental level, because they are largely blind to

(1) forms of capital that as such are fundamental to the whole of the economy (e.g. social and ecological capital, Goudswaard 2006), and to
(2) core elements of the economy like reproduction and the free provision of care in households and neighborhoods (Waring 1999), as well as to
(3) the 'service economy' embodied in e.g. soil & ecology husbandry in agriculture and in an optimal utilization economy of commodities (Stahel & Jackson 1993).

Probably their productivist origin in the short-term maximization of production for war sealed their tunnel vision. When they were propelled to the center of economic policy anyway, the effect was the imposition of all-out industrial production for war on all segments of the economy. It was hardly a miracle that e.g. households and ecologies were lost sight of by economists and politicians.

The 'collapse' of economic modeling and SNAs is first of all a tragedy. A core element of the post-war ideology of High Modernism - with its optimism about the manageability of nature and society 'for the common good' - proved vacuous. The expectations of the post-war generation have been shattered (cp. Tinbergen's 'maximalization of national income' as the goal of economic policy, Tinbergen 1959; further Abert 1969 Preface, van den Bogaard 1999). However, when we realize that this High Modernism was also the ideology justifying the imposition of the government's power projects (from the expert center), we start wondering if this tragedy might also be a starting point for the re-discovery of the small & local life of people & plants. (See Weil 1952/ 1987 p.217 for a consideration of some grave questions that we leave untouched for the present).

It is obvious that we should never start again with an 'ideology of power', with concepts and practices that disregard children and plants from the very start. An obvious solution is to give the small & local, including children & plants, a definite central place in our models of the economy. Such a thing was indeed what the Finnish economist Laura Harmaja, whose major work was published in 1946, envisaged with her studies in the role of the household economy and household production into Systems of National Account (cp. Pietilä 2002). But the US-based, UN-endorsed post-war SNAs were a great hindrance in accepting this and similar profound approaches to the economy. Because they monopolized policy-related accounting and modeling, they helped mainstream economics to become increasingly inflexible.

In regard to agriculture vs. industry Pietilä (2002 p.26) seems to hit the nail on the head:

> '*The most fatal shortcoming of prevailing economics as science is that it does not distinguish the* ***cultivation economy*** *from* ***the industrial economy****, the living economy from extraction and manufacturing*'.

She is also right when she writes (l.c. p.27):

> '*Economics as science is based on logic of industrial production, extraction and manufacturing of 'dead elements', nonrenewable energy and resources. When this logic is applied to the cultivation economy, the same demands of efficiency and productivity imposed on agriculture and husbandry as on industry, the system is bound to run into difficulties'.*
> *'Nevertheless, national and international economies have been run this way…. This misperception and mishandling of cultivation economy is the reason why agriculture has become such a problem both in national and in world economy. This is also the reason why no solution has been found for the food problems of humanity*'.

She and others did more than criticize, they also advanced other concepts and models of growth. Pulliainen & Pietilä in '83 advanced the hypothesis that (Pietilä 2002 p.20)

> '*revival of the self-reliant, non-monetary local and household-based production of goods and services makes economic growth unnecessary in small industrialized countries like Finland without necessarily jeopardizing the quality of life*'.

Authors like these – including many feministic and Third World economists – are correct when they say that the local life of children and grown-ups in households and neighborhoods must be situated at the center of the economy, as the essentially **free economy** of non-monetarized care and reproduction. Note that a classic definition of economics as the study of the provision of materials for households fully allows this approach (cp. Halperin 1977). As a matter of fact, all of the post-war institutional economics that is related to the work of K.Polanyi c.s. focuses on such a concept of **substantive economy** (e.g. Mendell & Salée (eds)1991, Hollingworth et al. (eds) 2002).
As the example of breastfeeding demonstrates, High Modernism refused to recognize the fundamental character of this free economy. Likewise, it only recognized (monetarized) industrial inputs to agriculture, while actively cutting access to & use of (non-monetarized) soil resources at the same time. But of course - *vide* breastfeeding - all of the fundamental resources of the free economy are in need of protection. We want to keep them accessible to the common (wo)man and prevent them from being replaced by monetarized, external supplies that are not only inaccessible to many people, but **also lack the fundamental qualities of the free resources**.
Note that many authors have pointed out that post-war 'economic growth', more often than not, consisted in shifting functions from the free, non-monetarized centre of the economy to its monetarized surroundings, essentially suggesting an imaginary growth. This of course makes no economic sense, and any healthy economy takes great care to protect its life-supporting centre. It is a most foolish development of the 'globalized economy' that it removes such safeguards from e.g. the agricultural economy.

An economy is viable only when:

(**A**) its life-supporting core is (1) delocalized (2) non-monetarized (3) rooted in (fed by) the local ecology, household and community (4) allowed to manage its own local resources

(**B**) protection from conflicting interests, and especially from infliction by monetarizing interests, is provided for (1) the life-supporting core (2) an array of economic activities feeding public life

(**C**) the 'market economy' (in whichever form) with its monetarization is relegated to the circumference, yet is (1) regulated to prevent 'anarchic' tendencies taking over (2) held accountable for its support of the life-supporting core and of public life.

Note in this connection that 'economic gains' of scale enlargement only too often stem from cutting off community- and ecology-life-support-functions from local enterprise – which, in fact, causes heavy losses to the local and national economy.
It is only when working from such a concept, acknowledging that local care is central to the economy, that transnational economic activities can receive their rightful, life-supporting place. Anarchic approaches have nothing to commend, not even when propagated by the WTO. For when fringe developments begin to take over the essential life-supporting center of the economy, they will suffocate life and strangle the economy. If anything, post-war High-Modernity's approach to the agricultural economy exemplifies this process.

There is no need to explore the manifest instabilities of the current globalized economy. Nearly half a century ago a giant actor in the field, China, had the biggest famine in history inflicted upon it by Mao's centralizing policies – that were inspired by the West's ''high yielding' agriculture (Dahlke & Bork 2004). These days the mad scramble for wealth and power by its ruling class initiated a still bigger 'Leap Backwards' (Economy 2007), and yet China is quickly expanding its power in e.g. Africa. Evidently we are in for enormous upheavals when we stick to our 'globalization of the economy'.
Still even these massive threats are no *fatum*, and as China's example shows, the threats are closely connected with derailments starting in the agricultural economy. On the other hand, if China returns to the gentle and local approach, which is the only way to support life, many Last printed 13-03-10 14:56of the present threats will crumble, or at least diminish. There is indeed great scope for 'New Peasantries'.

Addendum: contemplating the current Recession

J.K.Galbraith designated most of our post-war, big-enterprise system, that is at the core of technocracy in both the capitalist and the communist world, as the 'planning system' (Galbraith 1973). The arms industries and the explosives/fertilizers industries are obvious examples, but so are the oil companies and food & seed giants. All are completely dependent on '*systems maintenance expenditures*' (O'Connor) by the government (that spends the bigger part of the budget on them, cp.McCarthy & Rhodes 1992 p.138f.). Only the comparatively small-scale economy that must do without convenient connections with government is the 'market economy'. Galbraith explained (l.c. Ch.18):

> *'The planning system, in the absence of state intervention, is inherently unstable. It is subject to recession or depression which is not self-limiting but which can become cumulative. … The consequences of recession … in the planning system then over-flow with profound and damaging effect on the market system. The latter suffers more from recession than does the planning system wherein the instability originates'. 'Hardship for the small businessman or farmer is severe. While the market system can contain movements in demand arising from within itself, it is extremely vulnerable to adversity emanating from the planning system*'. '*With the rise of the planning system the economy became systematically subject to downward instability – to recessions*'.

Note that from this point of view (1) the breakdown of communism was part-and-parcel of the inherent instability of the planning system, and a sign that its capitalist equivalent was soon to follow (cp. also Goudzwaard 1993) (2) the never-ending story of post-war farm foreclosures originated probably in the inherent instability of the 'cartel' of government and agro-industries, not in that of small-scale farming. It witnesses to the extreme petrification (*sensu* Mannheim) of post-war, mainline economics that those evident signs of instability did not penetrate.

> A 'financial economy' was introduced, instead, that proved wholly vacuous – and that parasitized on the broader socio-economy. Note that Galbraith's analysis derived not only from his close knowledge of big enterprise, but also from his close analysis of the Crash and the Great Depression, and from his wider knowledge of economic history.

But then, mainline economics excluded exactly the critical reflection and discussion that alone would have enabled it to develop as a living tradition (*sensu* McIntyre). Instead of that,

> '*Les économistes apparaissent ainsi comme des producteurs de calculs, d'estimations quantitatives, commandés puis récupérés par le pouvoir politique enfin de prendre mesure des forces productives de la nation et apprehender du même coupe les dépenses qu'elles occasionneront'. 'Aspirant à devenir des scientifiques, … les économistes ne s'aventurent pas sur le terrain de la critique intellectuelle d'un système économique vis-à-vis duquel ils produisent des théorie et des modeles qui le légitiment, faisant de la croyance en ses bienfaits une quasi-religion'. 'On y verra certainement une homogénéisation abusive de la profession*' (Pouch 2009, 65, 67, 69).

Note that post-war planning is all of the 'end phase' type, in which a future situation is designated as the truly desirable one and planning is part of the systematic implementation of this pre-set goal (van Houten 1978 p.33). '*Evaluation is extremely rare, so that feed back is impossible*' (l.c.). Indeed the closed character of economic studies in those decades is only too evident, never attaining to a real distance from the 'planning system' with its functional rationality. Warnings were sounded for the extreme destruction of capital that is a result of the evident failure of many of those big projects (l.c. p.32), but to no avail, a sad proof of the petrification that is inevitable where functional reality leaves no room for ongoing discussion and evaluation. Yet, the choice for alternative approaches to the economy, with accounting methods that openly acknowledge the limited possibilities of quantification as well as the severely limited scope of monetarization, is neither 'wishful thinking' nor 'unscientific'. E.g., as to openness about limits of quantification Functowicz & Ravetz' approach has been around for quite some time (Funtowicz & Ravetz 1990). In it science-for-policy is (1) open, in a systematic way, about the qualities of the quantities that it presents (Ravetz & Funtowicz 1990) and (2) is open also about the wider imponderables and uncertainties (Ravetz 1990a). When such possibilities for systematic openness are not being used it is difficult to avoid the conclusion that openness is not being sought.

The need for a complete re-evaluation of dominant economic theory and policy is proved also by the massive derailment of, especially, the economy of big enterprise that has been demonstrated by first-rate authors (Witteloostuijn 2001, Brockway 2001, Stiglitz 2003). That mainstream economics was a prime factor in this derailment derives from its stubborn adherence to money as the one and only indicator of economic value: one could be sure of 'growth' if 'profits' said so. And so the 'growth' of the Military Industrial Complex was accepted at face value. Likewise, when growth in Hollow Corporations (relocating production to low-regulation regions) accelerated from the late 60s on, 'growth' was once more 'proved' by the extra profits made, irrespective of losses to the local/national socio-economy (which includes the irreplaceable loss of practical know-how). More recently, economic experts were once more content when 'growth' was ascertained by boosting food speculation. Penetrating criticism of any of those economic 'possibilities' is a must, if an economic advisor really wants to show the way to a healthy economy (cp. '*Postneoliberalism – A beginning debate*', Dev.Dialogue No.51, Januari 2009). Yet, even now, with a deepening Recession, mainline economists do not come up with such probing analyses.

> Just like they kept silent when, a few years ago, a M.P. from Sweden in the European Parliament presented a report showing that for some decades the sex industry had been the motor of EU 'economic growth'. Evidently Söderbaum 2000 does not exaggerate when he speaks of the '**Monetary reductionism**' of neo-classical economics.:
> '*Neo-classical economists … may understand the difficulties or even impossibility of meaningfully estimating 'option values' and 'existence values' in monetary terms. They nevertheless stick to the idea, or I would say illusion, of monetary valuation. The alternative of giving up the idea of a monetary calculation at the level of all impacts does not seem to have been considered and other issues about paradigms and ideology are largely avoided*'.
> And he adds: '…*is this particular conceptual framework and ideology a fruitful one for our attempts to deal constructively with environmental and development problems? Do the analysts or 'experts' really know what they are doing when reducing a multidimensional complexity to some alleged monetary equivalent?*'

The choice of post-war mainstream economists to avoid '*issues about paradigms and ideology*' (Söderbaum 2000) and to refuse discussion and cooperation with e.g. institutional economists – exemplified by their 'deafening silence' towards the publications of K.Polanyi c.s. – meant that their discipline cut itself off from tradition. Since progress in a discipline is always dependent on renewed discussion within a tradition-conscious field of human activity (MacIntyre 1990, 1981/1984), the utter neglect of tradition as illustrated by the dominant a-historic approach of mainstream economists boded ill. For as Hodgson writes about his own journey of discovery in the more recent history of economics (Hodgson 2001 p.xiii/xiv)

> '*I discovered that there was a Lost Continent of theoretical, methodological and empirical studies in economics, largely hidden under the twentieth-century rubble of fascism and war. It contained important thinkers who should in justice rank among the most important social scientists of the twentieth century. Yet they have no memorials in the technocratic domain of modern mainstream economics. Few from the Lost Continent have escaped this oblivion. Many are absent from the classrooms and the textbooks of modern, global academia*'.

As to this 'absent-mindedness' the efforts of High Modernity's social sciences to present their disciplines as timeless truth fit for propagation all over the globe were a great 'help'. Hodgson writes about his explorations behind this façade (l.c. p.xvi):

> '*For example, despite his revolutionary contribution to macroeconomics, J.M.Keynes is revealed as fostering a neglect of the problem [of historic specificity in science] and helping to promote a postwar fashion for general theorizing in economics. At about the same time, Lionel Robbins attempted to place microeconomics on ahistorical foundations and Talcott Parsons did much the same for sociology*'.
> (refer to l.c. Ch.13 for a close analysis).

> Considering e.g. that Parsons did so while denying the historically extremely well-versed social scientist Rosenstock-Huessy – then a refugee at Harvard from Germany – any position or influence already gives an indication of the qualities of this denial of history and historical specificity. But the refusal of history-conscious discussion did not lead to 'global victory' but likely delivered the death-blow to mainstream economics. Reading e.g. Aristotle (Aristotle

n.d.) one surmises that a discipline which neglects its history, does so at its own peril. Indeed, mainstream economists and other High-Modernity social scientists made a grave error in effectively institutionalizing Henri Ford's dictum 'history is bunk'.

For sure, time and again economists well-versed in mainline's theories analysed its short-comings (e.g. Bürgenmeier 1992) and proposed alternatives (e.g. Dumas 1986). Yet, mainline economics shrank back from the consequences, and found an easy way out in marginalizing the best informed members of the guild (like they had done with Boeke after the war). Indeed, there are so many valuable non-mainstream economic publications that mainstream must have done its utmost to ignore and/or marginalize them. Just a few more of such publications that I came across in the course of my research:

(a)
paramount examples are those that use a careful historical perspective – besides works already mentioned Bürgenmeier's '*The social construction of the market*' (1996), and Waring (1999, 2nd ed) as an example of the valuable contributions from feministic side (note that such publications have since long been endorsed by 3rd world authors, e.g. Lee-Smith & Trujillo 1992)

(b)
Haan and Goudzwaard leave no doubt about the value of the contributions of Latin American authors which are highly critical of main-stream economics (Assmann & Hinkelhammert 1992 analysing central concepts of main-stream economics *as ideology* is highly relevant)

(c)
in the Western world, Daly & Cobb 1993 is already a non-mainstream standard work, congenial to Goudzwaard's various publications in which the re-socialization and re-ecologization of the economy figure prominently. 'Institutional economists' have steadily continued their research during the past decades, in spite of continual marginalization by mainline economists (cp. Mendell & Salée (eds) 1991, Hollingworth et al. (eds) 2002)

(d)
in addition to authors already referred to (e.g. Brockway 2003), there is a great variety of critical authors (e.g. Dumas 1986, Robertson 1999, Engelen 2002). And surely no economist can afford not to read works like Bourdieu 2000 and similar works (e.g. Danby 2001) which provide a broader social-science critique of economics.

More than sixty years ago the great economist Boeke stressed that western-style economic policy, in forcing the all-out monetarization of the rural economy also in Asia and Africa, induced its destruction, while not offering any real prospect for the rural population that saw its traditional modes of living disowned (Boeke 1940 p.49f.; 1946 Ch.XI). In the 60s Myrdal followed his lead (in his three-volume 'Asian drama'), warning that 'agricultural development' was mostly leading to an existence-without-prospect (e.g. in slums) for displaced rural inhabitants. Many critical economists from Latin America then stressed the same points, while Albertini in his 1976 standard work applied Boeke's concepts and framework.

Some years ago, Breman with his '*On the way to a worse livelihood*' brought a thorough sociological proof that Boeke had been right, also in stressing that industrialization was no substitute for agriculture. At present, and more than half a century after Slot (1950) 'disproved' Boeke's explanations that the foundations of mainline economic theory were insufficient to encompass non-western economies, Boeke is in high esteem among Third World scholars (e.g. Anghie 2000).
All-in, post-war, mainline economics in its relation with economic policy was not only gravely wrong, but neither has it anything to offer that can help us overcome the current Recession. For a sustainable agriculture, and for sustainable society at large, we have to start

from foundations that are true to the life of men and plants, instead of abstracting from them (in terms of money, etc).

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2. Rurality & agrarianism

deleted from economic

and agricultural policy

From an urban perspective ruralities are far-away entities, not only out if sight, but out of mind as well. It is typical of our age that our urban-based policy makers and populace at large are no longer conscious of the fact that our cities are completely dependent on the remains of those ruralities. The reason seems to be that, while 'food safety' issues are primary news items, food security is not. That is, securing sustainable food production is not part of government policy.
Essential to the beliefs of post-modern man is his firm conviction that modern agriculture, whatever its ecological details, at least solved the issue of food provision. But in reality modern agriculture is non-sustainable and acutely vulnerable. Its vast monocultures are prone to succumb to resistant pests, and the prospects for (computerized) 'precision agriculture' are bleak. Meanwhile, world food stocks gradually decreased, a decrease recently compounded with the growth of food speculation. Already now it is the poor of this earth who pay the bill.

The combination of an impending oil shortage and of the urgent need to revive food security policies, forces us to re-consider the issue of local agriculture and food provision – which entails a renewed interest in the rich palette of ruralities. Yet such a revival of interest is greatly hampered by our massive loss of historical sense. We all are heavily burdened with that anti-historical faith in progress in which 'the best prevailed, of course'. Ruralities are usually considered museum pieces. In order to bring about a change of mind, the historical flame needs to be rekindled.

Such rekindling we need all the more because we urgently need to know the extent to which our devastating 20th century *in fact* has impacted agriculture and rural society. Below I shall give some examples, to help us regain a true historical view, which is a precondition to re-entering the domain of real-life agriculture and food provision.

2.1 The neglect of rurality – an example from agroforestry

One summer day my wife and I were on a day's walk in the hinterland of the Adriatic coast in Croatia, the Velebit mountain range. It was somewhere between Karlobag (on the Adriatic) and Gospić (in the Lika plain). During our long walk we hardly met any people. The low stone walls that were visible everywhere pointed to the fact that this had been a truly rural, agricultural region (cp. Gams 1993). But now the forest had taken over – with the exception of an old graveyard that suddenly popped up in the forest, not far from what once must have been the village of Crni Dabar. Though overgrown with grass, human hands had managed to keep it free from shrubs and trees. Nearby a big cistern, constructed in 1940, indicated that the pre-war government actively assisted the local peasants.

Returning from our walk to the mountain cabins at Prpa, our well-informed warden explained to us that harsh post-war government policies, exclusively interested in (big) industry development, had induced this region's de-population. Formerly it was largely self sufficient, and yielded wool and cheese for export. Now vast stretches were deserted, without a trace of farmers, sheep or cropland. Yet this specific part of Velebit had been rather well accessible, at least since empress Theresa had a good road constructed from Karlobag to Gospić, in the 1730s. The region had been integrated in a wider economy, to a certain extent, until post-war modernization policies put an end to it all.

Post-war agricultural policy in Yugoslavia:
Barić 1967 informs us that '*The first five-year plan, for 1947-1951,…was modeled on the Russian five-year plan, and aimed at raising industrial production to about five times the 1939 level… In agriculture, the production targets were remarkably specific …*' (p.258), and adds (p.259): '*Apart from collectivization, early plans for agriculture included compulsory deliveries at extremely low prices, detailed sowing-plans – so that farmers no longer had any control over the crops in which they would specialize – and discriminatory income tax*'. (Cp. also Waterston 1962 p.12 f.).
Pavlović 1971 explains (p.191): '*government was in a position to exercise considerable pressure on the peasantry, first and foremost through the officially controlled collection, distribution, and sale of foodstuffs, then through its tax policy. As early as the summer of 1945, the compulsory sale of surplus production to the state was introduced, the quotas and prices fixed [very low] in advance. ….Non-delivery [of the full quota] was severely punished, often by confiscation of the land [*with the peasants sent off to compulsory labor projects – l.c. p.194 n.25*]. No credit was available to the private farmer; he paid higher taxes; it was made difficult for him to obtain consumer goods, basic tools, and equipment*'. (Note that licensing of craftsmen was '*at the discretion of local authorities*' – l.c. p.216).
Bombelle 1968 p.23 relates: '*In addition to a very low level of investment in agriculture, peasants were subjected to many restrictions and inequities. Examples include high delivery quotas for individual peasants, exorbitantly low prices of agricultural goods, confiscation of mach-inery for the benefit of new machine tractor stations, inability to obtain credit, politically motivated chicanery and threats, and absence of any favorable perspective for the future*'.
Finally, Bičanić 1973: '*The land reform of 1945 transferred great areas of forest to state owner-ship* [already before the war encompassing large tracts, Alcock 1977 p.569] *along with quite large areas of agricultural land. .. Village shops and inns were confiscated and replaced in 1948 by cooperatives and state commercial enterprises. This was more for political than for economic reasons…. Village forests and pastures were also made part of the new state sector*' (pp. 24,25).
Mountain areas covered about two thirds of Yugoslavia (Hoffman1975 p.258, Halpern 1969 p.320), yet, public investment in their farming communities, after a limited pre-war start, was at a zero level (Hamilton 1968 p.191 f.). Access to their communal resources now was greatly limited, while forced-labor projects with 'surplus rural labor' (Hamilton 1968 p.99, McDonald et al. 1973 p.355) often left only women, children and the aged for agriculture. Combined with still other types of local harshness of policies (Wolff 1974, 429f.), and in combination with the deep scars of a horrible war, this boded the end for communities that had proved viable for centuries (Dalmatia: l.c. p.432).

With the destruction of this rural society, its specific landscape, biodiversity, and culture got lost as well: they had always depended on the peasant's expertise and care. We only caught a glimpse of it, when on our walk we came across patches of old beech forest with lush grass undergrowth. The peasants of old had actively maintained some version of *agro-forestry* fit for this difficult Karst region, and these patches may have been remnants of it.

Gračanin explains (1962 S.265): ‘*für die Bodenerhaltung auf Weideflächen ist es aber im Karst fast immer viel besser, wenn sie von Bäumen geringen Schlußgrades (0,1- 0,3 und mehr) bewachsen sind, die dann einen Windschutz bieten, als wenn der Bodenschutz nur durch die Krautschicht erfolgt*’.
As to goats ‘destroying’ forests, Papanastasis’ ‘*Integrating goats into Mediterranean forests*’ (1986) explained that only under rather closed canopies, where understory vegetation is lacking, goats are forced to browse on seedlings/young branches. Since then specialist research linked up with peasant practiecs (review: Papachristou et al. 2005), and although many foresters still are repeating the old prejudices (for a rebuttal see Vera 2000), agro-forestry research now re-discovers the rationale of the peasant’s practices (e.g. Watson et al. 1984, Papachristou & Papanastasis 1994, Lin et al. 1998).

But there were even no longer any traces left of the patches of cropland that the peasants had succeeded in establishing and maintaining for centuries, with such inventiveness and diligence. Maybe they practiced some type of wheat-sheep rotation; that had been of great importance in parts of Dalamatia for centuries (as in other regions around the Mediterranean, Trenbath et al.1990 p.344). In it the annual pasture plants survive as dormant seeds or as weeds in the cereal crops, with their associated nitrogen-fixing Rhizobium strains. These Rhizobium strains probably inhabit the rhizosphere – or even certain tissues – of the cereal landraces, used by the peasants, as plant growth promoting bacteria. With a life cycle adapted to the leguminous weeds, as well as to the cereal, these Rhizobium strains were at the base of a regenerating cropping system (but one that is destroyed by herbicides as well as by industrial fertilizers).

There is not much literature on the plethora of regional farming systems, that till recently were found everywhere in Europe (and elsewhere). Especially specific agro-ecological descriptions are very rare indeed. As to former Yugoslavia, Lodge’s ‘*Peasant life in Jugoslavia*’ (1941) is a rare work focusing at sympathetic descriptions of daily life and practices of the peasants. Beuermann’s ‘*Fernweidewirtschaft in Südosteuropa*’ (1967) likewise contains sympathetic descriptions, once more of a scholar who for quite some time lived in close contact with the people that were the subject of his research. But it was quite disturbing to discover that most authors, by far, are ready to ‘sit in judgment’ on a peasant whom they do not know (e.g. Hamilton 1968 Ch.9). Other works, like Gračanin’s ‘*Verbreitung und Wirkung der Bodenerosion in Kroatien’* (1962), are of high scientific level in a restricted way only, and give us very few examples of peasant’s practices.

Practices/agro-ecologies, a blind spot: In a series like ‘*Les communautés rurales*’ of the Societé Jean Bodin we find many interesting contributions, but little substantive information on practices and hardly any on agro-ecology. (Cp. Pt.6 on Europe Orientale, as well as all other volumes of the series, Commun.Rur.1986). In fact most authors publishing material about de-ruralisation don’t mention these and similar matters – e.g. Collantes 2006, 2007. As to pre-war Yugoslavia, a well-informed work like Bilimović 1927 touches on several broad aspects of agriculture, but hardly on anything about farmers’ practices or agro-ecology. Milojević 1939 is a rare study offering us some more insight. Later publications that (also) cover agriculture in the Interbellum and/or in the post-war decades in the Balkans/Yugoslavia have still less information on practices & ecology, even when they are of high quality in other respects – cp. Moore 1944, Conze 1953, Fischer-Galati (ed) 1970, Pavlowitch 1971, Wolff 1974, Raupach 1976, Jelavich 1983 Pt.I, Stirk 1994, Aldcroft 1997, Bideleux & Jeffries 1998 Pt.IV, Berend 1998 Pt.II & III.

The Velebit is one of the many Karst regions in Europe and elsewhere, and a third of total area of former Yugoslavia is covered by such regions.

Although there is no scarcity of publications on Karst regions and Karst phenomena in post-war decades, they hardly offer any information on the inhabitants of the regions and their practices. Herak 1972 offers geological descriptions, but chiefly as background to planned water-power projects (Herak 1972 p.54 f.). We learn from him that in the Dinaric Karst above 700m a.s.l. the construction of storage basins (for water power) receives priority, and that agriculture is arbitrarily limited to below 700m (l.c. p.62). A gradual reconsideration of this technocratic approach started only with the major catastrophe with the Lake Vajont dam (Italy 1963; Williams (ed) 1993 gives many examples of such grave problems). That is a chief reason that only recently we find some indication again of the peasant and his practices (e.g. Gams et al. 1993 p.86f.). But by then government policy had induced de-population of Karst regions a long time.
Quite generally, in all those regions with hill and mountain farming that experienced the loss of agriculture already, we can be sure that within a few years the last farmers of these ruralities will have passed away, taking their expertise with them. This will lead to dramatic losses of cultural heritage and the disappearance of the major part of the agricultural expertise developed during the past centuries.

Research within High Modernity's framework: Note that anthropological research on post-war urbanization/deruralization, was mostly situated within the framework of High Modernity, with traditional agriculture's practices and its recent history rarely considered closely, or its members interviewed (Bennett 1998 gives an overview of research on Yugoslavia, Simić 1973 a typical specimen). Such recent history was still 'ideologically sensitive' subject matter in the Yuogoslavia of the 1970s, in spite of the relative autonomy that social research had regained by then (Haberl 1978 S.18f.).
In countries with different political systems, this same ideology ruled the minds, academic minds included. Yugoslavia, with its new class of progress-minded manager-technicians who were at the center of the accelerated creation of a large-scale industrial system, was techno-cratic, but so were other post-war political systems (cp. Denitch 1976, Soergel 1979). Expressing a common ideology, technocracy got institutionalized everywhere. Its High Modernism ruled academic discourse no less than the political one.

The modernizing industrial projects of the government looked more impressive, in post-war years, than local/regional agricultures, and yet most of those modernizing projects have turned out to be dismal failures. Lately the great 'developer' succeeds in commanding huge sums of money in a few places. Yet we're quite sure that he'll be gone pretty soon, and that not long after that most of his projects will start deteriorating. It is important to notice that neither the government official nor the developer is 'more productive' than the peasant of old (of e.g. Crni Dabar). Neither of them has knowledge of the peasant's local resources and of their maintenance and multiplication. But even if they knew about these resources, they still would be strangers to the traditions of the peasant, and so would not know how to make a living with these resources.

In fact, all over Europe, as elsewhere in the world, hill- and mountain farming have always been viable because of the peasants' systems of agro-forestry that, in ecological terms (Scherer-Lorentzen et al. 2003 p.380)

> '*explicitly make use of resource complementarity and facilitation to increase and/or stabilize yields by deliberately selecting species with differing functional traits*'.

Yet as a rule, governments and their experts were oblivious to these farming systems, with their specialized expertise, and considered their practitioners 'backward'. Schumacher was one of the few scientists who realized that post-war industrialization would not do – certainly not for agriculture - and that there was e.g. 'no salvation for India except through TREES':

> '*Just imagine you could establish an ideology which made it obligatory for every able-bodied person in India, man, woman, and child, to do that little thing – to plant and see to the establishment of one tree a year, five years running. This, in a five-year period, would give you 2000 million established trees. Anyone can work it out on the back of an envelope that the economic value of such an enterprise, intelligently conducted, would be greater than anything that had ever been promised by any of India's five-year plans. It could be done without a penny of foreign aid; there is no problem of savings and investments. It would produce foodstuffs, fibres, building materials, shade, water, almost anything that man really needs*' (Preface to Douglas & Hart 1976).

Note that if we consider the recurrent attention, in post war decades, to 'feeding the world', it is amazing that **policy makers were oblivious to the potentials of agro-forestry**. For with the help of trees at least three quarters of the earth can become a habitat for man, supplying him with food, as well as fuel, shelter, etc. In comparison, industrial agriculture 'works' only in specific areas (and even there is accompanied by an array of as yet unsolved problems).

> Cp. Matthews (1989/91 p.201/2) for such an **agro-forestry practice** that created favorable soil conditions for patch-wise cropping, that of 'Hackwald' in e.g. the Odenwald in Hessen. But then, his book is dealing with forestry, not with agriculture… Even the International Council for Research in Agro-forestry, at its foundation in 1977, only had an imperfect notion of these peasant practices (Wassink 1977 overlooked research on hill farming in the UK from the 50s, Nichols 1959). And so research into agro-forestry in the West supposed it had to start from scratch … (e.g. Campbell 1989).
> For years Douglas & Hart (1976 and later) was the only better-known treatment of the subject in the West. Their work harked back to Smith's 1929 '*Tree crops – a permanent agriculture*', but was far less perceptive of peasant practices than this predecessor. Only recently works like Huxley 1999, Ashton & Montagnini (eds) 2000 became available - after publications like Aumeeruddy 1995 and Everett 1995 had acquainted us with *phytopractices* and *forest gardens* of the Third World peasant. Depommier's 2003 overview of agro-forestry practices in India is a must; Cairns (ed) 2007 offers other examples.
> Neglect of agro-forestry in India and elsewhere was connected with colonial government takeover of village common land that got notified as forest and wasteland (cp. Rao et al. 2003 for India, Ecologist 1993 for a broad sketch). Similar expropriations in the present are the 'foundation' of large-scale projects to grow cash crops for the benefit of external agents (van der Ploeg 2008 p.69f. for Catacaos (Peru) and Rice 2008 for the Tana Delta (Kenya)).
> Neglect of agro-forestry systems is closely connected with brute-force government projects of an extractive character that after World War II became dominant. Post-war agricultural policy was aiming mostly at large-scale transformation of agriculture. With agro-forestry ever built from patches of local resources, its practices were 'invisible' to government research focusing at large-scale projects with industrial inputs. Up to the present, the array of established agro-forestry practices has not yet been re-discovered by main-line agricultural research. Both policy and research are dominated still by the concept of 'industrial agriculture' that they started to promote so vigorously, in post-war decades.

Regarding its future merits, this agro-forestry approach is also the only fully sustainable 'solar technology'. For as Georgescu-Roegen pointed out decades ago, all other approaches are still greatly dependent on fuel-based, energy-intensive technologies, for production and maintenance, and so are transitional at best. Georgescu-Roegen's scientific approach earned him the enmity of many of his colleagues, whose attitude to entropy and energy problems he characterized with '*Economists set aside the issue with the opiate "come what may, we shall find a way"*' (Georgescu-Roegen 1976 p.xv). His main-line colleagues did not appreciate his incisive approach to measurement and modeling (e.g. Georgescu-Roegen 1964). Nor did they appreciate his sketch a (near) future for mankind in which an array of agro-forestry systems will once more offer the foundation for material existence (1976 p.xviii):

> '*With the increasing shortage of fossil fuels … the logical panorama for the future of mankind is a radical de-urbanization with most people practicing organic agriculture on family farms and relying on wood for fuel and many materials, as in the traditional villages*'.

Georgescu-Roegen and colleagues. Georgescu-Roegen (GR) was less of a lonesome traveler than one would be inclined to believe from our economic policies. The fact that there were very many more critical economists, who yet were ignored by policy makers, is a research subject on its own.
Part of GR's strength is his intimate knowledge of Rumanian peasantry (GR 1969a). Furthermore his insight in the physical aspects of production – not the least that of natural resource use - allows him to contrast agricultural with industrial production (GR 1969b) instead of conflating them. Reading Vollebergh's 1999 defense – against GR and Daly - of mainline approaches (in environmental economics) one wonders if these mainliners inhabit another world than GR and Daly do.
Quite typically GR's colleagues, active in alternative approaches to economy, had their voice chiefly heard in e.g. non-governmental publications like McGinnis 1979 (describing the vicissitudes of a Gandhian village-centered approach in India). Of late, local community economy of natural resources receives more publicity - e.g. Hanna & Munasinghe (eds) 1995 and Baland & Platteau 1996 – but main-line economists still cling to the suggestion that our technological resources make us largely independent of natural ones.

But as it was, governments not only in Yugoslavia but everywhere in post-war decades pitched their hopes on accelerated industrialization, backed by centralized research. Also agriculture was supposed to be industrialized according to the rules and regulations emanating from government-sponsored central research institutes. With these institutes leading the way to progress & modernity, the study of historic & contemporaneous farmers' practices was considered insignificant.

But surely, everywhere on the globe *it is always the rural where the locus of the co-production of man and nature is located* (van der Ploeg 1997, cp. van der Ploeg 1999 Ch.9.9). So it would have stood to reason to build agricultural policies from this 'shop floor' up. Yet everywhere in post war decades governments chose to turn away from real-life *ruralities,* the peasants' wise integration-into-one of farming, ecology and community (van der Ploeg 1991 Ch.1.3). They substituted a supposedly 'industrial' agriculture in their stead. The example of agro-forestry intimates that in the case of food and agriculture this was a puzzling choice indeed. In the next paragraph we shall look into some details of another agro-forestry region, and into some reasons for its demise.

2.2. Inducing de-ruralisation – Žumberak as an example

It is just before daybreak and from the road outside Gornja Vas, situated at the side of the high hills that are climbing towards Slovenia, I have a wide view of the lower hills of Žumberak. And there is a remarkable detail: it is a summer night in the mid-90s, but there are no (electric) lights to be seen anywhere. (Only at the end of the 90s, when returning with my son in law, we see electrification in progress).

Continuing my walk I soon arrive at the end of the paved road: it continues as a macadam road nearly up to Sošice, the principal settlement of this region (no more than a big village). That is no surprise: on an earlier walk, through lower Žumberak, I had already noticed that none of the roads were paved. In fact there were no official road signs either, except for the nice, wooden ones that the villagers had made themselves. (E.g. in Tihočaj and Jelinići). Summer 2007, when lost with a friend there in the middle of Žumberak, one such an older wooden sign helped us out. Something that I learned to see as symbolic for the great lack of government interest in this region and its population.

The road from Gornja Vas to Sošice was probably reconstructed in wartime. The Germans and the Croatian Ustashas 'needed' it for their ghastly practices (e.g. Pavlowitch 1971 Ch.3). But for the region as a whole truck road construction (macadam roads) started in earnest only in post war decades, primarily because of changes in forestry.

Just like many other rural regions, and contrary to what happened in pre-war decades, Žumberak also suffered from lack of government assistance in (primary) education. Here as elsewhere clerical teachers were banned, but for years the government did not even pay attention to the decline in number of rural teachers. Secondary and higher technical education received all emphasis for years, because of the government's focus at industrialization, while several rural regions lost part of their primary education (Pavlowitch 1971 p.267/8).

> When in the 1960s a primary school was opened in Pečno (a village in southern Žumberak), to accommodate students from the nearby villages, about 120 pupils showed up (two shifts a day). The villages were still lively enough at that time, as is apparent from the fact that each small village had its own small folk orchestra (personal communication by Marin Rimac, Vrhovčak, musician and son of 1960s school principal in Pecno). They had managed to live through the horrible war years and the oppressive post war decades, due to their specific rurality that had proved its viability for centuries already.

The government continued to 'strangle' the crafts by severely limiting licensing and increasing taxes (Bičanić 1973 p.34, Günzel 1954b S.229). It also tried to concentrate and 'industrialize' the existing small workshops, as a rule after nationalizing them (Hamilton 1968 p.94). This led to the shutting down of e.g. water-powered saw-mills and grain mills, and of wood fueled, local lime kilns. Spring 2008 we learned from an old veterinarian that just after the war there had been some 160 functioning watermills, of which presently 10, at most, are left. That means that the region's inhabitants lost access to and use of wood and chalk as local building materials. I remember being puzzled when I, from 1972 on, on my walks through Samoborsko Gorje and adjacent Žumberak met signs of rural-crafts-in-disuse. Later it dawned on me that post war policies had been instrumental in their demise. For before there had been a well-functioning system of rural crafts supporting the *regional ruralities* (evident even now from the museum village constructed in Kumrovec, Tito's birth place in a rural region near Zagreb).

It is clear that the war requisitions, the compulsory deliveries, and the heavy taxes (all *external forces,* for sure), had caused the decline of those ruralities. Summer 2004 I saw with some friends a sad result: a completely abandoned village, near to Sošice, largely overgrown by the forest already. The younger generation had no longer been able or willing to make all those extra efforts that were needed to revive the local rurality, after it had been stripped for decades. They knew that the government still had no sympathy for such efforts (only very recently policy makers started tackling the problem of depopulation). Not only most of the

former rural craft base had been demolished, but the former retailing & small trade network as well. More systematically than before I started pondering the many meanings of the word *industrialization.*
Fortunately years before, on my first walk through Žumberak (starting from Jarušje in Samoborsko Gorje), I had come across some rather well kept villages. There farmers had – as a rule due to cooperation - managed to make up for at least part of the losses inflicted upon them. Together they had preserved enough of the essential crafts, and had learnt how to develop them with the help of recent expedients, to continue farming. And they had found some substitutes for the retailing and small trade of former times. The closer to Zagreb, the more of these flourishing villages I discovered (the regions near to smaller towns like Jastrebarsko and Karlovac show the same picture).

Of course there is more to the subject than I can deal with here. Important for a good understanding of *industrialization* is that in countries like Yugoslavia, especially during the 1950s and the early 1960s, the rise of industry itself was dependent upon peasant-laborers sticking to their 'home base', to meet the vital needs. It was also strictly dependent on handicrafts providing '*a useful source of .. semi-skilled labor which required relatively little re-training for factory employment*' (Hamilton 1968 p.99).

> Ilešić 1971a (p.231) speaks of '***l'industrialisation renforcée** et la **désagrarisation croissante** de la campagne slovène', in the form of a 'suburbanisation dispersée de la campagne*' that is '*accompagnée d'un fort accrois-sement des ménages mixtes*'. He adds '*Il est évident qu'une telle structure est acco-pagnée d'amples migrations alternantes, car moins de la moitié de la main-d'œuvre industrielle en Slovénie habite le lieu même du travail*'.
> Cp. Ilešić 1971b for a more extensive treatment of this and related themes. Where urbanization was more massive, as in the region of Belgrade, such 'alternating migration' was not possible and e.g. housing got all the characteristics of Third World, big city slums, with food yet coming from relatives in the village (Simić 1973 p.94f.). Cp. also Günther 1966 esp. Kap. III.3, Halpern 1969 p.318f.

Facts like these demonstrate how dependent 'industrialization' was on the (forceful execution of the) policies of the government. It also parasitized, in fact, on a basic informal economy of care, plus a fund of crafts.

After the war Yugoslavia had a hard time surviving, yet one wonders if its pursuit of a crash development of its heavy industries was really helpful. The country started its difficult post-war path with increasing its Stalinist measures at home, especially forced collectivization of agriculture, plus the imposition of heavy delivery quotas:

> '*A system of high production quotas and compulsory deliveries of agricultural products, combined with punishment by forced labor for noncompliance, was established for private farmers. All prices were controlled by the state, and peasant incomes were held down by setting purchase prices for agricultural produce lower than average production costs and by charging farmers considerably higher prices for manufactured goods than urban dwellers*' (McDonald et al. 1973 p.355)

In line with the pre-war covering of imports with agricultural exports, the Tito regime wanted great increases in agricultural production '*to provide foreign exchange for imports of machinery and equipment*' (Bombelles 1968 p.39).

Note that machinery and engineering industries were needed for Yugoslavia to process its own rich resources, instead of just supplying them cheaply to the industrialized countries. For in pre-war decades government policy led to

> ‘*a progressive deepening of the penetration of foreign capital, and facilitated the satellization of the Yugoslav economy. Large foreign-nominated enterprises entered the country under the direct sponsorship of the state, supported by tax concessions, favorable freight charges, and similar incentives*’ (Allcock 1977 p.573).

In fact there were some close resemblances between pre-war Yugoslavia and many a Third World country in the present. It is evident that Tito had good reasons to change economic policies.
The real problem is that, in their attitude towards the peasant, Tito c.s. were no different from their predecessors. A contempt of the peasant was current a long time, as is evident from pre-war policies too. The railroads, for example, already a public enterprise in pre-war years, charged very high freight rates to the peasant, compared with the rates charged to big enterprise (Allock 1977 p.569).

> Contempt for the peasant was an old problem. E.g. the contributors to Stead (ed) 1909 give a proud overview of their Serbian state, but they are perfectly silent about the contributions of its large majority, the peasantry with its practices and customs. Only with the Balkan wars and WW I, with their immense loss of life, agricultural reform starts up in earnest.

Already the pre-war government had introduced a number of agricultural reforms (George 1949 p.54 f.). When these proved insufficient, it had prevented dispossession of the indebted peasant (the majority, by then). Then the Tito regime canceled all of those peasant debts, and for a few years was sure of the sympathy of the peasant.
Untill it became apparent that comminism considered him a ‘class enemy’. In Tito’s words (1948, cp.Singleton 1976 p.115): ‘*The peasant holding continues to remain small-scale production. It is here that we have a boundlessly broad and very deep-rooted basis of capitalism*’. From this distrust towards the own majority – more than from a distrust of Stalin c.s. - the Tito regime shifted most resources, by far, into the construction of armament industries (Hamilton 1968 p. 121, Bombelles 1968 p.27-32).
But of course, in the West the small farmer was despised too, e.g. from the comparison of his ‘primitive’ methods with an ‘industrial’ approach. And in most post-war countries the costs of ‘defense’ and the construction of armament industries soon weighed heavily on the state budget. Remember that in those years the Netherlands, France and the UK still fought their colonial wars, that were not only a financial burden to the population, but also prevented the de-centralization of government after its World War II centralization.

Denitch (1979 p.9) gives a summary of the results of three decades of ‘industrialization’ policies in Yugoslavia. It is apparent that is not the political system, but the centralistic, technocratic approach that is decisive:

> ‘*Not only is the peasantry .. still practically excluded from the most dynamic sectors of Yugoslav society and its institutions, but it is subjected to a number of conscious and unconscious pressures which clearly place it in an inferior position within society as a whole. To begin with, the very structure of representation is biased against the peasant. Even the system of representation on the commune councils is such that the socialist sector is guaranteed half of the seats, while there is no specific organization for the articulation of peasant interests as a whole, analogous to the trade unions. To be sure, representation is biased in the same way against the unem-ployed, housewives, and those in the private sector, but the very fact that the peas-antry can be placed alongside that triad is of course unacceptable to the peasants, who have contributed heavily to the economic development of the country as a whole. Culturally the peasants are subjected to pressures from the mass media, which define the only*

desirable existence as urban and modern. The effect is naturally that the young and the ambitious are under continuous pressure to abandon the villages ..'

Under capitalism, the oligopolistic economies of suppliers and buyers leave the small farmer without (decisive) representation. So there is no real difference with communism, and we sense that it is the dominance granted to the large-scale, industrial approach, as connected with the near-complete neglect of ruralities, that leads to the lack of representation of peasant and small farmer everywhere.

Most of those ruralities in Yugoslavia had proved robust and resilient, due to centuries of problem solving by the peasants. The peasants had developed the local natural resources in such a way that primary resources were robust, and that there were secondary resources to fulfill vital needs when the (preferred) primary resources failed. As in most other countries, farmers' varieties of hardy cereals (with modest but stable yields) were at the base of the primary resources, and trees and forests were at the base of the secondary ones.

But then the government imposed strict seeding plans (with prescribed seeds), as well as strict delivery quotas (cp. e.g. Waterston 1962 p.12f.). At the same time the sharp decline of crafts and retailing, in combination with taxes on draught animals and farming implements (or even their forced hand-over to 'socialized' agriculture), left the peasants without the implements they needed. This in combination with government interference in their traditional husbandry and agro-forestry practices, caused them to have neither the means nor the time for the maintenance of their traditional resources. So when a severe drought struck in 1950, it found the peasant with greatly diminished (access to) secondary resources, while the government continued exploiting the meager primary ones (often practically all that peasants had). Serious riots resulted. When a second drought hit the country in 1952, the agricultural output declined to half the 1939 production (cp. Pavlowitch 1971 p.229).

The cause of this disastrous development was, as indicated, the dismantling of the ruralities. By 1952 Tito still insisted on industrialization, but at least he realized that in regard of the peasant he was on a destructive course, and he relieved part of the pressure. Yet only in '59, after the explicit statements by Kardelj, the peasants got reasonably sure that no renewed course of collectivization would follow. Up till then the reconstruction of ruralities had only been possible in part - if at all. By then many a peasant had adapted increasingly to the government's system, if only for pure survival.

In the following years the centralizing industrialization efforts of the government, as coupled with the denial of initiative to the peasantry, led to increasing unemployment in the industrial sector and emigration of young farmers (e.g. Hoffman 1975 p.262/3). These were in fact results predicted by the pre-war agrarians, and were repeated manifold in the Third World, as we know (on agrarianism soon more). Only belatedly the government started paying attention to the peasantry, with its 'Green Plan', the 1973-1985 thirteen-year Agricultural Plan. While giving part of the initiative back to the peasant, the government's frame of mind in regard to e.g. agricultural research remained solidly centralistic.

Then when after the recent civil war the government's war debts to the West soon led to the intrusion of western multinationals in e.g. Croatia, the government proved no longer able or willing to maintain even its partial endorsement of peasant initiative as laid down in the Green Plan. With e.g. local marketing (by peasant women) coming under pressure, the peasant was left without a sufficient 'resource base' to withstand the big retailers' and the 'brute power economy' of the multinationals. Presently the gradual deterioration of rural landscapes began to bear witness to the failures of decades of centralized policies that neglected the rurality-based character of agriculture and food production.

2.3. Denying resources

Yugoslavia is one example among many, all of them different, but all having in common that the relationship between industrialization and de-ruralization is a complex one, with government policy at its center. Everywhere government policy was to blame for the loss of the socio-economic and ecological foundations of ruralities. With an all-out industrialization of the economy considered the only option for the future, non-industrial assets such as those of 'traditional' agriculture were considered non-assets. Then when matters got interpreted from the perspective of the end result - the common pattern in our post-war decades of High Modernity - 'traditional' agriculture got accused of a 'backwardness' which would inevitably lead to its dissolution. So Halpern states (1969 p.342):

> '*Traditional peasant modes of cultivation sanctioned by the yearly round of ceremonies and individual small-scale marketing at weekly town fairs are, in the long run, manifestly incompatible with modern technology and the increasing demands of expanding urban centers*'.

In other words: this monolithic High Modernism approach results in the ongoing destruction of rural livelihoods and ruralities.

Governments and their experts in post-war decades saw the expanding urban centers as token phenomena of 'modernization', not as questionable phenomena requiring close study and strong efforts at correction. Instead of the local provision of foods, feeds, fuel and materials from diversified local resources, this 'logic' brought the provision of big quantities of food or wood from far-away regions that had been chosen for the purpose and reconstructed for large-scale, industry-like production. In reality, large tracts of those urbanized regions could only subsist due to ongoing subsidies in primary products from the urban immigrants' 'home fronts' (in rural regions), plus big increases in 'urban agriculture'. Yet this hardly penetrated to the government, rigidly adhering to its 'industrialization' of agriculture.

To policy makers at large, the 'need' to gain control of big streams of food and wood was proof enough of 'industrialization' as the only modern option, and proof of the lack of relevance of traditional agriculture and agro-forestry. From their bureaucratic center, all they saw of livelihoods was the monetary income allowing people to buy the foods and materials that were provided along the channels controlled by the bureaucracy. But peoples' livelihoods at all times were more diversified than that, or even completely different from it. In the words of Birch-Thomsen et al (2001 p.64), a true livelihood approach

> '*seeks to elaborate on more than just income strategies. It seeks to gain an understanding of resource access, use, and allocation and on the way in which individuals and householders transform resources into livelihoods*'.

Bebbington (1999 p.2028 f.) takes a closer look at the subject:

> '*where rural people have not been able to improve their livelihoods, the principal reasons seem to derive from a failure or inability to:*
> *defend their existing assets; identify and secure opportunities to turn assets into livelihoods; or protect existing ways of turning assets into livelihoods (e.g. by losing a place in a market)'.*
> *'If we were, then, to build a framework for analyzing poverty-reducing rural livelihoods, at a minimum it would need to address:*
> - *the diverse assets that rural people draw on in building livelihoods*
> - *the ways in which people are able to access, defend and sustain these assets; and*

- *the abilities of people to transform those assets into income, dignity, power and sustainability: or in other words, to transform them into*
  - *consumption levels that reduce poverty*
  - *living conditions that imply an improved quality of life according to the people's own criteria*
  - *human and social capabilities to use and defend assets ever more effectively; and*
  - *an asset base that will continue to allow the same sorts of transformations*'.

Local people ever learned to distinguish a multiplicity of environmental goods or services, but their notion of those goods and services, and their actual ability to integrate them into their livelihoods, are shaped by social and institutional factors (cp. Leach et al.'s 1999 '*Environmental entitlements*' analysis). In regard to livelihood building, the greatly enlarged economic and government power that is part and parcel of post-war High Modernism causes external factors to loom large and marginalizes local assets and abilities.

Even 'investing in human capital' is of little help, when that investment is limited to skills and capabilities that fit into High Modernity's centralistic paradigm. That kind of investment means little, when what is needed first is the development of skills for local resource building. If we really want to understand 'human capital', we need to focus at the humans concerned within their local/regional culture, including the skills needed to access and shape local assets. What is needed is a livelihood analysis at the level of the local culture and environment. In Bebbingon's words (l.c. p.2034):

> '*Over and above the meaningfulness of a particular set of assets, then, there is a meaningfulness associated with the set of cultural practices made possible (or constrained) by … certain livelihood strategies. This becomes one more (very important, though understated) dimension of the meaning of poverty or wealth to rural people themselves'.*
> *'Beyond being simply meaningful, such practices are, however, also enabling and empowering. … There is, thus, a conjunction between place and the reproduction of cultural practices that are important inputs to and outputs of livelihood strategies*'.

Note that e.g. 'common property regimes' are culture-embedded and at the same time culture-shaping 'regimes', where practices, assets and skills reinforce each other. Constructing a house with local materials can mean the communal production of chalk in local lime-kilns – that need fire wood from communal resources, etc. It can mean also the assistance of neighbors, or even assistance from the local community at large. The rules that govern the use, maintenance, and development of communal resources are mostly unwritten rules. And yet the members of the local community respect these rules, because they are embedded in their common culture and 'enforced' by some of its highly respected members. Now with the 'enclosure' of common property (cp. Ecologist 1993), external rules and laws are used to dissolve the common property regime, and local culture and communal life come under great stress. Then either the locals find 'a way out', or their culture is progressively demolished, the fate of the Russian *mir* under Stalin (with his murdering of the village priests - the death blow to local culture). But note that it was not just Stalin or Tito who was the 'villain'. Everywhere in Europe it was the total wars, as well as the technocratic regimes, of the 20th century, that made it difficult to escape from external powers effecting consecutive 'enclosures'.

At present we have only some pieces of the puzzle and not its picture: the recent history of peasantries in Yugoslavia and elsewhere in Europe still waits to be written (this is true even for small farmers and their communities in democratic countries like the Netherlands). Instead of giving us historical accounts, High Modernism forced all of recent history onto the one axis ‘traditional-modern’, with the ‘development towards modernity’ presumably ‘driven by the progress of technology’. But surely, neither personal nor communal history is that simple that one ‘axis’ will suffer for description.

Europe’s development in the 20th century can hardly be called smooth: it was shaped by two horrible wars with a deep depression in between. **Any description worthy of the name will take this dark reality and its aftermath into account.** The rejection of the socio-economic and political options that go under the umbrella of ‘agrarianism’ is an important part of it, and it is this subject that we will now turn to.

2.4. Second thoughts on agrarian economies

Pre-war Europe differed vastly from its post-war constellation. This is eminently true of Central and Eastern Europe. The economic integration of the ‘Donau region’ was a very real one, as Karner et al. (1987) were able to prove. About agricultural production in the 1930s compared with the post-war situation Borgstrom & Annegers (1971) write:

> *‘Eastern Europe was a major exporter of agricultural products to the world market. As a region, it was the second only to Argentina as a global supplier. Eastern Europe then ranked as a major supplier of grain, mainly wheat, but also corn, rye and barley to Western Europe. Animal products were exported in quantity from all but Czechoslovakia. ... most of Eastern Europe was exporting a significant proportion of their pulse and oilseed protein.... During the 1934/38 period Eastern Europe had an annual grain export of 3.62 mln. m. tons, almost as much as Canada. ...*
> *... the 1961/65 average reveals a greatly contrasting picture....Eastern Europe has become a net importer on the world grain market and to the extent that neither Argentina nor Australia could, under current conditions, fill these demands on their own. ...*
> *In the prewar period (1934-38) Roumania and Bulgaria were important exporters of dry beans and oilseeds. .. The 1961/65 trade picture in oilseeds and pulses follows a similar reversal to that of the grain trade. Czechoslovakia, Hungary, and Poland have become major importers. Roumania and Bulgaria have continued to be net exporters, but at a considerably reduced volume. .... [Eastern Europe] now [mid 60s] depends upon US soybeans, Soviet sunflower seeds, but – most importantly – African groundnuts altogether with oilseed cakes and meal amounting to more than half a million hectares, computed in tilled land of E.European productivity.’*

The outcome of World War II cut Europe politically in two halves, yet their agricultural economies pretty soon became practically identical. For instead of a truly European agricultural economy we soon had one (!) excessively dependent on imports from far away, *both* halves of Europe accepting an ‘industrial’ agriculture ...that occupied vast tracts of fertile land elsewhere (especially in Africa that needed its fertile land itself)!

It is interesting that Borgstrom & Annegers also cast doubt on a notion that for decades had ruled publications on Central/Southeastern Europe: that of rural overpopulation. In 1965, e.g. Yugoslavia had three times as much arable land pro person than e.g. West Germany or Italy (l.c. Table 1). And of course, there are many regions in Europe where deruralization manifests

itself especially in a disastrous *depopulation* - a phenomenon that is historically connected mostly with devastating wars... And in regard to countries like Croatia, where many regions display plenty of 'space', the suggestion itself of overpopulation is surprising to Dutchmen who are used to a 'landscape' that is tidied up everywhere…
And yet the overpopulation thesis was popular with authors like Warriner (1939 and later) and Moore (1944). These authors started their analyses from a frightening lack of knowledge of peasant practices and agro-ecologies. When e.g. Moore wrote '*Plowing is customarily too shallow, and without regard to the possibility of erosion*', he evidently had no notion at all of the methods of hill/mountain farming practiced in the greater part of the Yugoslav arable area (with e.g. terracing often standard practice, and steep hill sides left in forest or grass). And when writing '*fairly large areas of arable land are annually left in bare fallow that adds nothing to the productive value of the soil but at best simply postpones the day of exhaustion*' he mistook time-honored practices like the sustainable wheat-sheep rotation - with the sheep grazing the leguminous weeds surviving as seeds or as weeds in the grain crop - for predatory cultivation in the American way.

It is hard to explain why experts were not able to recognize the value of ruralities that had stood the test of time. But this lack of ability caused, within a very short time after the war, that the agricultural potential of Central/Eastern European countries was lost sight of. This potential is proved by pre-war exports listed above, but more still by the regional ruralities that had enabled their peasantries to live through extremely unfavorable times and political regimes.
Foremost among the adverse regimes was the Ottoman empire. Its harsh tax policies, especially towards the peasants, are the subject of many a traditional folk song, and together with its ruthless militarism were major reasons for the stagnating economies. E.g. most of mining and ore processing, though important before, came to a virtual stand still under the Ottomans (Wolff 1974 Ch.7.5). But also the concentration of large estates in the hands of a few land-owners, as in Roumania or Hungary under feudalism, frustrated the peasants' possibilities. Mitrany 1951 gave the example of Roumania around 1900, before the agricultural reform:

> '*while the peasants were crying out for land only some 40 percent of it was in cultivation…..properties over 250 acres, covering 49 percent of the total area, were in the hands of only 0.46 per cent of all owners; but while they possessed half of the usable land, these large owners had only one-tenth of the draft animals and less than one-tenth of the plows in use*'.

That quote clearly shows who were to blame for the agricultural stagnation.

But after the reform things did not really improve, certainly not at the government level. Mitrany shows this by quoting a Roumanian economist (quote from 1927):

> '*The situation which before the reform existed on the land, where a number of latifundiary owners retained the greater part of the agricultural revenue, has now been transferred to the domain of trade and industry*'.

Now there was an unbalanced growth of industry and trade, and again a privileged few enriched themselves at the expense of the peasant. But then, this same phenomenon is evident in the much heralded 'industrial growth' everywhere (the so-called 'scissors').
In the Balkans this was especially so because after independence the governments did hardly any better in terms of taxation than the Ottomans of old. As Aldcroft (1997 p.177/8) writes, here during the Interbellum

> '*some 50 percent of the total cash income of the peasantry went into taxes, an enormous burden considering their low incomes…. Yet while the peasants were taxed to the hilt, other more prosperous groups…escaped lightly*'.

Hardly a miracle that for centuries already those peasants had been busy to maximize their 'non monetary income', e.g. by tending the secondary resources that agro-forestry offered them. Maximization of housing was not possible, unfortunately: the taxes levied on houses depended on the number and size of rooms. Under such circumstances, peasants could hardly be expected to construct spacious dwellings.

In Western publications, the importance of the non-monetary economy (in rural regions) was recognized especially by those agriculturists and economists who had been involved with non-western cultures (as Boeke and Timmer in the Netherlands had been with Indonesia, and King and Furnival in the UK with India). Yet it was exactly those experts that got marginalized after the war, while the agriculturists and economists focusing on a monetary economy (or a plan-economy) took over government bureaucracies - and even academia. These 'modern' agriculturists and economists refused to take the non-monetary economy serious. They took one look at the primitive wooden or stone houses, often with one or two rooms and built with local materials, and concluded that these were signs of a backward culture.
This opinion was challenged only recently: probably Bideleux & Jeffries 1998 were the first to offer contrary evidence (especially with their '*Rival interpretations of peasant poverty*', l.c. p.446f.). And Wingfield (2004, review of Berend's trilogy on Central & Eastern Europe), explicitly rejected this '*old-fashioned paradigm*' of backwardness/modernity that for too long had dominated research, stressing that '*we should be wary of assuming that there is only one model for social and economic development*'.

Once we are able to step out of that 'old fashioned paradigm' its lack of substance becomes only too evident. To give an example: much ado has been made about the peasants' small parcels of land. Yet, whoever takes the time to walk through e.g. Zagorje or Samoborsko Gorje/Žumberak, (formerly) typical peasant regions not too far from Zagreb, can see that the high relief of the regions only allows for small fields. Other regions show such high relief too (about 2/3 of former Yugoslavia). And what is more: (s)he will observe how human hands in the course of centuries have carefully prepared plots & terraces, leaving steep hill sides either covered with forest or in grass. Far from backward, this was judicious agriculture, aiming at the conservation and development of the natural resources at hand - but evidently not fit for large-scale mechanization.
As mentioned before, the notion of 'overpopulation' is in doubt, if the availability of land is taken into account. Due to the attacks by the Ottomans, e.g. Croatian and Dalmatian peasants in past centuries moved to more safe agricultural regions in the hilly and mountainous terrain – which they as a rule developed in exemplary ways. Certain regions remained rather thinly populated for military reasons (e.g. the Krajina) – and became fully available for farming only after the danger had waned. This is an important reason why e.g. canalization of rivers and drainage of certain marshy regions in Croatia was started at such a late date. Due to such measures, so much fertile land became available that also then the growth of the peasant population caused no 'overpopulation' (cp. Hoffman 1975, who yet neglects the pre-war projects of which I learned from Andrej Martinov, Zagreb, who'd as an engineer taken part in them).

I mention these things explicitly because they were hardly noticed by the authors of the half-century after the war (from Moore 1944 and Conze 1953 to Aldcroft 1997 and Berend 1998). Though several of them have been perspicuous as to other subjects (certainly an author like

Berend in his trilogy), in their treatment of peasant practices, agro-ecologies, and ruralities, they ‘saw’ only what the ‘old fashioned paradigm’ allowed – and that was disappointingly little.

2.5. Vital agrarianism

If we allow the Balkan peasant to step out of oblivion, we will also be ready to understand the great importance of pre-war agrarianism in those (and bordering) countries. Up till now the only ‘acceptable’ alternative in East and West was industrialization/modernization. In its combination with post-war political upheavals, it wiped out the memory of the fact that, except Czechoslovakia, Eastern European countries had been agricultural economies first of all, with a rich variety of mostly small-scale agricultural systems and traditions. Those traditions found expression in a strong *agrarianism which formed a viable alternative to both communism and capitalism*. As late as 1942 (in London), representatives of peasant movements in Poland, Yugoslavia, Czechoslovakia, Hungary, Bulgaria, Rumania and Greece, issued a statement announcing policies & objectives. Below you will find a few passageds from this document (Bideleux & Jeffries 1998 p.455):

> *‘The strength of the peasantry depends on the strength of their common institutions as much as on their ownership of the land... The peasants themselves should control marketing, credit and the supply of agricultural equipment by their own institutions, democratically organized’.*

And as to cooperative organization, this

> *‘should be extended to factories for processing agricultural produce, to the markets of the products thus made, to village communities engaged in special types of production and to the promotion of agricultural education’.*

Before the war, East European peasant parties – then encompassing a significant part of the populace ánd being the most democratic parties of their countries – took good note of a.o. Sovjet policies. That was an important reason why they chose for the development of peasant agriculture and rural industries *in situ* - rather than transplanting millions of peasants to over-crowded and undercapitalized urban industrial sectors. As Bideleux & Jeffries (l.c. p.453) rightly comment about especially the post-war situation:

> *‘The lopsided emphasis on large-scale, capital-intensive, town-centered heavy industries and mining, favored by East European dictators, economic nationalists, communist parties, military interests and some influential Western development economists.... has proved to be a dirty and costly mistake for which Eastern Europe is still paying a high social, economic and environmental price’.*

But then, not only Eastern Europe, but a big part of the world is paying this same high price. And in regard to the West, its own inability to ‘see’ its own (small) farmers was at the root of the policies marginalizing them. The West marginalized its (small) farmers just as harshly and effectively as totalitarian policies did.

World War II was a rupture indeed. For a time, a significant part of Europe hardly existed for most policy makers on both sides of the divide, except in a military sense. Evidently not only general and agricultural economy and policy would have profited from a close examination of the recent history of rural Central and Eastern Europe. But as it was, both Western and Eastern European authors proved *unable to observe* what was there ‘right under their noses’. The cause was not this sudden division of Europe, for that would still have left room to study this recent past of Central & Eastern Europe. Yet, for decades, such attention had simply been ‘not done’, prevented as it was by the general adherence to High Modernity.

This 'tunnel vision', with its simplistic conceptual paradigm ('modern vs. backward'), prevented Western experts and policy makers from seeing anything else than that which was on the supposedly straight road to Modernity. On this road, the application of (large-scale) industrial methods everywhere is crucial, and development is propelled forward by techno-scientific research in central laboratories. Only agriculture actively shaped with the same kind of methods is considered 'modern', all the rest is 'backward'. It goes without saying that this model is not empirical, but highly ideological. As a 'yes/no' model, it is completely unfit for any real-life applications (those at the 'shop floor') This is especially true for ruralities. Agrarianism as a broad concept *focuses* at the (rural) 'shop floor'. The 'centralization paradigm', to the contrary, completely *ignores* it. Therefore, there is no doubt about the democratic importance of agrarianism in pre-war Central & Eastern Europe, and its complete neglect, in spite of the 1942 London statement, is stunning, to say the least. After all, in post-war years several authors explained the matter to their western audience (e.g. Serton-Watson 1950/56 p.13f., Bideleux & Jeffries 1998 452f., Berend 1998 p.287f.). Most importantly, in 1951 David Mitrany published an extremely valuable study that indeed attracted the attention of a few knowledgeable experts (cp. Fischer-Galati (ed) 1970), only to be forgotten subsequently. But first we will take a look at agrarianism's view of industry, to prevent misunderstandings.

> The peasant parties were the most democratic ones of the Balkan countries in the Interbellum, as many authors have demonstrated. Their programs proposed **an agrarianism that was not anti-industrial** but, as Mitrany writes
> '*any new industries had two conditions above all: to produce things needed by the mass of the people and, using native materials in the process, to give employment to as large a number of them as possible. For this reason, the peasant movement wanted industries to be scattered widely in smaller units across the land to give the peasants additional employment during the slack seasons. All these conditions pointed in the first place to the development of domestic manufactures and processing industries…*'. As to '*large-scale capital-goods industries in urban centers'* he writes:'*the accumulation of capital needed for such a type of industrial development would inevitably mean for …the peasants for many years s still lower standard of living, a standard that, as in Russia….could be imposed only by dictatorial methods*'. Conversely, only local development of industries could be of help to the economy at large, because it would link up with the reality of the small-scale farming employing the majority of the populace.

After World War I the governments of the Balkan countries continued to focus on grains, especially wheat (as they had done before the reforms). Then the Depression taught what could very well have been some crucial lessons. The first one is of central importance - in Mitrany's words:

> '*As the wheat market collapsed, the Eastern countries in fact kept afloat on the buoyancy of their peasant economies.…Subsistence farming could adapt itself to a precarious situation: the fall in prices led many peasants to eat what they produced, rather than take some of it to the market, and also to use their spare time in improving buildings and fences, as there was less of the additional work from which they ordinarily raised some ready money; some of the younger folk for the first time could even take off a month or so to attend the peasant schools organized in that period by the peasants themselves…..It was indeed a paradoxical state of affairs, unfathomable by modern economic theory, when, as one might put it, state and trade were bankrupt, but the mass of the people were better off*'.

But this, of course, was also dependent on peasants being able to invest in their natural resources in a non-monetary way, and on being able to utilize them in craft-like ways, without dependence on outside economic agents.

The lessons in agricultural economy are also very important. 'Big is better' had always been doubtful in agriculture, and this slogan definitely did not apply to the Balkan:

> '*The large owners…got better results not because of economic-technical but political and social conditions. Their farming was profitable on a large scale and with extensive methods only as long as it could command semi-servile conditions of labor; and even in cereal crops the well-established peasants in the Banat and in Transsylvania consistently got higher yields than any of the larger estates*'.

The pattern did not change after the war: both communist and capitalist 'large estates' were (& are, e.g. US) completely dependent on 'semi-servile conditions of labor'. Significantly,

> '*the quantities thus made available for export were never a true surplus*' because '*the change to large-scale farming…almost always…left those who did the work with an inadequate share of the produce*'.

It is important to realize that the fundamental concepts themselves in agricultural economic theory cannot be the same as those that got introduced in modern industrial economy, because the peasant's

> '*chief tool is the soil itself, or rather it is partly a tool, partly raw material, a unique combination in the whole scheme of production. It is unique in that it is both a variable factor, affected by each period of use, and at the same time a constant factor, which cannot be replaced. What the farmer can get out of it depends greatly on the state in which the soil was passed on to him by the previous user, and his own way of treating it will affect the results obtained by the next user. Quite apart from immediate benefits, therefore, the very nature and spirit of "cultivation" seem to require that the man who tills the land should have constant use of the same piece of the same instrument*'.

Consequently, concepts that are devised for big industrial economies are not fit for agriculture. The government that prescribes them anyway, makes itself unable to see what is there 'right under its nose'.

The many initiatives in the field of peasant cooperatives taken before World War II were opening up brand-new perspectives, not only regionally, but also internationally (in the 'Donau region'). This array of initiatives has been neglected, due to government policies giving priority to centralized research, followed by top-down implementation. Yet what all those who were active in this field (of cooperatives) had in common was the conviction that peasant initiative and freedom was central to any development worthy of the name:

> '*even those who believed that cooperative farming was an essential and urgent step, without even the exception of the several left-wing peasant groups, wholly rejected all idea of collective farming on the kolkhoz model, because it would mean central control and the loss of peasant initiative and freedom*'.

In the Karst mountain region of Dalmatia, and more especially in the Velebit mountains, pre-war cooperative initiatives paid special attention to the poor peasant. That demonstrates only too clearly that there were no urgent reasons to carry out the anti-peasant, post-war government industrialization policies that spelled the end of Crni Dabar (the example with which we started the present chapter) and many other rural communities. Of course these policies had their 'reasons', but they did not originate in the needs and possibilities of agriculture. The precedence of heavy industry had its roots in war and, together with the choice for technocracy, in power politics (80% of the 1952 federal budget was for arament industries and defense, Wolff 1974 p.332/3.

That Tito's post-war policies did not 'grow from Yugoslavia's soil' is clear by now (see also Bokovoy 1996). He suppressed the various peasant parties and jailed their representatives, e.g. the courageous left-wing peasant party representative Jovanović. Still the treatment of the country and its inhabitants by the Germans and its allies had been ruthless (see besides earlier references: Schonfeld 1976 and Steinsiek 2007). This made it obvious to Tito c.s., and to a substantial part of the population (for some years at least), that centralistic measures were inevitable.
In fact centralism & technocracy reigned supreme after the war in non-communist countries also. We will still take a closer look at the long-term consequences of the centralistic war-economies and their sequels, post-war centralistic technocracies. We will see that on both sides of the Iron Curtain these policies were implemented by governments losing view of the small farmer/peasant.

2.6. Eliminating the memory of peasant production

Why was it that in Yugoslavia, and everywhere else, governments became oblivious to peasant production & ruralities after World War II, in spite of the fact that in the1950s by far the bigger part of the populace in most countries still depended on small-scale agriculture?

Probing for an answer we soon realize that the extremely simplified pattern of politics and ideologies of those years – with its reduction of the multi-facetted public & economic life to the juxtaposition of 'communism' and 'capitalism' – hardly encouraged a conceptually rich approach to agriculture and to the economy at large. Quite to the contrary, the 'bourgeois' and the 'Marxist' nations, many of them new,

> '*largely accepted a linear, progressive view of history. …their main ideologies and views of history both reflected the Modernist orientation*' (Shin 1999 p.786).

This extreme 'ideological reductionism' is puzzling enough, taking into account that many, if not most, countries in pre-war decades had shown far more diversity of orientation within their own borders. This often included reflection on the disasters caused by capitalism (Depression) and communism (Stalin). Many countries had seen agrarianism grow as a conscious and multifaceted alternative (l.c.), and yet post-war years wiped out its memory. But then, these were the years in which the peasantry as a whole got disregarded, because governments and (their) experts were 'sure' that it was detrimental to the introduction of Modernity. Though this is puzzling enough, we know at the same time that history is littered with political regimes that were ready to discard any consideration of their peasants…

The attempt to obliterate the peasant had been 'powerful' enough already before the war, in Russia under Stalin. But Stalin c.s. were quite convinced of their case: remember that Soviet Russia's 'progressiveness' was squared with its ideological relationship with large-scale technology (Gestwa 2004). Small-scale agriculture simply did not fit into the picture. Yet that was the end of a road that, when it started with Marx, still could have ended up elsewhere. For as Buber (1950 S.137) has shown, Marx admitted that his book '*Das Kapital*' focussed on Western European society, whereas the Russian *mir,* the village community that for ages had carried the larger Russian society, offered possibilities of its own for a road to the 'classless society'. Yet any real 'commune' concept has two levels (l.c. S.162/3),

> *'und zwar so, dass die eigentlich vitalen Funktionen sich "unten", die allgemeinen Verwaltungsfunktionen sich "oben" vollziehen. ... Diese unabtrennbaren Bestandteile der Kommunen-idee hat Marx hingenommen, ohne sie mit seinem eigenen*

> *Zentralismus zu konfrontieren und zwischen beiden zu entscheiden.... Von den drei Modi des Denkens in Dingen des öffentlichen Lebens, dem ökonomischen, dem sozialen und dem politischen, hat Marx den ersten mit methodischer Meisterschaft beherrscht, dem dritten war er mit Leidenschaft ergeben, mit dem sozialen ist er – so absurd das auch in den Ohren eines bedingungslosen Marxisten klingen mag – nur selten in näheren Umgang getreten, und nie ist er für ihn bestimmend geworden'.*

Significantly Marx's continuing neglect of the social realm (in its own right) hindered him in studying Russian agriculture as embodied in the *mir* as a socio-cultural-ecological unity. Then his centralism grew stronger and led to

> '*die Konzeption eines absoluten Zentrums der Doktrin und der Aktion, von dem die allein gültigen Thesen und die allein massgebenden Befehle ausgehen*' (Buber l.c. S.169/170).

For Lenin the big, centrally directed factory was thé social model:

> '*Die ganze Gesellschaft wird ein Büro und eine Fabrik mit gleicher Arbeit und gleichem Lohn*' (id. S.177).

The results soon became apparent, when shortly before his death an embittered Lenin had to admit: '*Wir sind ein bürokratisches Utopia geworden*' (l.c. S.179).

Yet it was Lenin himself who had rejected vehemently the result of Sergei Bulgakow's 1900 pioneering study on the comparison of agriculture and industry in a great many countries, with its conclusion of essential differences (for some aspects of the as yet insufficiently researched confrontation see Wielenga 1971, Evtuhov 1997). The vehemence of Lenin's rejection ensued from the total dependance of his concepts and centralism on the big factory model. Only the apparent man-made character of the factory could 'guarantee' any free building of a new society, not hindered by any 'essentials' as to men or nature. In the end, the theory of '*power through the factory/big project*' created this curious conceptual similarity between Lenin c.s. and big capitalists that would show up time and again during the 20th century (Scott 1997).

Lenin displayed his deep-seated distrust of the Russian peasants and the *mir*. For him agriculture needed a complete switch to large-scale production. In Lenin's words (cit. p.14/15 of Hunter & Szyrmer 1992; cp. Lenin-anthology 1974):

> *'Anyone who has carefully observed life in the countryside... knows that we have not...undermined the foundation, the basis of the internal enemy. The latter depends on small-scale production, and there is only one way of undermining it, namely, to place the economy of the country, including agriculture, on a new technical basis of modern large-scale production'.*

Before long the peasants and their *mir* were treated as enemies. and the *mir*-based agriculture was replaced by a process that was the analogon of the factory process, the work of engineers. Remember in this connection that no one less than Chrustchev was such an engineer - one of the generation originating in the 30s that prided itself on building a new society and a new agriculture (Schattenberg 2004, Gestwa 2005; see Schattenberg 2000 for a balanced comparison of old and new Russian engineers).

Being ardent technology-believers themselves, they fell in line with Lenin c.s. in their rejection of – enmity towards - peasants and *mir*. They wholeheartedly participated in the policies aiming at erasing the memories of the peasant and *mir*.

> Nowikow 1906, about the Russian village, writes with the spite and scorn that many, if not most, Russian intellectuals had towards the ***mir***. There is nothing of the sympathy that we meet with Mackenzie Wallace 1905, in Ch.VIII of his two-volume work on Russia (note that he is not uncritical). Quite different from Lev Tolstoi (with his Jasnaya Polyana), or from the philosopher Berdjajew (cp. his autobiography, Ch.VII), Nowikow c.s. have no sympathy with the Russian peasant. Bulgakow 1909/1999 is revealing about this negative attitude of most Russian intellectuals in those years (refer to Zernov 1963 for some more information). These intellectuals, in their way, were instrumental in the destruction of the Russian peasant and the *mir* - only to be devoured by the Stalinist Moloch in turn.

First the experts who were familiar with the peasant's practices (and their specific rationality) were marginalized by Stalin and subsequently persecuted (Solomon 1978/84). Among them the agricultural economists of the Chaianov line, the Organization-Production school, figured prominently (Nove 1990). *With their persecution the best-informed specialists of the age on peasant economies were silenced.* Other experts conversant with the peasant communities and their practices, many soil scientists among them, were silenced as well.
As to soil science, note that the progress of science in imperial Russia had been closely connected with Russia's small-scale agriculture (e.g. Hachten 2002). Indeed, soil science as a discipline had exactly this *Russian* background, and soon became widely known for it (e.g. Buber 1910). But, contrary to the attitude in late-imperial Russia, professional autonomy was not acceptable to Stalinism (Joravsky 1984).

After these experts, the elements with some sympathy for the peasants got purged from local party and Soviet cadres (Conquest 1968 p.20). Next the peasants themselves received a very harsh treatment, which became widely known in the West early on (Brutzkus 1932).

> **Eliminating peasantry and the mir**: direct accounts are very moving – Mandelstam 1972/ 76 Ch.29.III, Kopelew 1981, Sokolov 1990. As to the subject as a whole see contributions to Osteuropa 2004 no.12. Thorough studies – e.g. those reviewed in Nove 1993 – estimate no less than 7 million dead (8,7 million in Rosefielde 1996), chiefly because of the famine among the peasants, which was caused deliberately by the regime (Lewin 1974).

The legal prohibition of the *mir* - the self-reliant and communal peasant village – in 1930 was a reaction of the regime to its revival during and after the Revolution. It had been the *mir* that had carried out the confiscation and redistribution of the estates of the landed gentry. There were some 350.000 of these 'village societies' that embodied the real socio-agricultural life of Russia before collectivization (Altrichter 1979):

> *'During the NEP period [the greater part of the 20s] it was an active village institution alongside which the selsovet [the local Soviet creation] was something of a Cinderella'* (Lewin 1968 p.26).

Its prolongation was definitely against Stalin's totalitarian aims, but there was more to it. Stalin like Lenin before him was convinced that agriculture lent itself to an industrial approach (Hedlund 1984 Intr.). He despised the peasants for their traditional approaches that, he felt, were just slowing down progress. So the ruthless 'dekulakization' was 'redeemed' with the supposition that it was to open up vast industrial horizons for agriculture and food production:

> ‘*Farms were regarded as rural factories, and planners attempted to control them in much the same way that industrial plants were controlled. Soviet agricultural history is to a large extent about different attempts at finding the best way of exercizing this control, without questioning its existence*’
> (Hedlund 1984 p.22).

(That, of course, is exactly the same perspective that soon was at the heart of the US Agricultural Development Council, etcetera).
This perspective can only be considered meaningful if all that is nót at ‘the centre’ is deemed non-essential. And so we see:

> *The prohibition of the mir, with its own family & ecology dynamics of regulation of the local agricultural economy (Pallot 1982), meant an express denial of the importance of household & ecology in agricultural production.*

It stands to reason (…) that the body of experts who had studied this system of peasant farming as a *sustainable* system (Chaianov c.s.) had to be purged.

Already before the Revolution there had been other experts who, starting from standard modernization notions, had attacked the *mir*. E.g., the 1906 imperial edict giving Stolypin the free hand in modernizing agriculture by disengaging peasant households from the *mir*, started from western-liberal assumptions, not from a close study of the *mir*. The near-complete failure of this plan then led Stolypin c.s. not to some thorough evaluation of their own approach, but to an indictment of stubborn ‘backwardness’ of the traditional peasant (Pallot 2000). So Lenin and Stalin had some precursors in their indiction of the peasants of the *mir* as ‘backward’ and ‘opposed to modernization’. Evidently, already before the Revolution there had been experts who, basing themselves on standard modernization notions, had attacked the *mir*.

> The legal prohibition of the *mir* and the starving of the peasants were preceded and followed by a myriad of other measures **annihilating rural communities and culture** – Conquest 1968 Ch.1, Baberowski 1998; for documents Cummins (ed) 2000. A central role was played by the OGPU (the political police), while operations at the local level were led by the *troikas*, three-men commissions consisting of the First Secretary of the Party Committee, the chairman of the Soviet executive committee, and an OGPU-representative. They were ultimately responsible for the murder of some 90%, at least, of village priests in ‘37/’38 - who till then had survived as a last token of community identity (Binner & Junge 2004). But then, the elimination of church & pope had been purpose of the ‘collectivization’ campaign from the very start (Maeder 2000). 1940 saw fewer than a few hundred churches still open from the 54.000 ones registered in 1914.

The annihilation of the *mir* originated from a rejection of this Russian type of communal and family farming, that included a rejection of its local resources (incl. its interactions with the local ecology). The disastrous results of these policies were eventually admitted, be it very partially, after Stalin’s death (Broekmeyer 1983 Ch.IV). *But note that many of its assumptions and practices, especially those related to agriculture’s ‘industrialization’, were those of post-war Western governments too!*
As e.g. Scott (1997) has analysed: with their centralization of knowledge and power, and the accompanying denial of the knowledge and practices at the ‘shop floor’, these assumptions are common to the whole of the western ‘modernization’ effort. In fact the specific combination of *technology and power* that we meet here is not only part & parcel of the ‘modernization’ efforts of liberalism and of communism, but of Nazism as well (Aspaturian 1974).

Altogether, Stalin's ruthless policies initiated an immense urban migration (Hoffman 1994, Rosenberg & Siegelbaum (eds) 1993), and virtual slavery was imposed on most of the remaining rural inhabitants (cp. Arseniew 1966 Kap.IX.2 for a thorough treatment). Descriptions of the post-war system that followed (e.g. Broekmeyer 1983 and 1995/96) leave no doubt about the sad failure of this transformation of Russian peasant life and economy. Resistance to the government measures - for which Stalin mobilized primarily young, male workers as shock-troopers - was especially strong on the part of the peasant women (Siegelbaum & Suny 1993; Farnsworth & Viola (eds) 1992). It was they who most strongly sensed the onslaught on the organic character of family life and peasant farming. With bitter irony, the produce of their remaining small garden plots would provide Russia's populace with a number of essential foods during the next decades (Dodge & Feshbach 1967, Broekmeyer 1983 Ch.IV).

When Chrustchev in the 50s followed the US example of the large-scale cultivation of hybrid corn (Anderson 1967), this was an effort to prove the superiority of communism, and a last-ditch effort to defend his own lifetime investment in a cruel system. His Virgin Land program (McCauley 1976) had to offer late evidence that the havoc of the 30s (and later) could still lead to success. Chrustchev's great efforts in the field of agriculture indicate the depth of his involvement (Broekmeyer 1983, Crankshaw (comm.) 1973). Their failure shattered his last hopes and led to his departure from politics (Medvedev & Medvedev 1976, Filtzer 1993).

This could have been the moment of introspection for both the USSR and the US, as to their agricultural policies. Nothing of the kind ensued. The country that in the first decades of the 20th century had made the biggest advances in agricultural economy sidelined both the experts concerned – Chaianov c.s. – and the peasant & his farming. The rest of the world, though noting the failure, was in the grip of the same technocratic ideology and unable & unwilling to start a re-evaluation.

2.7. The West consolidating the elimination

There is no exaggeration in using the phrase 'biggest advances in agricultural economics', for up to 1930 peasant farming had been the norm globally, and compared with it other ways of farming had been far les significant (numerically and otherwise). Economists and agri-culturists conversant with agriculture in the Eastern Hemisphere in pre-war years (e.g. Boeke, Timmer, and Furnival), recognized the peasant economies in their own right and were well acquainted with Chaianov's theories.
But after the war, with Boeke c.s. soon marginalized and most experts from the Chaianov school no longer alive, peasant economy as a discipline was faced with oblivion: main-line economists in the US and elsewhere did not even know about it. Contributing to this ignorance was, that outside Russia Chaianov c.s. had been best known in the specialist literature published in German. But the Third Reich had nothing but contempt for peasant farming and turned down any ongoing research in it. Next, the literature that still was extant after the war was ignored by the American translation program for scientific literature. As a result one will look in vain for a treatment of the Chaianov school approach even in the works of a comparatively erudite author like Theodore Schultz, the best known of the American agricultural economists.
But then, the English agricultural economist Currie was apparently still less informed than his American colleague Schultz: he did not pay any attention to the farmer's family, community, and ecology in his 1942 publication. Given that he was a prominent 'expert', what could be expected of the others?

And indeed, Warriner's 1939/1964 '*Economics of peasant farming*', which focuses on Eastern Europe, not even mentions either Chaianov or the *mir*. But then, she leaves out *any* anthropological description worthy of the name. In the same vein Clark & Haswell's 1964 '*The economics of subsistence farming*', that included Russia in its focus, does not mention either Chaianov or the *mir*. Also those authors miss out on ethnography/ anthropology.

The antipathy of Western economists: One root of this troublesome attitude was the antipathy displayed by many Western economists towards the post-World War I agrarian reform policies in the Balkan countries (and elsewhere in Eastern Europe). Keynes (ed) 1922 on '*Reconstruction in Europe*' was the most widely disseminated publication of those years on the subject. Section VI, Part III on '*The peasant revolution in Eastern Europe*' is revealing. Economists like Namier and Sering are not able to find any good in the reforms – even though the volume as a whole contains several contributions from the Balkan countries themselves that draw quite another picture (e.g. Jonescu-Sisesti 1922a who gives real figures on Rumania, and Prohaska 1922 on Yugoslavia). As to the background and thrust of the reform policies, Jonesci-Sisesti 1922b is quite accurate, and Namier not at all. Cp. Roger 1926 for a short account fully vindicating Jonesci-Sisesti 1922.

As to post-war US, e.g. Nash's (1966) '*Primitive and peasant economic systems*' purports to give an anthropologically based overview, but it remains very superficial. Its complete sub-servience to the concept of 'modernization' (e.g. Ch.7) prevents any really *sympathetic* description of 'peasants' or their economy.
Similarly Dalton's (ed, 1967) '*Tribal and peasant economies. Readings in economic anthropology*' does not live up to its title. Its Pt.I '*General views*' is dominated by the 'traditional-modern' axis and oblivious to wider approaches.
And Potter, Diaz & Foster's (1967) '*Peasant society: a Reader*' once more shows a lack of *sympathetic* research. The editors just mention that '*we have not utilized the voluminous writings of historians and economists who have studied European peasantry*' (p.V), proving that they did not take the effort to digest European approaches & methods.
In short: these experts display **great ignorance of peasant economies**. Unfamiliar with the peasants' expertise of land, vegetation, and ecology, they cannot make sense of his agri-cultural practices, either in the context of the family or the village community. The few authors who display more knowledge and understanding are not part of the circuit then influencing agricultural policy and economics (e.g. Arensberg & Niehoff 1964 with their '*Introducing social change. A manual for Americans overseas*').

Only in Joosep Nõu's massive 1967 '*The development of agricultural economics in Europe*' the names of Chayanov c.s. emerged again. Nõu was a multi-lingual author, conversant with much of the original literature published in a.o. German, Russian and English. Yet his book was published only áfter agricultural economy and policy had been dressed up completely in High Modernist garments. Also the mid-1960s re-discovery of Chayanov by a small circle of specialists in the Anglo-American world was without any influence on the 'modern policies', that started from the common opinion that nothing could be learnt from the peasants. So for decades even the modest re-discovery of Chayanov etc. was of no avail. 'Tunnel vision' in terms of modernization was evident, seeing that e.g. anthropologically and historically solid economic research, like that from the school of Karl Polanyi, was sometimes mentioned, but nowhere digested or used.

We arrive at a tentative and two-fold conclusion.

1. Post-war (agricultural) policy in e.g. the US and Europe completely ignored role of the small farmer (or peasant) even in their own country.

The policy makers did not understand the practices of the peasant, in the context of his village community, because they were completely ignorant of his expertise in terms of the land, vegetation, and ecology. Impatient to stimulate ‘progress’, they were ready to relegate ‘traditional agriculture’ to the dustbin of history.

2. ‘Experts’ tried to find excuses for the criminal treatment of the peasants under Stalin’s regime. They called it an ‘unfortunate decision’ of the Bolsheviks to bring about too much progress at once (e.g. Warriner). In that way they erased the victims and their way of life from memory. Similarly, by banning authors who, like Boeke with his ‘*The voiceless Far East*’, defended the peasant’s communal-agro-ecological existence in its own right, the policy makers were left with approaches starting from the ‘modernizing’ point of view - approaches starting from the assumption that the peasant was doomed to disappear.

The best one can say of the ensuing post-war agricultural economic policies is that they were built on extreme ignorance. Having lived through disastrous decades, the new experts as well as the policy makers made the peculiar choice ‘not to look backward’, but to ‘look forward’ only, and they discarded the foundation of agricultural policy in the peasant’s millennia of experience. Even the pre-war decades, in which a range of peasant societies - those of Eastern & Southern Europe among them – had indicated ways to a vital agrarianism, were thus ‘forgotten’.

Wars and revolutions always cause disruption of personal and communal existence. So in itself it is quite likely that especially World War II, the biggest war ever, led to grave consequences also for agriculture and agricultural policies. We simply have no reason at all to assume that a gradual evolution took place, leading to progress. Quite to the contrary, a substantial investigation of real history and practice is indicated. Now the ‘substance’ of agriculture is first of all: the soil. So it stands to reason we now turn attention there.

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3.

Regaining perspectives

that High Modernism made us loose

A shrunken world,

and the way out

As hinted at before, High Modernism as a government project achieved many of its aims in a virtual way: by changing language and concepts. To give a convincing example: the N-fertilizer industry, providing ingredients for the explosives industry, received a boost from the war. Greatly strengthened also in its relations with government, its managers and policy makers deemed it destined to enhance agriculture's productivity. Then from its position of power it replaced 'fertility of soil' by 'fertilizer supply'. This became even easier, because in the post-war years of restricted budgets it was one of the few agents able to finance research.

Yet as stressed before, simple power would certainly not have sufficed, if the government, industry and many farmers had not shared the same High Modernist faith in the constructability of nature (and society). For 'fertilizer' seemed to bring the very power that all were looking for. And of course, its role squared with the government's conviction that it had to increase central regulation. A conviction that itself had deep roots in the war.

When we replaced 'soil fertility' by 'fertilizer supply', it was a change in language, not of nature. That is quite fortunate: the simple world that got constructed with the new language seemed perfectly legible, but it soon left little freedom. Fertilizer concentration now being the only thing that could be varied, we hardly had any freedom left to face the array of problems that presented themselves. But, we could at least become curious again about the world outside this self-constructed cage (in which industrial fertilizer was 'all there was'). We will take a look at some broad aspects of the construction of the cage, and at the way out of it. Then in some of the following chapters we will subject several of the scientific issues to a closer examination.

3.1 Peasant wisdom

Some thirty years ago I observed a scene that has stuck like a photograph in my mind. I was taking a walk in Samoborsko Gorje, then still a peasant region in the steep hills some thirty kilometers west of Zagreb. Approaching the village of Otruševac from the direction of Samobor/Vrhovčak, I caught sight of a peasant family harvesting the grain in a triangular field, somewhat lower than the road. In the field the grain clearly was interspersed with weeds, yet not overgrown by them. Separation of weeds and grain was done by hand, when the grain was harvested. As I learned later, the weeds often also had a function for those peasants (use as feeds etc.- Vieyra-Odilon & Vibrans 2001). Often in Southeastern Europe they could hardly be called 'weeds', as when they were part of a 'sheep-wheat' rotation, more generally a 'feeds-grains' rotation, in which especially leguminous 'weeds' grew from dormant seeds during the fallow year following the grains, restoring carbon and nitrogen to the soil. During the fallow year cattle grazed on the weeds, and when some reappeared among the grain, after the harvest it was used as cattle fodder. Which demonstrates that 'weed' is a relative concept indeed.

Weeds? '*It is concluded that species that become noxious agricultural weeds are those that adapt to fertilization and develop resistance to herbicides, and in general do not thrive in natural ecosystems*' (Odom, Park & Hutcheson 1994, Summ.). Many weed problems are 'made in modern agriculture'. In low input, low-tillage systems seed-predators are able to considerably reduce broadleaf weeds and their competitive ability (Brust 1994) and, quite different from industrial inputs, organic amendments '*mitigate heterotrophic weed infestations*' (Sauerborn et al. 2003). Biological weed control is perfectly possible (van Driesch & Bellow Jr.; Upadhyay et al. (eds) 2000), but high-input systems thwart its application. The overriding importance of biological factors was noticed also by main-line research, e.g. Doyle et al. (2001 p.84) writing '*There has been a tendency to develop weed management strategies to achieve economic goals without linking the strategies to biological factors and without investigating how different factors interact*'. Yet main-line weed research manifests half a century of 'addiction' to industrial inputs, looking for 'sustainable' solutions **within** its fertilizer & herbicide paradigm and apparently not able to question this paradigm itself.
This paradigm dominates its **models development** too because
(**a**) it is a great exception when it includes the industrial fertilizer regime in its models
(**b**) the real soil with its unknown complexity is nowhere in the models, instead it gets 'shrunk' to the inert substrate-plus-industrial nutrient concept that 'guided' industrial agriculture from its inception (**c**) where modellers try to include plant and seed physiology, the quantities required are rarely available so that 'curve fitting' prevails (instead of model valuation). All in, the danger looms large that weed science chooses '*models where the uncertainties in the inputs must be suppressed lest the outputs become indeterminate*' and so becomes 'GiGo science' ('Garbage in Garbage out', Ravetz 1990 – see §1.14)

The fields in Samoborsko Gorje – usually in this hilly region – received stable manure (mixed farming was still common). So they neither had the infertility attracting 'thorns and thistles', nor the high mineral nitrogen that kills so many useful leguminous weeds and stimulates really obnoxious ones. As a rule weed pressure in those fields was limited, with the weeds yet appearing mostly having some use for the peasant. As long as hand harvesting was prolonged of course, for only then selection of the different 'weeds' by the experienced farmer is feasible.

For this non-mechanical harvesting requires expertise of a local and ecological character that is not available with the energy-intensive mechanical reaper. The reaper embodies expertise from institutes & industries far away. Because it substitutes mechanical operations for people, it is blind for the multifaceted diversity of the local field, and is fit only for mono-cropping in a simplified landscape. Note that its design is not 'general' in character, but mirrors the greatly simplified world of the far-away expert. It is completely ignorant of the prior shaping of field and wider landscape by the peasant and his ancestors and wider community, even when those labors were essential for a sustainable local agriculture (hedgerows, terraces, etc).

Within traditional agriculture weeds are a partial asset (e.g. Duke 1992) – an often important part of the knowledge-intensive local resources. Vandana Shiva (Shiva 1993 p.25/26) gives the example of *bathua*:

‘*an important leafy vegetable, with a very high nutritive value and rich in vitamin A, which grows as an associate of wheat. However, with intensive chemical fertilizer bathua becomes a major competitor of wheat and has been declared a ‘weed’ that is killed with herbicides. Forty thousand children in India go blind each year for lack of vitamin A, and hrbicides contribute to this tragedy by destroying the freely available sources of vitamin A*’.

The example puts similar discussions about ‘yellow rice’ in a clear perspective!
As to the wider scope of ‘weeds’ Shiva (l.c.) continues:

‘*Thousands of rural women who make their living by basket- and mat-making, with wild reeds and grasses, are also losing their livelihoods because the increased use of herbicide is killing the reeds and the grasses. The introduction of herbicide-resistant crops will increase herbicide use and thus increase the damage to economically and ecologically useful plant species. Herbicide resistance also excludes the possibility of rotational and mixed-cropping, which are essential for a sustainable and ecologically balanced agriculture, since the other crops would be destroyed by the herbicide*’.

These examples indicate that herbicides cut off access to and use of highly valuable natural resources and as such cannot be part of a sustainable agriculture package. Yet weeds as an asset do depend on the framework in which they appear: that of local biodiversity and of knowledge of health foods/feeds and of medicinal herbs. There is an overlap here with the traditional home garden with its household-centred specialties (fragrant and medicinal herbs among them).

It will be clear by now that ‘**peasantry’** is a concept that has at its core the relative self-sufficiency of peasant farming, as deriving from knowledgeable and socio-culturally embedded use of local resources, of soil and vegetation first of all. For present purposes Kearney’s (1996 p.61) opposition of ‘farmer’ and ‘peasant’ will do: ‘*The farmer also produces value from the land, but whereas the farmer produces exchange value, the peasant primarily produces use value. This self-provisioning by means of cultivation is the rock-bottom defining characteristic that distinguishes the peasant from other rural types*’.
If in what follows I use the indication ‘*small farmer*’, I expressly mean to indicate *someone who got involved by outside pressures in some market network that makes him ‘commodify’ at least part of his production processes & produce and that at the same time reduces his access to his potential local resources that would allow him an ongoing relative self-sufficiency.*
After these digressions we can return to the home garden.

Within traditional agriculture the home garden is mostly the domain of women. In other important ‘domains of care’, women also figure prominently, in e.g. hand milking and in tending the small flock of poultry. Their superior care and tenderness is a solid foundation, not only for the wellbeing of animals and garden plants, but also for the production of saleable surplusses.
Furthermore women’s knowledgeable participation in the farm-economy is an important part of its flexibility in times of change (in good times as well as bad). As such women’s participation is essential to the remarkable adaptability of traditional agricultures that earned them Joan Thirsk’s definition as ‘*alternative agricultures*’ (Thirsk 1997).
Care, as well as locally integrated expertise, are essential elements within (resources for) traditional agriculture. Because they have an integral and life-promoting character they are akin to wisdom (*vide the title of this paragraph*). But where the focus changes to ‘power from a distant centre’ – the expert centre servicing government and industry – the distant power approach is at cross-purposes with proximity and care. That is an immense *agricultural* loss above all. The common depiction of the post-war change-over to large-scale, energy- and chemicals-intensive operations as unqualified ‘progress’ is silent about this loss of essentials.

But then, note also how the concept of *fertility* was redefined in those years into external-inputs-based *productivity*. Agriculture was no longer regarded as dealing with the ‘fruit of the earth’, with crops growing from the earth’s ‘womb’, but with productivity resulting from an injection with industrial fertilizer. The language and concepts of agriculture, as used by the government and its experts, acquired a curious *masculine* character, in a sense not befitting traditional peasants/small farmers, either in Europe or elsewhere. Something of what was lost, and needs to be regained, is expressed in the following fragment of an inter-view with two lady farmers in Schönberg (Germany) (Bernholdt-Thomsen & Mies 1999 p.78):

> *(L.) I feel, this relationship to the animals is the first relationship that needs to change. An animal is not a thing, it is life. And the same is true for the earth.*
> *(M.) And that requires a caring and nurturing relationship?*
> *(A.) Not only caring and nurturing but a loving relationship. I remember my father in my childhood, how he used to go over our fields. I can still see him before my eyes – I don’t know how to put it... If I was to describe how God would go over the fields, that was my father. Almost like that, when he went across the fields or followed the plough. How can I put it? Sometimes you see very old pictures, sometimes you see that man behind the plough. There is no hint of agressiveness, there’s a loving attitude.*
> *(M.) Correct. I remember my father in the same way’.*

The change-over to ‘industrial agriculture’ was a break with ‘traditional’ agriculture at a deeper level than that of the introduction of machines and other industrial inputs. It was *a break in language, in expertise, in caring relationships*. Such a total break calls for some thorough, historic explanations. An evolutionary representation of events (with the present automatically ‘the best’) will not do, even if it has been the trend up till now.

In the meantime, the army of (young) experts that appeared on the scene in the first decades after the war were educated *in another language* than the language of care. The hesitant start at government level - in pre-war years in the US and in the Netherlands – with an exploration of the actual life & practices of the small farmer got cut short by the war (we will take a close look at it in later chapters). The enlightened officials who after that introduced the institutionalization of the new agricultural research and policies were certain that the solutions to agriculture’s problems were all of the centrally conceived kind: fertilizer, pesticides, mechanization.
And so we see the new army of experts after the war, who knew all about the centrally devised solutions, but were strangers to soil/ecology- and farmer/community-based agriculture. The new experts hardly had a clue that soil fertility ultimately depends on .. the tender care for soil & soil life. Perusing the voluminous collective volume (and review) of these experts’ labours in the post war decades, Bunting (ed) 1970, we hardly ever read about either the peasant/farmer or the soil & ecology. There is not a trace of doubt in those contributions that those ‘locals’ can be conveniently left out and that the world is waiting for guidelines from the expert in the central research institute (in line with government policy).

Technocracy was rampant everywhere after the war – we will repeatedly look at that historical phenomenon – so the accelerated growth of an agricultural expert system disjunct from real life (of local farmer & ecology) was in a way ‘nothing special’. But it goes without saying that the ensuing system will be plagued by the same central problem as technocracy at large: people and nature cannot be manipulated at will, and that will show up at some point. But that, of course, clashes with the convenient projections of the expert and government official. The uncontroleable situation caused by the supposition that all is under control is well designated by Sikorski (1993 p.165/7/9, 175 – note he changes ‘technocracy’ into ‘techno(an)archy’):

> ‘*Mastery is the truth of Technoarchy, distant power the measure of its truth. Knowing all things as Man’s utility and means to more power, Technoarchy conceals all other ways of being, shutting itself off from the mystery of the earth, imposing its rationality on everything, insisting on the appropriateness of the technocratic utopias it builds. In doing this, it produces contingency as its shadow, its other, and makes itself radically vulnerable to its eruption. Cutting itself off from the earth, repressing it, and dismissing it, it makes its utopian dreams of reason contingent on the earth’s erupting only in the ways it has planned out for it’*.
> *‘Radically unlike Technoarchy’s machine-centered economies, nature’s economies have room, even a necessity, for slack, ambiguity, and play, for anarchy and chaos’.*
> *‘Unlike nature’s economies, which, lacking a centre, are more stable the more complex they are, Technoarchy’s economies, requiring submission to the centre, become more unstable and more difficult to manage as they become more complex’.*
> *‘As the specialist posits variables and manages their relationships, building his utopia, he closes himself off from the earth and conceals from himself the world’s real complexity. Seeking total control within the simple enclosure of analytical procedure, the specialist abandons everything that escapes his system of definitions, leaving it totally out of control…*’.

That technocracy’s economies are the more instable the more complex they become, is well established (e.g. Perrow 1984). The destruction caused by constructing such economies is extensively documented (e.g. Josephson 2000). Conversely, that the stability of the complex *economies of nature* is dependent on diversity, ambiguity, redundancy, play, and ‘chaos’ (cp. the physical concept), is also no more in doubt, in regard to e.g. agroecosystems (Giampietro 2004). So some questions arise: (1) What ‘nature’ did post-war technocrats in fact envision? (2) What historical circumstances made them ‘rise to power’ politically? There is no good reason to take post-war technocratic developments at face value - especially not in the realm of agriculture. Instead we will have to take a close look at the real course of their history.

Intermezzo: science & technology, image versus everyday reality

But note that a central dogma of High Modernity is the *progressive character* of science & technology. This dogma made lazy researchers, experts and politicians. Conveniently suggesting that ‘last is best’, it discourages reconsideration of ‘established’ methods and results - especially when these have been implemented in official regulations and established projects. This laziness squares with e.g. the pre-war science philosophy of Kohnstamm, in which such re-evaluation of methods and results is a never-ending process (Kohnstamm 1926 p.173). Kohnstamm knew already that ‘progress’ is a concept with many faces. In regard to S & T, we consider three of them.

1. Most industry managers and researchers interpret the successful sales of an industrial product as progress, and government and its experts are inclined to follow them. Leaded petrol, asbestos, and bottle feeding were once widely hailed as progress, and maintained a ‘recognized’ position for many decades, in spite of clear indications of their detrimental influence (Cp. Lindrigan 2002).

2.The second face of progress is a more stylized one. It shows up when, in retrospect, a few ‘facts’ in the past are chosen as precursors of a (techno)scientific development. From its present size, we are then sure that it has improved impressively. To give a simple example:

progress is evident if we measure agricultural progress by tractor sales, neglecting both prior animal traction and the specific character of local agriculture. Because such an approach hand-picks some 'facts' and is ignorant about all the rest, it leaves out most of what was relevant. The resulting picture is way off the mark in a historical sense (cp. Feyerabend 1978a). E.g. herbicides and industrial fertilizer constitute progress only if we are silent about the great losses they inflict upon peasant agriculture (e.g. losses of leguminous 'weeds'). Likewise, industrialization on a grand scale is progress only, if we are silent about the displacement of the production of the artisan and of small-scale handicraft industry (e.g. Stalinist strangling of kustarnyi industry, Pethybridge 1974 p.228f.).

3. The third face has its roots in education. There is no doubt that the way in which most people interpret progress in the field of S & T is based on the way the relevant material was presented to them in the textbooks they studied in college (Funtowicz & Ravetz 1997 p.802). More often than not the S & T these textbooks (and countless other sources) present, clashes with everyday S & T. S & T is always contingent, and very human indeed, because they are part of all the upheavals and dead ends that humans experience, both personally and as a member of their community.

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Bibliographic note on **everyday science & technology:** For the 19th century see e.g. Michael Faraday (cp. Gooding & James (eds) 1985), early 20th century Kohnstamm (1908, 1916, 1921, and esp. 1926). Also Fleck 1935 (repr. 1979) is a classic.
After World War II publications by the renowned physical chemist–turned-philosopher Michael Polanyi were ignored by the dominant 'science philosophy' of the day, that then met its rebuttal with: Kuhn 1977a, MacIntyre 1977, Feyerabend 1978. Some literature: Polanyi 1958, Authors Coll.1961, Polanyi/Grene 1969, Polanyi & Prosch 1975. A widely known pupil of Polanyi is Jerome Ravetz (1971, 1981, 1990).
Mainstream's positivism notwithstanding, eminent science historians like Hooykaas (1940, 1947, 1959, 1961, 1970, 1971), just like Kuhn (1962/1969, 1977, 1987) and the historian-philosopher Feyerabend (e.g. 1978) did much to acquaint us with the human, historic endeavour of science. It does not resemble the stylized versions.
Then from the 70s on a number of sociologists started studying everyday S & T, and delivered us much valuable material - e.g. Latour & Woolgar 1979, Knorr-Cetina 1981, 1988, 1994, 2003, Barnes & Edge 1982, Lynch 1985, 1991, Lynch & Woolgar (eds) 1990, Star 1989, Collins & Pinch 1993, Pickering 1995.

3.2. High Modernity

In pre-war years large-scale government projects in some countries had become very visible already: Stalin's Don Basin projects, the US' TVA-project as an example of its ever increasing scale of irrigation- and river/dam-projects (Huxley 1943), the Netherlands' Zuiderzee-projects. Especially the TVA project had been celebrated as unmitigated progress. Even in those pre-war years the destructive sides of some such projects had become visible, esp. in the US and in the USSR. But most of the rural landscapes had been left untouched, and phantasies could still roam unfettered.

Then World War II introduced the enormous, big-industry based, productivity of the US on the world scene. The 'productivity gospel' next became a chief 'export product' of the US (e.g. Tiratsoo & Tomlinson 1997). This was facilitated by the fact that all nations aimed to solve their problems as soon as possible. All experts started dreaming of employing this 'industrial power', that they witnessed for themselves in the US.
The serious pre-war US disasters in regard to soil and agriculture had been described rather well, e.g. in '*The rape of the earth*' (Jacks & White 1939) and in '*The life and death of the land*' (Whitaker 1947). As a result, after the war there was some restraint, stemming from soil conservation, to let agricuture's 'industrialization' accelerate (e.g. Bennett 1950). But when the Cold War set in motion a competition between the big powers (focussing on big industry and large-scale projects,van Popta 1971), nature was once more considered clay in man's hands.

Symptomatic of those years are books with titles like: '*Ingenieure bauen die Welt. Erdumfassende natürliche Raumplanung*' (Krüger 1955). A quote illustrates the frame of mind of those decades:

> '*Die Notzustände während und nach dem zweiten Weltkrieg haben die Einsicht und den Aufbauwillen der Finanzgrössen und Staatsmänner vertieft, dass ein wirklicher Wirtschafts-aufbau vor allem in den unterentwickelten Ländern in einem vormals als gigantisch und absurd empfundenen Umfange unbedingt durchgeführt werden muss, wenn soziale Katastrophen vermieden werden sollen*' (l.c. S.207)

In a curious way, the feelings of urgency got interwoven with the power display of the war years, to make this transition from '*gigantisch und absurd*' to '*muss unbedingt durchgeführt werden*'. It was known that things went wrong, with several of those large-scale projects, yet the sense of crisis made engineers and politicians alike adhere to the slogan '*muss unbedingt durchgeführt werden*'. Once more an ideology was born not just from power play, but from a reaction to some clearly perceived and overwhelming needs (Goudzwaard et al. 2007).

A note on the psychology of progress: For several reasons after World War II there was a deep psychological urge not to look back at the war years. Soon myth construction would help pushing it away. At the level of people's existential needs, the attraction of 'progress' was quite understandable. We see e.g. social scientist Fred Polak (his 1947 thesis is truly remarkable) in the Netherlands dedicating himself to 'science-based progress', refusing to look back at this war, that had brought such a horrid fate to his friends and relatives at the hand of the Nazis (Polak 1950, 1951, 1954).
The reasons of others to 'go for progress' may have been less profound but, just like Polak, in the next decades they would hold on to their faith, even in the face of disappointments (if not disasters). Mansholt, the 'author' of the Common Agricultural Policy of the EU, is one of them. Polak, in 1985, was well aware that several of the 'opportunities' of the 1950s had proved to be unrealistic, yet his futuristic faith in S & T was unshaken, as is evident from his projections for agricultural high-tech and biotech (Polak 1985, XII).

Yet, with the irrational transition indicated, we had definitively left the realm of common human endeavours (where S & T belong), and had entered that of ideology, the ideology of High Modernity (with technocracy at its core - for half a century Jacques Ellul would not tire of warning against it). For a time people did not entertain the slightest doubt that (Nash 1966 p.123)

> '*At the center of the modern socioeconomic world, the industrial revolution is the application of a growing science to all branches of production… [Follows a 'set of outlooks' that the author deems necessary]… Modernity is the social, cultural and psychological framework which facilitates the application of science to the processes of production. And modernization is the process of making societies, cultures and*

individuals receptive to the growth of tested knowledge and to its employment in the business of daily living'.

In the 50s till 70s a host of social scientists labored to dress up 'modernity' in all the apparel that the social science of the day could deliver. Most elaborate was Inkeles & Smith's 1974 '*Becoming modern*'. In it all of the advanced social science techniques of those years were being used to provide an impressive foundation for 'modernization' (also Inkeles 1973). Yet, looking back, the circular reasoning is embarrassingly clear (cp. Nederveen Pieterse 2000).

In the meantime, with 'modernity' thus the common faith of all 'rational' thinking citizens, doubting the faith was evidence of 'backwardness'. As the sharp-witted biochemist & literator Erwin Chargaff characterized the mood of those years (1982 S.130):

'*Wer den Fortschritt nich mitmacht, ist rückständig, zurückgeblieben, unterentwickelt*'. He then pointed to a central aspect of it all:

'*Der Fortschritt entpuppt sich als die Flucht für die Verantwortung*'.

In no time at all, High Modernity and its products – from mass produced cars to centrally developed educational testing in schools – were referred to as 'irresistable progress'. With general evaluation thus lacking – at a fundamental level certainly - the feverish modernization of the era had the character of a 'flight forward'.

Yet to most of those concerned it was a heroic effort to transform a traditional world of people and places into a world where 'rationalized production' would boost volumes (and profits), setting people free from the contingencies of e.g. local ecology and community. In the eyes of those involved in the effort - mainstream economists dominant among them - 'rationalized production' here meant 'factory-like production'. Indeed the input-output model is

> '*the basic conceptual model favored by economists since the second World War. National growth is likened to factory production and the problem of development is to get the proper inputs to achieve maximum output rates and returns. This model…is a useful paradigm for planning. And it has mensurability (primarily through the GNP).*' (McGranahan 1972 p.97).

The conviction that factory-like production can be (and should be) imposed everywhere in the economy is a peculiar part of High Modernity. Such production can be planned and steered at will, was the general opinion. Enough of a reason, it seemed, not to give it a try here and there, but to impose it everywhere. At the heart of this 'factory ideology', in its relation to agriculture, there was the conviction that now the chains that used to hold the farmer and agricultural production 'bound to the clod' had been broken 'by science and technology'.

A note on Modernization's literature: Of present day authors *Eisenstadt* is a straight descendent of the mid-century 'modernization' trend, but one with rather an open eye for its complexities (Eisenstadt 2001a & b). Similarly having gained a broader perspective, but with an economic accent still akin to this paradigm, is *Pomfret* 1992. *Lübbe* 1997 illustrates how & why this modernization option could seize the mind of the post-war generation that, esp. in Germany, had become rootless because of their people's recent history. The book also illustrates the closed paradigm that resulted.
Perusing the many books on 'modernization' of American authors of the 50s and 60s, one is reminded of Stephen King's sketches of those American fifties in his book '*On writing*', giving us a vivid impression of a rather harsh and competitive society. A difficult starting point for anyone intent on 'developing' people in other cultures...

Von der Mehden is right when he calls modernization's own literature 'academic scholasticism' (1986 p.13 f.). As he indicates '*it is somewhat frustrating to find*' - even in regard to the minority of authors with thorough field experience in the Third World - '*many of their conceptual contributions to the general modernization literature....so data-free and lacking in concrete illustrations from the authors' experience in the field*'.

To get a clear picture of the convictions that steered the first post-war decades of agricultural policy, one should consider the following quotation from Mosher 1971 (p.12/13):

> '*Farms can only be operated efficiently at constantly rising production levels.....in the presence of a set of what we may call agri-support activities. These are not farming, but are essential to progressive farm production. They include the industrial and commercial activities of mining or manufacturing, processing, and distributing fertilizers, of producing and distributing improved seeds, of manufacturing and distributing agricultural chemicals, of manufacturing and distributing farm implements and other equipment, of collecting, transporting, storing, and processing farm products, and of managing farm credit. Agri-support activities also include a set of non-commercial activities: research to develop a constantly improving farm technology, extension services to help farmers develop appropriate knowledge and skills, and education and training to provide the skilled technicians needed by all of these agri-support activities.* ***If farms are the assembly lines of agriculture, then farming localities within which farmers have ready access to all of these agri-support activities are the factories'*** (emphasis added, JV).

The strong faith in 'factory methods' for agriculture was common to agricultural policy on both sides of the Iron Curtain. Both were technocratic through and through, with their common ideology aiming at centralization of expertise and power (with the concommitant denial of knowledge and potential at the 'shop floor'). Both capitalism and communism, as well as other systems stressing centralization, were hierarchical systems in which the 'top' decided about the 'floor' .

When the Cold War caused the two power blocs to accept the same ideology in terms of transforming nature and society, it was especially the increasing distance between the 'floor' and the 'top' that thwarted the questioning of the common ideology. In regard to agriculture, this was the more so because this peculiar post-war *mix of technocratic conviction and politics* had pervaded the international scene, e.g thanks to the activities of bodies like the (Rockefeller dominated!) US Agricultural Development Council and the FAO. A quotation from the 1967 President's Science Advisory Committee report '*The World Food Problem*' (see Schutjer & Coward Jr. 1971 p.29) illustrates the mix indicated:

> '*The modernization of agriculture in the developing countries will involve capital investment, provision of inputs in the form of seeds, fertilizers, pesticides, water, and machinery, organization of distribution and marketing systems, education of agricultural specialists and extension workers, provision of production incentives for individual farmers in the form of land-reform and pricing policies, and other changes in the social and economic structures*'.

Power structures and institutionalization policies definitely played an important role, yet it was not just 'power play'. The unity of approach stemmed from a common conviction, that also had a moral side, the common serious attitude pervading these first post-war decades.

For activities under the aegis of the US (cp. its Agric.Dev.Council where Nelson Rockefeller was Chairman of the Board of Trustees) see Wharton Jr. (ed) 1969, Mosher 1966, 1971, 1976, Rogers & Svenning 1969, Moseman 1970, Wharton (ed) 1969/70 (Preface!), Leagans & Loomis (eds) 1971. Refer to a later chapter for FAO's policy history up till the 1970s.

The mind-set of those post-war decades was still far away from that of the 1990s, that '*Greediest decade in history*' (Stiglitz 2003). Below you will find a quote from John D. Rockefeller (3rd)'s February 1965 address to the Conference on Subsistence and Peasant Economics (cp. Wharton (ed) 1969/70, Introduction):

> *'The list of questions that need answers is long. For example, how can a poor farmer, barely able to grow enough for his own survival, afford even the simplest investment in seed, insecticides, and machines that may produce a better crop? How can his struggling government afford to give him the subsidies, the technical assistance, the extension programs that seems that he must have? ...How can he be induced to put aside centuries-old methods to experiment with the new and the foreign when he is experimenting, literally, with the food his family must have to survive?'.*

With the benefit of hindsight the narrow, technocratic perspective is clear enough. Yet these were no cranks, but people who within their own frame of mind were trying to find solutions to pressing problems of food and agriculture. The desires to build a better world, after the traumatic experiences of a war of global extent, still were present.

The faith in 'factory methods' guided the efforts, yet, none of its adherents took pains to compare their application to such widely different human enterprises as industry and agriculture (in regard to e.g. their material foundations). Yet, it is immediately apparent that

- **(a)** agriculture is essentially open to soil, environment, and ecology, while industry 'works' only in strict confinement (because it needs high energy and materials intensities)
- **(b)** agriculture's chief production process, photosynthesis, is a low intensity high area process, the converse of the high intensity low area processes that are fundamental to industry.

These differences seem fundamental enough. Their wholesale neglect in post-war 'industrialization' of agriculture boded ill for the 'factory project' in agriculture, and the problems presenting themselves can hardly be called a surprise. We'll soon return to this subject, but we will first need to take a closer look at the modernization project.

3.3. Modernization's progressive political framework

Not only with Rockefeller, but also with his far less prosperous compatriots, the development-scene was one of the rich, speaking from their 'level of attainment' about the poor. Hardly the first time in history, except now there was this intimate connection with the technocratic perspective. The moral obligation of the rich countries was narrowed down to assisting the poor ones to '*catch up*'. The transformation of their agriculture was presented as a core element of their '*take-off into economic growth*' (Rostow), that alone would bring them into the realm of plenty.
Taking distance of former direct aid, president Johnson in the 60s refused further food supply to famine-prone India unless the HYV-package was accepted (e.g. Doel & Harper 2006). Less than two decades earlier the shift to this package's formation as the one and only focus of agricultural research and policy in the own country (the US) had been a grave choice indeed. For any and all options centering on (the own resources of) farm and farmer had been

expelled from the public realm (as we shall see). With that the big industry perspective prevailed in agricultural policy.
The fact that this perspective was not only singled out everywhere in broader economic policy, but also became more rigid, had its specific historical reasons *not stemming from agriculture*. I will mention a few of those reasons, to highlight the historical ontingencies. I will start off with the beginning of the 60s in the US, and take some effort to familiarize the reader with some of the complexities of those years that made the majority go along with the wrong choices.

A turning point was the definite choice of America for a kind of permanent war-economy. In his Januari 17, 1961 farewell address, that was broadcasted to the nation, president Eisenhower had publicly warned against the risks of a Military Industrial Complex taking over American economy & society (cp.White 1996 p.302 f.). Now, after nearly half a century, with at the end of 2007 already 3500 billion dollar spent on the Irak war, we are painfully aware that his warning fell on deaf ears. When we read both his statements

> '*we have been compelled to create a permanent armaments industry of vast proportions*' and
> '*we must guard against the acquisition of unwarranted influence....by the military-industrial complex*',

we see a man who himself was at a loss as how to reach the goal he had set for himself. But in any case, with all his conservatism he had not lost the notion that all this weaponry was

> '*to protect the great values in which we believe, and they are far deeper even than our own lives and our own property...*'.

He still remembered the events of World War II very clearly, and what was more

> *'Fundamentally, Eisenhower had rejected the idea that there could be a military solution to Cold War problems or that America could shape the world's destiny*' (Ambrose 1988 p.181/2).

The Kennedy's did not accept these limitations on America's role, though they apparently were not enthusiastic about the MIC. In any case John Kennedy in his last speech (White 1996 p.305/6) defended his armament policies before the *Chamber of Commerce* in Fort Worth (Nelson Rockefeller was his antagonist by then – Collier & Horowitz 1976). The choice of McNamara as Secretary of Defense was hís, and with that choice he gave his approval to the great rise in the production of nuclear armaments (Ambrose 1988 Ch.10). But there was a counterweight: Kennedy still gathered critical minds like the economist J.K. Galbraith around him (Galbraith was his ambassador to India). Those people were sidelined after his assassination, when his successor Johnson from '63 till '68 dominated

> '*public life as almost no one before him. Even under Roosevelt there had been room for a varied, often colorful, cast of officials – men of independent stature who often asserted their clashing views to press and public rather than conceiling them in "eyes only" memoranda*' (Kearns 1976 p.213).

In those years senator Robert Kennedy was Johnson's one and only political opponent. The voices of Bob Dylan, with his 1962 '*A hard rain's a-gonna fall*' (sung during the Cuban missile crisis), and Barry McGuire's in 1964 with '*Eve of destruction*', were widely heard, but hardly by the world of politics & commerce.

For some years the majority in the US was not only quite satisfied with its 'American religion' (see later), but steadily drifted to Nixon's ultimate defense of America's 'right' to use a disproportionate part of the earth's wealth and resources. A National Security doctrine and policy surfaced (Nelson Rockefeller c.s.) that, in effect, re-made the US economy into a

permanent war-economy. The restraint voiced by Eisenhower was rejected, and those years without substantial political and economic opposition allowed an unrestricted growth of the MIC as the 'guarantee' of America's mission (e.g. Johnson's *Great Society,* Kearns 1976).
When Johnson supported the 1964 military coup in Brazil, nobody reacted. Other coups the world over followed, with the US in the same disquieting role, a.o. in connection with the establishment of military bases in the countries involved (Bonner 1987 Ch.9).
Most of these coups, by far, led to the repression of the peasantry. After the coup in Brazil, for example, the Brazilian government strongly promoted remodelling Brazilian agriculture after the American example. It used to be largely a smallholders system, and was changed into a to capital-intensive system producing for export. The results (Korten 1995 p.49):

> '*the conversion of agriculture from smallholders producing food for domestic consumption to capital-intensive production for export displaced 28.4 million people between 1960 and 1980 – a number greater than the entire population of Argentina*'.

Meanwhile the application of the McNamara policy, aiming at financing the sky-rocketing US armament costs with the export of advanced weaponry, got into full swing. This resulted in the acceleration of the arms race and, within a few decades, established the MIC every-where on the globe. The ultimate result is perplexing. Many observers from the West indeed condemn the increase in terrorism and regional wars, yet seem unable to grasp that their own 'safety-doctrine', relying on its arms manufacturing & trade, is at the heart of this increase.

> For a succinct exposition of this **safety doctrine,** which was idolized at the expense of the people who believed in it, see Goudzwaard, van der Vennen & Heemst 2007. For the McNamara policy see Sampson 1977 Ch.6. For McNamara the person amidst the perplexities of history see Blight & Lang 2007. For the connection between our arms trade and regional wars etc. see Stohl & Meyerescough 2007.

Then the end of the Bretton Woods brought new rounds of impoverishment for the poor countries, because now the rich countries were freely creating international liquidities to the detriment of the poor ones. Efforts at mitigation by way of the UN New International Economic Order got frustrated, because everybody stuck to the technocratic order. This was the order of the industrial countries, and these countries now cashed in (see Dube 1988 p.45f. and Pomfret 1992 Ch.10.2 for some critical accounts of the NIEO). But note that they at least partially did so because of their own problems with 'liberalization'. For it was not the 1973 'energy crisis', but the 'liberalization' of financial markets with the end of Bretton Woods, that destabilized economies everywhere. Interest payment by US nonfinancial corporations started eating away up to half of their profits from about '76 on. Next, developing countries saw the real interest on their debts soar from '79 on. (Cp. Duménil & Lévy 2002).

The 70s also saw the creation of world grain markets no longer under some system of international control, that in a way hád existed thanks to a.o. the World Wheat Treaty of the 40s. But now a 'market' developed that was dominated to an unparalleled extent by a few American giants and that was closely intertwined with the 1972-1974 years of famine in countless countries (Gerlach 2005). Before long it accelerated the destabilization of small-farmer agriculture everywhere. History got the chance to repeat itself - but then at a lower level of society.
The 'grain market' evidently had a big influence on international relations in food and agriculture. So did the repeal of Bretton Woods, with a.o.the ensuing liquidity creation by the rich countries leading to a slump in the real profits of (agricultural) exports of Third World countries. More in the background, the arms race with its immense costs (financial and

human) was exactly the reverse of the ‘swords into plough shares’ kind of change the world needed (cp. esp. Goudzwaard, vander Vennen & Heemst 2007).

> As to the grain market history, think of the huge grain exports, especially from the US, since the 1870s that had destabilized European agriculture - and which at the same time, because of oligopolies of rail transport and grain trade in the US, had brought poverty (and death even) to crowds of farmers in the US itself. Think also of the Russian grain exports in the 1930s (Madsen 2001 p.356/7), causing great hardship and famine among Russian peasants, and at the same time deepening the global crisis of agriculture. By then, that crisis was grave already, due to the Depression making it easy for big industry to further widen the gap between the prices of industrial and agricultural products (also Madsen 2001).

All of those ‘powerfull’ changes meant that the technocratic option with its big-producer perspective got institutionalized even more strongly, *but without any considerations of sustainability of the ensuing global technological system and, worse still, without any reflection on the (lack of) robustness & sustainability of primary production*. Significantly the ‘powerful’ system, originating in decades of faith in ‘unlimited’ S & T, did not itself have the concepts & methods to link up again with serious research probing the ‘down to earth’ limits and exploring realistic alternatives. It seems that the very concept of ‘power’, inherent to the technocratic system, did not allow the understanding of alternative approaches of long standing (cp. Schumacher 1961 and Myrdal 1955), or exiting new research. As so often in history – think of Peter the Great in Russia and Frederic the Great in Prussia - the war-related economic, institutional and political framework became so dominant, that options related to the life of man & creation were discarded.

Yet looking back to the mid-sixties, it is clear that most of these troublesome consequences were not immediately visible. In those years the unbridled optimism about the constructability of nature & society did not only prevail, but reached a climax with the landings on the moon. For a short time, about all policy makers on the globe were completely convinced of the superiority of the ‘high-tech’, big-industry perspective.
Earlier, the end of the colonial era had made policy makers in the many new states look in earnest for a future that would no longer be dominated by the inequalities and poverty of the recent past. Already then the promises of grand-scale solutions, as presented by people from divergent political backgrounds, made all of the new governments pin their hopes on science & development as exemplified in the industrial countries (Dube1988, So 1990, Therborn 1996). Even the well thought-out approaches that started from the common people, like that of Gandhi’s, got rejected in favor of ‘power approaches’ (explanation in Myrdal 1968, see also Schumacher 1961). Then in the (50s and) 60s the ‘successes’ of the industrial countries strengthened these convictions still further. All in, and **in spite of great political and religious differences, for a time the world was one in its faith in ‘High Modernity’**.

> That faith in itself is extremely puzzling. After all, there is nothing as divisive, both in the past and in the present, as the chasm created by modernity between ‘backwardnes’ and ‘progress’. Former socio-cultural divisions rarely touched people’s *humanness*, but this chasm alienates them from most of their relatives (e.g. ancestors) and from most of their neighbours. Eventually it alienates them also from their own children. For a most incisive critique emphasizing these and related points see Berdjajew 1925 Kap.X.

The strong faith that, especially in regard to agricultural production, the industrial countries had conquered nature, was at the centre of this ‘High Modernity’. With centralized research greatly extended during and after World War II, and the central experts not aware of their limits, nothing seemed capable of stopping the systematic transformation and extension of agriculture after the example of industry. The near-complete lack of regard for the role of agriculture and the rural environment, in US and Western European publications on post-war economy and culture, quite likely derived from this conviction, that now nature and food production were under control. It was in agriculture, first of all, that technoscience was 'certain' to go from victory to victory! As a result, especially in agricultural research and policy, re-evaluation of this technoscience and its results was out of the question.

Yet the acceptation of this comforting conviction - that control (of food production) was assured - can hardly be called rational. Everywhere the dependance on local agriculture had been crucial, during and just after the war, in an agonizing way. And yet people who had not forgotten this recent past, e.g. Louwes in the Netherlands in the House of Parliament, had to remind the government of this crucial role of local agriculture. To no avail: farmer and local agriculture were out of the picture since the beginning of the 1950s. Agriculture was considered solidly 'on track' thanks to 'modern agricultural research'.

And so neither of the two volumes in the Netherlands, that in 1955 commemorated the 10-year-anniversary of the liberation (Damsté & Cocheret (red) 1955, Van 't Veer & Schrofer 1955), even mentioned agriculture. The picture on the cover of one of them gives an artist's impression of the ruins of war opening towards the shining modernity of a town with high-rise flats, industry and harbours. There is no rural landscape, not even anything like the immature landscapes of the new polders. Only at the third look one discerns big, featureless agricultural fields, with a farm in the corner of the picture that, except maybe for the red roof tiles, could be an industrial building as well.
This picture expressed a central aspect of government policy: except for some nature conservation efforts (and few in the field of culture), the patchy and diverse social and ecological landscapes that still dominated society in pre-war decades were relegated to the dustbin of history. Of course there were quite a few voices pointing out that life itself was suffering the same fate. But it is only too evident that these voices fell on deaf ears: they did not speak the language of High Modernism. And because that language was spoken everywhere on the globe - in the capitalist, in the communist and in the non-committed countries - policy makers were quite sure that they were on the right track with their efforts at all-out 'modernization'.

3.4 Regaining evaluation

More than half a century later the voices of doubt have multiplied. This increase indicates that the generation that had shaped and implemented the High Modernist policies, got relieved by new ones. Those new generations, unlike the previous ones, have neither wrestled with the traumatic experiences of World War II, nor made an existential investment in building a new world of High Modernism. Indeed, it is evident, from thorough studies like Tony Judt's (2005) '*Postwar*', that historical research covering war and post-war decades is increasingly able to look at this period's characteristics as historical phenomena, and not as some 'definitive' traits heralding 'the end of history'.
These authors' critical stance now includes technoscientific subjects that are at the heart of post-war society (all over the world). They do not bother analysing some 'aberrations' at the periphery of a main stream of technoscientific 'progress', but **question this 'progress' as such**, and in a detailed way. Importantly, these researchers are also willing and able to scrutinize the period's gospel of economic growth into the realm of plenty, **including the core belief that its industrial agriculture had definitely conquered nature**.

(Josephson 2002 is a good example focussing esp. on the US and the USSR. Diamond 2005 to the contrary, though often well informed, is still very much constricted by modern western culture).

> **Critical analyses of post-war Modernization:** An early and readable account of pre- and post-war modernization hypes is Margaret Mead's personal one (Mead 1974). K.Polanyi c.s. treated several aspects incisively, but they were ignored (Polanyi et al. (eds) 1957, Polanyi & Pearson (ed) 1977, Bienefeld 1991). Systematic accounts of the different post-war stages are given in Dube 1988, in So 1990 and in Wehling 1992. A study placing 'modernization' in its wider historical context is Touraine 1995. Wagner 1988 & 1994 takes a sociological point of view and Wehling 1992 a social scientific one; they both include the more recent historical aspects of the subject. With its self-critical Indian stance, Ramachandra 2003 is especially valuable. A geographic approach, including openness to changes in language and culture, is offered by Gregory 1993 & 1998.

Among the different publications enquiring into the matter is James Scott's 1997 '*Seeing like a state. How certain schemes to improve the human condition have failed*'. It has been an outstanding body of work for a decade now and has provided information for the broader discussions. Scott is one of those authors who point to this fact that a 'radically simplified' version of agriculture is at the heart of post-war High Modernism (esp. l.c. Ch.8). A quote:

> *'The simple 'production and profit' model of agricultural extension and agricultural research has failed in important ways to represent the complex, supple, negotiated objectives of real farmers and their communities. That model has also failed to represent the space in which farmers plant crops – its microclimates, its moisture and water movement, its microrelief and its local biotic history. Unable to effectively represent the profusion and complexity of real farms and real fields, high-modernist agriculture has often succeeded in* ***radically*** *simplifying those farms and fields so they can be more directly apprehended, controlled, and managed'* (p.262)

Here Scott touches on a great many subjects that are fundamental to sustainable agriculture and food production. As we shall see, governments everywhere got impatient with '*the profusion and complexity of real farms and real fields*' and went for '*fast and powerfull*' changes. The specific war-time and post-war circumstances made for greatly enhanced central government bureaucracies with greatly extended competencies, that now reached down to the local level, where before they had only limited access.
At the same time applied research experienced an accelerated growth that, in fields like agriculture, was completely to government direction, esp. in countries like the US and the Netherlands, where it became research *as defined by* government policy – concepts, methods, and all. Research was limited to 'modernization' and effectively made an extension piece of government policy and bureaucracy. This research, as constricted by the straight-jacket of government policy, was institutionalized in record time, but it completely bypassed the farmer and his local resources. That was a process in which the circumstances of the war played a decisive role. With these origins, post-war policy and research definitely suffered from a lack of concepts and methods that were adapted to farmers and farms.

> **Government direction of research, examples:**
> In the Netherlands the Institute for Biological Field Research ITBON, founded in 1940 (no result of occupation policy), up to 1949 was part of the Central Organization for Applied Research TNO. In '49 it merged with the Agricultural Organization TNO that, in spite of its name, was strictly directed by the Ministry of Agriculture. Its research into the effects of hedgerows - started already in '42 and first published in '49, included effects on the crops in the agricultural fields bordered by the hedgerows.

In spite of its high level, and its positive indication of ecological influences, any further research got strictly limited to the meteorological influences on the fields enclosed by them. Only years later (literature) research in some wider aspects was allowed again. This indicated that positive research results were in to be had, that would be to the credit of local farmers and horticulturists. But the Department of Agriculoture in those years was greatly 'simplifying' the Dutch landscape, with its grand-scale re-allotment works. The ITBON research did not fit into the picture and was discontinued (van der Linde 1958; cp. ITBON 1965).
Yet recent research leaves no doubt that this policy was a result of 'ecology blindness', cp. de Leval 1996, Burel 1996, Herzog 2000, Landis et al. 2000, Benton et al. 2002, Boutin et al. 2002. We meet that 'blindness' also with the Department's attitude towards the research of Anne Post and Johanna Ruinen. These ladies had acquired international fame with their research, that was highly relevant in regard to natural resources in agriculture. And yet, they had to take jobs outside the research network, because the government refused to finance follow-up research (Post 1962, Ruinen 1956/61/65/71/74).

The discord soon became apparent, and it made politicians and researchers more impatient still with this 'tendency to follow tradition'. Convinced of the superiority of their approach – a conviction not unrelated to their position of power – officials in industrial countries took the lead in transforming the agricultural sector into one more manageable 'from the centre', by virtually removing farmer and ecology and replacing them with machines and uniform-itized soils.
It was proclaimed as a token of progress, but note that the (small) farmer in the 'developed' countries had no more voice in the process than the peasant in the Two Thirds world. Also in a country like the Netherlands, the great majority of the (small!) farmers was faced with a government (and its experts) refusing any input from their side (their resources and networks were no items in research and planning).

In the special circumstances of the war and of the post-war years, a greatly enhanced government bureaucracy, with its policy-directed circuit of institutional experts, issued top-down directives that practicians had to follow, by force of law. Officials and experts were absolutely convinced that their philosophy was right. And yet, when they were in control, things were manipulatd, and now after half a century the power play of it all is unmistakable (Scott l.c. p.264):

> '*...given the presumptions about expertise embodied in modernist agricultural planning, the actual schemes were continually bent to serve the power and status of officials and of the state organs they controlled'.*

Who is aware of e.g. the huge sums of money spent in those decades on re-allotments and so-called 'agricultural development', realizes at once that the presumed necessity of it all was indeed shaped by government power (see Benholdt-Thomsen & Mies 1999 p.74 f. for an example). Seeing that it entailed the near-complete neglect of the small farmer and his knowledge, we have ample reason to take a fresh look at his case, both in the rich countries and in the Two-Thirds World.

Now the 'weeds' example, with which we started the present chapter, illustrates that the small farmer knows more of locally relevant matters than the expert in the camp of the government and industry. It is the expert's refusal to take into account the peasant's expertise, and his insistence on the introduction of the machine, which *turns weeds from a partial asset into a wholesale problem* (and into a lasting one, at that). Indeed as indicated before, such knowledge-intensive traditional resources like 'weeds' are phased out by 'modern farming'. A contributing factor is the re-formulation of 'fertility' as the liberal gifts of industrial nitrogen fertilizer, for this strongly reduces plant species diversity (Gough et al. 2000, Rajaniemi 2002, Suding et al. 2005). Yet the loss of e.g. herbal medicines and 'health-feeds' is not taken into account, as a rule, when the story is told of the introduction of 'modern agriculture'. Instead, the forced dependance of the farmer and the local community on supplies from outside – forced, because local resources and their use got phased out as a result of enforced government policy - is counted as 'economic growth'.

As is to be expected, considering its background, in post-war institutionalized agricultural research, as a rule we come across the same kind of limited 'product research' that is evident in big industry. It is **research** that is promoting the use of centrally devised industrial products and **that is silent in regard to products and production methods based on local resources.**

> The following example will demonstrate this.
> When big industry celebrates the invention of **nylon**, it is not for this synthetic's qualities in clothing. After all, its **very simple structure cannot compete with the intricate hierarchies and composition of natural fiber**, with greatly inferior interactions with water vapor/ humidity as a result. Cp. Atalla (ed) 1987, Rebenfeld 1992, Krassig 1993 on cotton/cellulose, Kaplan et al. 1991 on silk. Nylon never-theless got promoted by industry because its potential of central, large-scale production that allows for centralization of power, at the expense of (ever-local) natural fiber production.

In agricultural research, modeled after industrial research, it was especially the same focus on centrally devised and produced inputs that made it miss out on the farmer's local organic and soil resources, after the war. This was not because of a lack of research methods that were adapted to these resources. A great many new biochemical-analytic methods, for example, were in a way ready-made for their investigation. Yet, the focus chosen by agricultural research caused it to ignore such methods completely (as we shall see in due course). Thus, the choice for top-down direction de-vitalized agricultural policy & research. Its roots in official policy definitely did not enhance the flexibility of agricultural research Remember Mannheim stressed that **'petrification' would follow the choice for functional rationality in research and policy**.

The consequences were huge. First government-backed agronomic science was firmly institutionalized within the policy-framework *that expected it to issue 'central directives' to farm & farming*. Then the ongoing development of such 'central directives' became the *raison d'être* of the big agro-industry, and gave it the impetus to grow to global size. Yet the centralized knowledge and power of these institutes and agro-concerns is worlds apart from the local, but 'wide open' paradigm of the farmer. In Scott's words (l.c.):

> *'the **imperial pretensions** of agronomic science – its inability to recognize or incorporate knowledge generated outside its paradigm – sharply limited its utility to many cultivators. Whereas farmers...seem paradigmatically alert to knowledge coming from **any** quarter should it serve their purposes, modern agricultural planners are far less receptive to other ways of knowing'.*

Did agricultural research & planning work from a 'tunnel vision', to the detriment of all concerned? To gain some understanding of the situation and its origins, in what follows we will 'descend into the soil'.

3.5. Descending to ever widening horizons

A real urbanist by birth and education – I was born in Amsterdam in 1948, and had my first run of university education there – I was conscious of the fact that my knowledge of 'soil' was non-existent. As a child of my generation, not yet completely locked up in the urban sphere of life, I was aware of the soil as a great natural asset, with its fundamental importance for life. As a chemist I realized that *soil fertility* was a characteristic of this hierarchic soil, and that it was presumptuous to equate it with the injection of some bulk chemical.

Quite wisely a former generation had placed a cubic meter of 'black earth', chernozem soil at the International Chamber of Measurements at Paris as a benchmark of soil fertility (Hope 1997 p.86). And yet, without a close consideration of this complex soil and its complex fertility, IGFarben started to promote its industrial nitrogen fixation process as the proper fertilizer production process, in spite of the fact that it had been designed for destructive purposes (World War I explosives production).
The nature of these two approaches was unmistakably different:

- the one emphasizing that soil fertility is an earth-bound, non-reducible entity,
- the other in sharp contrast offering it for sale as an industrial product.

In the Interbellum the 'agricultural' and the 'industrial' approach both made themselves heard. In fact, in Germany itself the experience of hunger in World War I had taught the acknowledgeable expert that a gift of 'fertilizer' does not make up for a lack of careful treatment of soil with organics. The problem was, nevertheless, that this information was not heard from the 'agricultural stations', that were established by the 'fertilizer industry' after the war. Research focussing at the farmer's organic resources was badly under-financed, compared with the 'product research' (focussing at fertilizer gifts) that the 'fertilizer industry' was willing to finance. Still, it was only with World War II that the 'fertilizer industry' was suddenly moving to center stage, relegating organics-based agriculture to the margins.

But, as indicated, the war-related character of 'fertilizer' is indubitable. Moreover, for a chemist like me, there is no good reason to presume that 'soil' is an entity that is covered by the chemical industry, or by industry at large. So when in the early nineties I got the chance for some years to take a closer look at the soil – thanks to the activities of the Department of Soil Science of Wageningen University – I was delighted. R.C.Foster's superb electron microscopic pictures of soil micro life further peaked my interest. As Bowen & Rovira (1999 p.4) write especially about Foster's work: '*Much of our knowledge of the physical state of the rhizosphere and of the rhizoplane have been derived from transmission and scanning electron microscopy*'. (Watt is one of the few who carries on with Foster's work, e.g. Watt et al. 2006).

Foster's delicate pictures of wheat roots indeed enable one to descend into soil (see esp. Foster 1985, 1988. The wheat roots: Foster, Rovira & Cock 1983 pp.37/51/52).
In the **first** enlargement – a sugar cube enlarged to the size of an office desk – root hairs become visible. The **next** enlargement – sugar cube to room size – is impressive due to the many details of the root & root hair surface. Then the **third** enlargement – sugar cube to sizeable building – shows us that there is more detail still and introduces us to fungi and bacteria. Remarkably, the details in this world of the wheat root do not become less, in greater enlargement. To the contrary: at each new level they are new again. We evidently do not reach the 'smoothness' that is the precondition for e.g. description using calculus and differential equations, but have to turn to fractal geometry and similar mathematical treatments instead.

Fractals, health and stability: Fractal geometry (and chaos) is of fundamental importance in the sciences, Mandelbrot 1982. Physics (Coffey & Kalmykov (eds) 2006)), chemical engineering (Biardi et al. (eds) 1994), geology (Dauphiné 1995), hydrology (Molz et al. 2004) biology (Kaandorp 1992, Paar et al. 2001), geology (Radlinski et al. 1999), soil science (Young & Crawford 2001, Guber et al. 2004), physiology (West 2006), and enzymology (Ricard 1999), need its concepts and methods. It became evident that *disease is the loss of complexity*, with the stability of biological entities dependent on their ability to function as nested systems-within-systems. Each system level adds to additional stability of the overall fractal structure (Chauvet 1993, West 2006). With the application of fractals in soil science actively pursued a long time already (e.g. Burrough 1983) , *it is clear that also soil health & fertility depend on the particular soil's complexity, not just on some chemical nutrient status*.

'Uniformity' is not to be found in this micro-world. There is definitely order there, but it is both heterogeneous and hierarchical. Soil micro-life is of a dazzling variety and, with its placement in the soil hierarchy, partakes also of the latter's complexities. The soil dynamics (water, organics, etc) is completely dependent on this variety and complexity. **The order that we meet here is essentially different in kind from that of technical materials.**

Now it is evident from pictures like Foster's that there is space for an immense number and variety of bacteria. Indeed it has been evident for nearly a century that this variety must be enormous, and some decades ago the educated guesses were that

1. only a small percentage of the soil bacteries had been discovered by then
2. one cubic cm of soil contained at least several thousands of different bacteria.

With the adaption of molecular biological methods to the soil there came more possibilities to develop methods of sampling and assessment. As was also found in other environments - e.g. a contaminated aquifer, a ruminant rumen, and the oral cavity – not just new bacteria, but also new phylogenetic divisions (no cultured microbial species) were discovered.

Bibliographic note on soil micromorphology: The hierarchical character of soil (micro) structure: Emerson, Foster & Oades 1986; Lee & Foster 1991; Oades 1990, 1993; Oades & Waters 1991; and Kilbertus 1980 for a unique contribution on soil micro-aggregates. Further on soil microstructure and hierarchy: Tiessen et al. 1984 ; Jastrow & Miller 1991; Jocteur Monrozier et al. 1991; Dorioz et al. 1993; Colchin et al. 1994; Puget et al. 1995; Robert & Chenu 1995; Balesdent 1996; Besnard et al. 1996; Jastrow et al. 1996; Chotte et al. 1998 ; Sessitsch et al. 2001 ; Wilson et al. 2001.

Soil microbiology sampling & estimation – some references: As to recent developments concerning sampling & estimation, see Grundmann & Gourbière 1999; Tiedje et al. 1999; Ranjard, Poly & Nazaret 2000; Theron & Cloete 2000; Colwell 2006, Torsvik & Øvreås 2007, with e.g. Kuhad et al. 2004 placing it in a wider (also agricultural) context. As to the discoveries in the other environments indicated see e.g. Dojka, Harris & Pace 2000, and Wade 2006.

And so, recently, at least an order of magnitude could be assigned to the possible number of different bacteria in some specific Amazonian soil. That order was to the fifth power of ten (Lunn, Sloan & Curtis 2004), as such fitting in with earlier estimates (Staley 1997, Kuhad et al. 2004, cp. Torsvik & Øvreås 2007). In other words, descending into soil we meet a dazzling number of bacteria, the overwhelming majority for the first time. And we're quite certain that we have not discovered more than a fraction of a percent of the different soil bacteria. Dijkhuizen's (1998) speculation of some 10 billion species is no longer considered extravagant. Therefore, given the well-known fact that most soil bacteria do not lend them-

selves to our culture techniques, there is not a remote chance that we will ever be able to control them. The best we can do is to take great care of their environment, the soil.
In regard to the micro-life sustaining soil and agriculture, a real knowledge economy is one focussing on care, not one deluding itself into believing to have all knowledge, and therefore the ability to manipulate.

3.6. Paradigm change

Still most textbooks and reference books for agricultural researchers and practicians do not present this picture of soil micro-life. On the contrary, they define it as a rule as:

- (unspecified) saprofytes 'mineralizing' organic materials in soils,
- ammonifiers and nitrifiers providing mineral nitrogen in their wake,
- cycling of ammonium in the 'microbial population' and of
- denitrifiers transforming mineral nitrogen into gaseous compounds (esp. dinitrogen oxide and nitrogen gass).

Instead of the incredible variety characterizing real-life soils – including its unknown diversity of mineral and organic substances and complexes - these authors offer us a view of a neat and limited micro-world, providing the plant with just a few mineral nutrients, esp. the industrial nutrients ammonium and nitrate. Its simplicity led to enthusiastic efforts in modelling nutrient delivery, but also to a lack of field validation of the models (Benbi & Richter 2002). And in regard to the inputs for those models: none of the mineralization-related techniques that has been proposed is satisfactory (cp. the 'point clouds', in spite of log-log presentations, in Booth, Stark & Rastetter 2004). Consequently, we are at a loss with modeling, and with the advises and official regulations based on it. (Some specifics: Watson et al. 2000, Abril et al. 2001, Wang et al. 2001, Vinten 2002).

That gives us reason to pause. For it is undeniably significant that half a century of serious efforts has hardly brought any improvement. Very recently, however, there were some important paradigmatic results. In combination with the re-discovery of organic-N nutrition of plants, the ongoing lack of conceptual & methodical progress caused some well-informed ecological researchers (the renown Chapin among them) to **change the paradigm. This now starts from the in-situ, plant-derived, organic substances delivering a scala of plant-available organic-N nutrients.** Only at the end of all processes mineral-N nutrients do appear. The conceptual 'industrial hegemony' is broken.

But it testifies to the great strength of the former paradigm – that is still regularly taught ánd is the starting point for current agricultural advice – that it needed half a century of fruitless efforts to get researchers to question its presuppositions!

The N-nutrition paradigm change: primary references are Aerts & Chapin III 2000, Aerts 2002. Schimel & Bennett 2004 is a recent and extensive review (note it is weak in history). Frank & Groffman 2009 and esp. Jones et al. 2009 review the many conceptual and methodological uncertainties that are now apparent.
Note that the need to look at organic-N transport within the plant made plant physiologists aware, for years already, that plants are able to take up amino acids and peptides - Rentsch, Boorer & Frommer 1998, Williams & Miller 2001, Lalonde, Wipf & Frommer 2004. The number of amino acid and peptide transporters proved big indeed, and that forces us to reconsider our cherished assumptions – Hirner et al. 2006 and Murcha et al 2007 and refs.

Indeed, this move did not occur within the agricultural research and extension circuits, but within the far smaller one of ecological research into wild plants nutrition. In that field it was still possible to look more closely at what is factually present, that is, to gain a broad view of the possibilities for plant nutrition, and to escape the tunnel view that results when only (industrial) fertilizer is considered. We turn to the example of *savannahs* to intimate the difference (see Young & Solbrig 1993 for a renowned publication on savannahs).

Savannahs represent 25% of terrestrial biomes and are second only to tropical forests in contribution to terrestrial primary production. Large tracts of savannahs, e.g. mixed grass-tree humid savannahs of West Africa, have very low nitrification rates (Lata et al. 2004 refs.). This is mostly ascribed, for half a century already, to the grass species concerned exuding e.g. phenolics, that then inhibit nitrifying micro-organisms. If, what is quite exceptional, some grass varieties allow high nitrification, these are smaller and grow more slowly than the varieties dominating the low-nitrification sites (l.c. p.609). Evidently, the low-nitrification varieties benefit greatly from preventing most of the 'mineralization' pathway, and likely use the plethora of organic-N compounds becoming available (before, in the end, ammonium is split off, that is toxic already in rather low cocentrations).

> Recently Ushio et al. (2009) in summarizing their agricultural model studies wrote: '..*plant nitrogen acquisition was maximized at intermediate levels of phenolics, but only when plants could utilize organic nitrogen. Furthermore, this pattern occurred over a broad range of parameter conditions*'.

But note that an ample gift of nitrate will, in all likelilhood, give a precipitous decline in phenolics exudates. With now these phenolics' inhibition of nitrification removed, nitrate will show up as 'the primary plant nutrient' that main-line paradigm states it is. At the same time, soil organic matter mineralization as a whole will accelerate.
Note that in this case it is the fertilizer gift itself that is responsible for the increase in nitrification, and for the losses that are concommitant to it (nitrate leaching – nitrogen loss by denitrification – soil organic matter loss). In other words:
it is quite conceivable that the ample use of mineral fertilizer, that dominates the experiments of mainline agricultural research, has promoted as 'standard' what in fact is a disturbed soil and plant nutrition system.

> **A standard excluding the consideration of reality:**
> It is pertinent to note that, in low-input systems, Graminae *quite generally* exude phenolics that prevent nitrification (Munro 1966). The same is true for e.g. beech forests (Boquel et al. 1970). Note that in beech under natural circumstances (and irrespective of growth rate) nitrate is 'almost absent' in leaves and always less than 3 % of total soluble N-compounds in fine roots (Fotelli et al. 2002).
> Conversely, to decide from measurement of nitrate contents (of e.g. leaves) to the 'N-status' of a plant is a curious step indeed: if mineral fertilizer is not provided as the main nutrient, that 'N-status' is likely reflected in soluble organic-N, not in min-N (cp. also Guak et al. 2003). To prove the 'need' for nitrate fertilization by measuring nitrate in leaves is, in plain fact, hardly more than circular reasoning.
> The predominant use of mineral fertilizer (in hydroponic or sand systems) in research in plant physiology, only strengthened agricultural researchers in their circular reasoning. Then, for a long time they inhabited the PKN-cage together. Untill some researchers, whose work brought them outside this cage, discovered it could not contain reality.

Other studies, e.g. European forest ecosystem studies (E-D.Schulze (ed) 2000), invalidated other standard assumptions of mainline agricultural research. E.g. Bauer et al. (2000 p.91):

> '*ANPP [aboveground net primary production] for spruce across the European transect changed independently of needle N concentration, which is the opposite of what is known for annual or crop plants.… Stand productivity for these forests appears to be under the control of yet another factor than leaf nitrogen concentration*'.

So what micro-world was depicted, for more than half a century, by agricultural research and extension? It was a micro-world in which only those nutrients, that the mineral fertilizer industry has to offer, play a part. In other words, its authors described a soil micro-world completely in line with our fertilizer industry, and 'thankful to receive its gifts'. In a miraculous way, the man from the industrial laboratory feels perfectly at home in matters concerning the soil. It will be clear by now that **this is a picture of a virtual soil**. The industrial expert is at home in his industrial ecology (office and laboratory), but not in the soil's (micro)environment. He simply puts the stamp of his laboratory ecology on the soil and constructs a soil micro-environment with its concepts and products. And it is this virtual soil only that lends itself to manipulation by (the protocols devised by) the expert. It is also only in this virtual soil that 'fertility' can be equated with 'mineral-N provision'.

In complete contrast, up to World War II (1) organic manuring and (2) intercropping and/or rotation with nitrogen fixing crops were the primary means to enhance soil fertility in intensive types of agriculture. Care for the soil and its flora & fauna was part-and-parcel of the concept of 'soil fertility'. The activities of earthworms was a well-known signal that the health of the soil was intact. But after the war the use of mineral nitrogen fertilizer sky-rocketed. The traditional knowledge- and labor-intensive methods of care were supplanted by mechanized agriculture - without the care needed for the micro-life of the soil. In parallel, soil microbiology and related subjects did not partake of the fast growth of agricultural research and extension after the war (Smith 1948). Main-line agricultural science did not avail itself of the lessons of soil microbiology, and of those of soil (micro)morphology, etc.

Mainline research developed its tunnel vision of soil and soil fertility, and experienced an impressive institutionalization. Disciplines bordering on agricultural science increasingly depended on this institutionalized circuit - and started to copy e.g. its peculiar soil concept. Before long, disciplines like plant (root) physiology adapted to the 'tunnel vision' with its 'virtual soil'.

Even ecologists started to equate the soil resource to its substitute within the 'tunnel vision', the laboratory artifact. Hammer summarizes (1998 p.126):

'*Ecologists' reductionism of the soil resource can be categorized under three broad assumptions: (1) the soil landscape is comprised of univariate gradients (2) the upper few centimeters of soil represent the temporal plant-rooting environment (3) N is a surrogate for soil fertility*'. And he illustrates how '*these closely related misperceptions*' got to dominate much of ecological research. That is a.o. true where 'N' is considered a surrogate for soil fertility (l.c. p.129, 130):

'*Ecologists seem to be fascinated with surface soil nitrogen (N): they seem convinced that N and phosphorus (P) are the only plant nutrients of consequence….*'

'*An ecological focus on surface N and P probably can be traced to Vitousek, who has written that N limits productivity in forest ecosystems and that N mineralization controls the availability of other nutrients. The former is not always true, and the latter is not based on published research*'. With Vitousek, as to plant productivity often quoted as a very authoritative researcher, we realize that the many publications starting from his conclusions partake in his misperception as to 'N', as it derived in turn from the 'virtual soil' of mainline agricultural research….

The impressive growth of mainline agricultural research was part of a post-war technocracy that essentially did not admit any limits to its ‘conquest’ of nature and society. But note that already Pascal rejected such ‘limitless’ presumptions as the products of a ‘weak reason’: when reflecting on the infinitely small and the infinitely big, he concluded that they equally do escape us (Pascal 1670/1954 art.84). In the same vein, Foster c.s. helped us to understand that, descending into the soil, our horizons get ever wider (and our modesty ever deeper). Quite in contrast to Foster’s exemplary researches, an agricultural science pretend-ing ‘to know’ is unable took look beyond its own constructs. Its pretenses make it weak indeed: only an awareness (and next explication) of its limits can help it grow stronger.

3.7. Technocracy rules - by collapsing the world

Some three-and-a-half centuries ago, Pascal emphasized:

> ‘*La dernière démarche de la raison est de reconnaître qu’il y a une infinité de choses qui la surpassent; elle n’est que faible, si elle ne va jusqu’à connaître cela*’.
> (Pascal 1670/1954 art.466)

Post-war technocracy was built exactly from the denial of this insight, and it was this denial that determined technocracy’s dealings with the farmer and the soil.

As Kearney (1996 p.58) describes it, post-war modernization, both that from the left and that from the right, quite generally caused the disappearance of small producers:

> ‘*Indeed, right-wing, like left-wing, modernization policy promotes not only the dissolution of small producers but also their increasing incorporation into increasingly distant and complex forms of control and surplus extraction’.*

In post-war decades, rural society entered, in industrial countries, a heroic era in which agro-ecological landscapes got ‘rationalized’ away. They were transformed into greatly simplified spaces befitting wholesale oil-based mechanical operation.

The ultrafast change-over in those years to a cheap-oil economy, and especially the boom in road transport, made traditional ways of performing agricultural operations unacceptable, and, to the urban-based planners at least, even ‘unthinkable’. Car and truck, with the road infrastructure, dominated the wider scene. Therefore, traditional agricultural operations were perceived as remnants of a non-modern society. Uekötter (2007 S.38) illustrates how the changeover in agriculture to oil-based, mechanical operation, was itself conducive to the loss of contact with, and expertise of the soil:

> ‘*Während der Landwirt bei der bis weit ins 20. Jahrhundert gängigen animalischen Traktion den Boden beim Pflügen unmittelbar im Blick hatte, sitzt er heute üblicherweise auf einem gut gepolsterten Traktorensitz in einer klimatisierten Kabine und dreht dem eigentlichen Geschehen auf dem Acker seinen Rücken zu*’.

From the presumption that they knew all about it, technocracy’s experts designed ‘modern’ production techniques that severed direct contact with soil and ecology.

As a result of this severance of direct contact, the growing problems, though originating in the ‘modern’ techniques, were not even noticed. To the contrary: the experts became still more confident that they ‘knew all about it’. These were heroic years (*sensu* Bulgakov 1909), with their government-directed efforts at scale enlargement in agriculture, efforts that were at the core of a faith in the constructability of nature and society that was historically unique. We still experience the flywheel effects of those years, yet our complacent and consumptive decades appear rather pale, compared with those post-war decades of faith in High Modernism. Within this 'dream of the age' (Butterfield), soil and farmer were completely substitutable by industrial nutrients and centrally devised protocols. In regard to the soil and the farmer's close care for it, we entered a half-century in which our dream-based paradigm made us miss out on most of the evidence.

> Evidently, there is no substitute for close **knowledge-from-experience**, for sympathetic understanding. That is no trivial matter. Frankl (1972) pointed to the fact that, where reductionism is allowed to 'define' man, we get entangled in nihilism. After the Holocaust experience, he realized that unreflected, reductionist parlance about 'genetics and environment' determining man, left no more freedom for human dignity than the 'Blut und Boden' rant of the Übermensch. So much had become clear from e.g. contributions to the Ciba-symposium '*Man and his future*' (1963).
> Specifically in regard to computer modelling, warnings have been sounded from the start. Simon's 1973 '*The structure of ill-structured problems*' warned that most real-life problems are 'ill-structured', that is, they develop new and unpredictable contingencies in the course of (computer-based) solution. Up to a point, Star (1983 p.207) is right when she writes, '*The process of creating well-structured problems from ill-structured ones is an essential part of scientific work*'.
> But it stands to reason that not abilities in operating the reductive methods, but abilities to re-situate the 'impoverished' results in the real-life world of the higher-order phenomenon, decide about someone being a 'good scientist'. Any clinical chemist working with e.g. histochemical techniques knows that much (cp. Horobin 1982). There is no substitute for non-reductive knowledge.

With technocracy projecting plant, soil, and man into a lower-dimensional world that it could rule, it lost the higher dimensions that are characteristic of the real world, and with that the degrees of freedom that are inherent there. So in the present thesis **we are not speaking about a lost world of colourful peasantry, but about the technocracy-induced loss of the many degrees of freedom that in the real world are inherent to agriculture** and food production. And we conclude: **technocracy rules by shrinking the world. Ultimately, it leaves no space to soil and ecology, or to human creativity to work with these resources.**

Scientist-philosopher Dippel stressed that 'aletheia', 'openness' about one's concepts and methods and their inherent pre-suppositions and limitations, is a characteristic of good science and technology. Good science and technology knows about the many-dimensional character of reality and rejects a 'one-dimensional theory of truth' (Kohnstamm 1926 p.172; refer to the integral chapter for an extensive treatment of relevant questions). Yet when the dream of technocracy induces us to delete the analysis and discussion of the reductive approaches from the account of our scientific labours, and allows us to skip the essential phase of returning to the real-life phenomena,

> '*scientific "facts" become reified and their production histories lost. Those histories are further obscured by the shorthand of presenting results, both in publication and in the process of production. Retrieving those histories, by observing the process of deletion, should provide us with some important data about the connection between work process and "facts"*' (Star 1983 p.207).

Note that Susan Star does not state that science is 'just a construct', but that a-historical science lacks the possibility of truthfull evaluation. This makes clear that the proper way to operate with reductionist methods in science is not a subject for philosophers, but for the researcher himself (of course a philosophical treatment can be valuable in its own right, e.g. Smith 1984). Reductionism within science starts ruling when & where a researcher is no more obliged (or even allowed) to evaluate his research results, in a confrontation with the higher-order object of his studies. Then, from this implicit denial of the object, the splinters that his method left are taken for the goblet. Post-war mainline agricultural research started with such a denial of soil, ecology and (historical) farming. So how could it evade to end up with splinters?

> In a way, it is easy to manipulate science. Just **delete history from science education and research**, and tell everybody that 'these are the scientific facts', and then the pupil will not even surmise that there is a wider world, and the researcher will not even know how to deviate from the prescribed path. Unfortunately, this type of manipulation-by-miseducation was/is only too often part of 'science education'. In the words of Funtowics & Ravetz (1997 p.902): '*Anyone who has studied science textbooks was exposed to the deep difference between that sort of learning and any other. In natural science textbooks, all is impersonal, and all is certain. Values are absent from view, and equally invisible are disagreement, doubt and plurality of interpretations. The textbook implicitly promotes science as 'objective knowledge'….*' More often than not, the researcher needs some thorough re-evaluation of his 'science education'!

Post-war technocracy was essentially reductionist. Its extensive institutionalization of reductionist S & T confronts us, in past and present, with big institutes with hard working experts, whose labors nevertheless are rather hopeless. Trewavas explains (1999 p.32):

> '*It is the simplicity that appeals. Any real complexities can be simply ignored or set aside for future investigation without any thought as to how they can be achieved. Concentration on one small area at a time can give the impression that, in some way, the whole will at last be understood, despite that there are almost and infinite number of small areas to be studied. By protecting the experimental system from any environmental perturbation and with control of all the parameters, the behavior can be investigated one step at a time, even if this bears little or no relation to the real circumstances under which plants grow. ..… A simple calculation indicates the likely environmental complexity involved. The plant environment can be separated in at least ten different components. If ten different settings of each environmental parameter can be distinguished, then the possible number of real environments a plant may live and survive in is* 10^{10}. *Methodical means are, therefore, unlikely to investigate this situation adequately, but the attempted investigations to understand this situation do provide for career structures, publications, and all the other trappings of present day biology*'.

This type of post-war institutional research could 'solve' anything only by pronouncing the results from its completely collapsed computer or laboratory systems valid for the real world. With its pronouncements it 'caught' economy and agriculture in a completely collapsed world of 'flatland phenomena'. Historical research is of decisive importance, if we want to evaluate the 'results' of this era, and recover space to live. But as to agriculture, there are some real barriers here: Uekötter (2007) did not exaggerate when he spoke of '*die Geschichts-vergessenheit der Agrarwissenschaften*'.

Policy-makers presumed that agriculture was at the center of technocracy: a post-war government project that enlisted a crowd of enthusiastic experts. Reductionist in their research approaches, these experts did not even have the conceptual language that was needed, to discern that their results were in need of confrontation with the real-world objects.

Why re-position your research results into the soil, when you are certain that 'soil' is 'essentially' inert solids plus mineral nutrients (in solution)?

Why re-position your results into the local ecology, when you are certain that your industrial inputs more than compensate for its former uncertain contributions?
Why bother about the displaced farmer, when your own protocols have taken over his roles (except than as a tractor driver)?

In the technocratic frame of mind, there was no need for recent agricultural history, except then as the story of ‘our wonderful progress’.
As a result, for half a century, historic evaluation was not part of the research program. Note that even the few long-term field programs were strongly constricted in their usefulness by the reductionist approaches (that left most questions unasked).
When then, in the next chapter, we will focuss at both soil and history, this will largely prove to be a new focus. As a consequence, the next chapter will be a long one.

References to Chapter 3

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4.

The peasant’s resources

Resources unknown

Post-war agricultural policy and research constitute a core High-Modernist project of governments everywhere, aiming at the progressive industrialization of agriculture. Although operations performed on the soil were part of the project, the soil itself was out of focus, except than as a recipient of irrigation and industrial fertilizer. With the changeover to the application of external, industry-derived, resources, as prescribed by distant experts, the maintenance and development of on-farm soil resources was neglected (or forbidden). Fundamental to the project was the supposition that farming was in need of a ‘scientific’ approach, the work of experts in central institutions backed up by government and industry. The farmer had to forego his traditional ways and open up to all of this enlightenment.

Note that where of old we had the ‘covenant’ of farmer and soil, this High Modernist project implemented an expert-centered system working with industrial means. Both the new expert and the system fit into the technocratic aims of the age and were for that reason considered desirable by government officials and industry managers. The great needs of the war and post-war years induced officials and managers to choose for the centralized approach that a strong policy seemed to require. That in implementing this approach, agriculture threatened to be dislodged from the soil was not immediately apparent, not the least because the farmer continued with his labors in spite of getting ever more subjected to directions from the centre. Strong policies and massive subventions made for a fast introduction of ‘industrialized’ agriculture, and the presumed success made researcher and policy maker confident that scientific agriculture had come of age.

But note that the transition was an abrupt one, initiated and imposed in a top-down way. A peculiar isolation was distinctive of this ‘expert system’ from the start: instead of carefully linking up with farmer and soil, it devised its protocols in distant centres. That makes it quite worthwhile to take a close look at its real-life connection with the soil first, and from there to probe its removal of the farmer from agriculture’s centre. Once more, the paper traces of a broad selection of research from the past will prove a great help. For though most post-war agricultural research, by far, was subservient to strong government direction, there still were some important strands of research outside it, and, as is usual with scientific research, these strands produced their own paper traces. They will help us discern the distance between the real soil and the new expert system.

I will repeatedly work with temporary conclusions on the way, but I bid the reader to consider that I will draw the final conclusions only at the end.

4.1. On (not) fertilizing the organic way

Throughout history, sustainable crop yields have always been attained thanks to some form of organic husbandry, and sustainable yield increases have followed from intensifying this organic approach. That kind of intensification is well-known, due to e.g. the yield increases in Flemish agriculture, before the advent of mineral fertilizers (Dejongh & Thoen 1999, Thoen & Vanhaute 1999). In post-war years, researchers with long experience still acknowledged that fact, e.g. Åslander (1958 p.987):

> '*Dhar (1954) on the basis of investigations and observations throughout a lifetime makes the following statements: Practical farmers in many countries prefer to grow good quality crops by the uptake of nitrogen slowly but steadily supplied by the soil humus*'.

Especially from the labour- and knowledge-intensive processes of organic manuring, it was known that agricultural sustainability always was related to the ongoing investment of care aimed at the local soil. Labor intensity and yield increases went hand in hand – with this labor intensity encompassing both qualitative and quantitative aspects. For it was only with the growth of experience-based expertise of the local resources and their maintenance, that the sustainability of higher yields was attained. If anything defines out the post-war 'Green Revolution', it is not the actual yields, but the rejection of the traditional expertise and labor-based methods to increase the yields, and the use of mineral and mechanical means in stead. This development was due to the efforts of post-war researchers and policy makers to parallel the industrial approach.

But note that sustainability was always the Green Revolution's Achilles heel: its yield increases were obtained with irrigation, mechanical cultivation, industrial fertilizers and pesticides/herbicides, and each of these aspects of its 'industrial approach' made it more fragile. Where the use of mineral-N fertilizer caused weak stems & tissues - both in a physical and in a (bio)chemical sense – lodging , drought and frost problems, and disease & pest pressures intensified. Problems of irrigation, like salinisation, had been manifest for a long time (e.g. the Punjab). The ecological unsustainability of the 'chemical approach' became abundantly manifest in e.g. Sonora and Indonesia. Mechanical cultivation and scale enlargement proved to lead to soil deterioration and erosion, a subject studied from the 1920s, and more recently systematically mapped out by German researchers (cp. Robert Bosch St. 1994).

Soil deterioration: For salinisation cp. Ghassemi et al. 1995. For secondary pests in Indonesia see Settle et al. 1996. For pesticide resistance see Clark & Yamaguchi (eds) 2002 as an example of information from the agrochemicals industry, with its narrow scope deriving from the chemical paradigm that is still dominating mainline research. See Bosch 1976 for wider background information (also his Ch.6 on political censorship of scientific publications). Agro-ecological approaches to pests and diseases require concepts that are outside main-line's chemical paradigm, even when researchers consider e.g. reduced pesticide use (Lockeretz 1991).
In regard to erosion, Bork et al. 2003 is authoritative, as is Larue 2001. Some important historical contributions: Burges 1936, Bennett1939, Jacks & Whyte 1939 (3rd impr. 1944), and contributions to Soil Science Vol.64 (1947). More recent expositions: Eckholm 1976, Helms & Flader 1985 (USA), Boardman, Foster & Dearing 1990 (Europe), Tato & Horni (eds) 1992 (Africa/farmers initiatives), Greenland & Szabolcs (eds) 1994, Napier, Camboni & El-Swaify (eds) 1994 (conservation on farm), Pimentel et al. 1995 (became widely known), Reij, Scoones & Toulmin (eds) 1996 (indigenous conservation). Lal 1998 is extensive, but is not as strong as Pimentel 2006 and it lacks the depth of Bork et al. 2003.

Yet researches like those at the International Rice Research Institute (e.g. IRRI 1997) leave no doubt that there is more to the unsustainability of the Green Revolution with its fertilizer-responsive varieties than the problems signified. To quote one of the recent accounts (Schmidt-Rohr, Mao & Olk 2004 p.6351):

> '*In long-term field trials in which initial yield levels of lowland rice approached the yield-potential ceiling, yields declined by over 35% during 20-30 years of double and triple cropping*'.

Now note that outside the IRRI fields there is nothing unusual in the yield potential not being reached (Browning 1998), but within those fields not only are all the means that high-input agricultural research can provide being used indeed, but also the labor intensity is (very) high. Now it is exactly within that optimum frame that the yield decline indicated showed up and that high fertilizer use proved non-sustainable (see also Kundu & Ladha 1995). But note that the process was hardly new, because already World War I & after had demonstrated it painfully. Because of its importance there follows an extended quote of Haselhoff 1928 (S.105 f.) - who by the way proves his point with the yield declines experienced in spite of greatly enlarged mineral fertilizer gifts:

> '*Bei dieser Bedeutung der organischen Stoffe für die Fruchtbarkeit des Bodens ist es klar, daß da, wo diese Stoffe fehlen, die Bodenfruchtbarkeit zurücktreten muß. Deshalb ist bei fortgestzter einseitiger Anwendung künstlicher Düngemittel der Rückgang der Erträge unvermeidlich. Die Ansicht, mit Kunstdünger allein in der Kultur vernachlässigte und deshalb in der Fruchtbarkeit zurückgegangene Böden wieder ertragreich machen zu können, ist falsch; in erster Linie bedürfen wir dazu der natürlichen Dünger mit ihren organischen Stoffen, durch deren Zersetzung das organische Leben im Boden ermöglicht und gefordert wird*'.
> '*Die Ertragsrückgänge durch die Einwirkungen der Kriegs- und Nachkriegszeit lehren uns, daß trotz verstärkter Anwendung von Kunstdünger in den folgenden Jahren die Erträge zurückbleiben, wenn die Bodenbeschaffenheit infolge mangelnder Bodenpflege und unrichtiger Düngung für das Pflanzenwachstum nicht günstig ist …; es ist leichter, die Fruchtbarkeit eines Bodens zu zerstören, als den Boden wieder in einen Zustand zurückzuführen, der gute Erträge ermöglicht*'.

Since the mid-1990s we became aware of the same grave problem. World cereal production is stagnant, while the surplus quantity in store is decreasing strongly (e.g. Grubben & Partohardjono (eds) 1996 p.71). Next the lack of constructive policies leads to the present shortage of e.g. rice, and to speculation with rice in which big banks and funds do participate. UN-reporter Jean Ziegler dubs the financial experts concerned 'financial criminals' (Volkskrant 25-04-08). That means, of course, that great expectations have been dashed, and that there are great difficulties ahead, because traditional approaches, with their organic soil fertility-building, have been displaced by the 'industrial' package everywhere.

Kim, Barham and Coxhead (2000) in their careful '*Recovering soil productivity attributes from experimental data*' conclude (p.251):

> '*Thus, while [mineral fertilizer] N can contribute significantly to yield in the short term, relative to the benefits offered by crop rotations it offers little to the maintenance of underlying soil productivity in the longer term. … Our results indicate that N cannot substitute for soil productivity in the long term*'.

They notice, that the widely shared assumption of agricultural economists and policy makers that '*intensive wheat production with good cultural and fertilizer practices…is not a threat to the long-run productivity of soil*' is simply wrong. What is more: it was known to be wrong already **before** the Green Revolution was pushed everywhere. A quote from the 1941 '*Forty-*

year studies of nitrogen fertilizers' (Prince et al. 1941 p.260; cp. Greaves & Bracken 1946, in Utah; and Gerzabek et al. 2001 for a more recent long-term field experiment):

> '*Except for the cylinders receiving 16 tons of manure per acre annually, the soil contained less nitrogen at the end of the 40-year period than at the beginning. The nitrogen losses from the soil averaged 13 percent under the lime and green-manure system of soil management, 25 precent when the green manure was omitted, and 35 percent when neither lime nor green manure was employed. … The organic-matter content of the soil was dependably maintained and increased only when manure was applied at the rate of 16 tons per acre annually, and when the soil management system included the use both of lime and green manure'.*

Other long-term tests had similar results, so why did leading experts in the half century since the war miss out on them?

4.2. Stagnant production - rooted in technological victory?

An example is the influential Wageningen professor Schuffelen. He put great efforts in building 'industrial agriculture', but then increasingly met opposition from the (then young) environmental movement at the end of the 1960s. Hurt he exclaims in his valedictory address (1974): '*It is no more the experts, but the so-called engaged, who factually are ignorants everywhere, who do most of the talking*'. Then after a short 'history' of mineral fertilizer he arrives at the statement (Schuffelen 1974 p.79):

> '*The history of mineral fertilizer use and the long experience gained in research and practice otherwise do show clearly that the plant nutritive function of organic manures without any objection can be taken over by the mineral fertilizer*'.

But, there is no history in his 'history', not even in the descriptive sense which is required in e.g. chemistry (cp. Gmelin's Handbuch der anorganischen Chemie). Chemical Abstracts and Biological Abstracts could have helped him out, but he did not use them. Curious, for sure, but in this respect he was at one with most colleagues in agricultural research; Bruning's 1969 overview leaves no doubt about that.

Note these were 'a-historical' times anyway: agricultural research was 'rootless', *but so were research and education in general!*

> A most curious example was the widely propagated doctrine - outside the small circle of informed chemists – that the Periodic System of the elements could be derived from the (Rutherford) model of the atom. Yet, the Periodic System has to be in place if we want to position the radioactive elements in it, an obligate step on the road to a model of the atom. Without the panoply of the analytical chemistry of the elements it is impossible to position the radioactive isotopes. That is, the introduction of a model of the atom presupposes a wide array of chemical concepts and methods which is reflected by the Periodic System. Only in the topsyturvy world of the 'educated man' of the 60s and 70s it was the other way round. It was easy to trace and mend this curious mistake: Alfred Romer's anthology (1970) of some 26 of the historical contributions from the decisive years (1896-1913) was readily available (as a Dover paperback). Yet, to the 'educated man' of the 60s and 70s history was hardly relevant: he was certain it had been superseded…
>
> These are the same years in which mathematics instruction in schools got swamped by ideas ('set theoretical') aimed at teaching the schoolchildren the manipulation of abstract structures, which would allow them to easily arrive at the desired results.

Morris Kline's biting criticism in '*Why Johnny can't add: The failure of the New Math*' (1974) was definitely justified, yet, it took years to register with the educational world which entertained no doubts at all about its topsyturvy world. Kline showed that also mathematics needs a historical approach - cp. his '*Mathematical thought from ancient to modern*' (1972).

Schuffelen and his colleagues did not doubt the task assigned to them: facilitating the triumphal march of 'modern agriculture' with the help of the industrial means that had become available. Note that the subdivision of the task in many specialist disciplines was not conducive to a critical examination of the instruction itself. That helps us to understand, in a general way, why so many specialists in so many institutions were baffled when they were confronted with severe problems. Yet, Schuffelen c.s. were qualified researchers, who had certainly done some background research. Why, then, did they still miss out on most of the evidence?

A first problem was presented by the sources themselves. It was common to fall back on the four-volume '*Literatursammlung aus dem Gesamtgebiet der Agrikulturchemie*' (1931-1939). Note that its editors had warned the reader that they had no access to important sources (e.g. Band II, S.XIII, XIV). And indeed, a quick glance shows that they did not even mention important researchers (e.g. Magrou or Molliard), or essential subjects (e.g. mycorrhizae, or organics uptake/exchange by roots). Furthermore, they do not offer the reader anything like a sound subdivision of the research field. In fact, it is a bit of a 'hodge podge', riddled with a host of omissions.

In the background was the unsatisfactory nature of much of the Interbellum agricultural research itself, as recently explained by Uekötter (2006 a&b). In those decades too it was research that limited itself to industrial fertiliser use that received most finances by far and tended to dwarf all other kinds of research. In Uekötter's words (2006b S.283/4 and S.288/9):

> '*Aber mit dem Wachstum des Düngemittelmarktes entstanden auch Großunternehmen, die der Forschung und Beratung durchaus ihren Stempel aufzudrücken vermochten. … Die wichtigsten Konsequenz der Produzentenmacht lag wohl weniger in einer gezielten Propagierung fingierter Resultate als in der Einseitigkeit, die sie in die Fortschungspraxis brachte: Während die Forschungen zur zweckmäßigen Anwendung des Kunstdüngers stets auf freundliche Unterstützung hoffen konnten, gab es kein industrielles Interesse an Studien über den organischen Dünger, auch wenn die Bedeutung des Wirtschaftsdüngers für den Humushaushalt des Bodens unstrittig war. … All dies war jedoch ein schleichender Prozess, eine Art Wissens-erosion, die nicht den Charakter einer scharfen Zäsur besitzt*'.

Specifically as to fertilizer advice Uekötter explains about this 'Wissenserosion':

> '*Der Aufstieg der landwirtschaftlichen Wissensgesellschaft war also bei Lichte betrachtet auch eine Wissenserosion: Die sehr stichhaltigen wissenschaftlichen Vorbehalte gegen allgemeine Düngungsempfehlungen wurden nach und nach gewissermasen "abgeschliffen" und gerieten in Vergessenheit, ohne dass sie je inhaltlich widerlegt worden wären. Nur so lässt es sich erklären, dass das Verfahren der chemischen Bodenanalyse zum Standardverfahren bei der Ermittlung des Düngebedarfs avancierte.'. … 'So konnte die Agrikulturchemie die Vertrauenskrise, in die sie mit den irreführenden Versprechungen nach dem Ersten Weltkrieg hineingerutscht war, am Ende zu ihrem eigenen Vorteil bewältigen. Indem die Agrikulturchemie wissenschaftliche Konkurrenten wie die landwirtschaftliche Bakteriologie … erfolgreich attackierte und zugleich dem Wünsch der Landwirte nach exakten*

> *Düngungsempfehlungen weiter entgegenkam als je zuvor, konnte sich dieses wissenschaftliche Establishment nicht nur behaupten, sondern eine hegemoniale Position erreichen, die sie für Jahrzehnte gegen Anfechtungen jeglicher Art immunisierte. Erst die Vertrauenskrise der 1920er Jahre war die Geburtstunde jener engen Allianz von Agrikulturchemie und landwirtschaftlicher Praxis, die bis in die jüngste Vergangenheit zu den Eckpfeilern des agrarischen Wissenssystems gehörte*'.

Indeed, it was widely known that e.g. the displacement of organics by mineral fertiliser had induced serious soil fertility problems, yet, the proponents of the 'chemical paradigm' did not start a re-investigation of their concepts and methods. In the tumultuous pre-war and war (WW II) years that followed, the unsatisfactory character of discussion and research was prolonged. When after the war the choices of those years initiated a strongly a-historic mindset everywhere and consequently also in research, the new generation of researchers was no longer aware of the broader discussions and did not probe behind the '*Literatursammlung*'.

> Uekötter hints at the close connections between mainline agricultural research and big industry. Reading Waeser 1932, who also treats the agricultural application of industrial fertiliser, I was indeed struck by the haughty disregard by big industry of anything in agriculture but the application of its own products. There is not even a trace of independent research comparing organic and industrial fertilizer. Big industry-related research is completely silent about the many ways in which organic fertilizers build soil fertility. Instead, it consistently speaks only about the 'equivalent' quantity of mineral nutrients that organic fertilizers will 'supply' (after 'mineralization') and stresses the uncertainties of this supply.
> Note that the unsatisfactory nature of such industry-related research was widely known before WW II. In the words of J.D.Bernal (quoted in Hofstra 1937 p.53 n.2): '*Inside industry, scientific research is necessarily valued only in so far as it reduces cost. As such it occupies a position similar to rationalization, speeding up, or plain wagecutting, except in so far as research can be used directly for the purposes of advertisement*'.

Then when we turn to the first post-war years, the dominant influence of American research in those years is common knowledge. After the massive war efforts with their accelerated research, the long-term perspective that is needed in agricultural research had not yet been regained. Just consider the parallel advertisements for 2,4-D (herbicide) and DDT of a chemical company in 1946 (Rasmussen 2001 p.311). Both were war products, the herbicide emerging from biological weapons research. As to 2,4-D we read:

> '*KILLS WEEDS! Or your money back.*
> *Spectacular new product of research and know-how of seven great companies, Weed-No-More certainly is a marvel of modern science. It's the workless way to a weedless lawn. Weed-No-More kills weeds for you… Simply spray them away*'.

As to DDT the text is:

> '*DESTROYS PESTS! Safe, sure, long-lasting. … Trust the world's largest insecticide makers to bring you such a wonderfully effective DDT formula which you can use with absolute safety*'.

The use of herbicide was promoted by *175 million* advertisements in 1946 alone (l.c. p.310). With this 'marvel of modern science' becoming thus widely known, who could still entertain any doubt? Then note that in those years the story of hybrid maize was told in a similar way (e.g. Cohen 1949 Ch.6 on herbicides and Ch.11 on hybrid corn). The near-complete lack of balance is obvious, but of course, a sense of balance shortly after such a total war was hardly to be expected.

But note there was a peculiar kind of ‘rationality’ in the air. As Smith (1991 p.98) reminds us,

> ‘*One of the great ironies of World War II was that the horror and destructive force of modern methods of warfare restored the public’s faith in the possibilities of scientific progress*’.

This was evidently ‘progress’ of a very specific kind, and alien to the local life of people and plants. Oil-based mechanization received a tremendous boost from the war and, quite likely more important still, it caught the fancy of people everywhere, also in agriculture. Wendell Berry relates his own experience (Berry 2005):

> ‘*I remember well a summer morning in about 1950 when my father sent a hired man with a … mowing machine and a team of mules to the field I was mowing with our nearly new [tractor]. … I had been born into the way of farming represented by the mule team, and I loved it. I knew irresistibly that the mules were good ones. They were stepping along beautifully at a rate of speed in fact only a little slower than mine. But now I saw them suddenly from the vantage point of the tractor, and I remember how fiercely I resented their slowness. I saw them as “in my way”. …*
>
> *We were mowing that morning, the teamster with his mules and I with the tractor, in the field behind the barn on my father’s home place, where he and before him his father had been born, and where his father had died in February 1946. The old way of farming was intact in my grandfather’s mind. … He had worked mules all his life, understood them thoroughly, and loved the good ones passionately. He knew tractors only from a distance, he had seen only a few of them, and he rejected them out of hand because he thought, correctly, that they compacted the soil.*
>
> *Even so, four years after his death his grandson’s sudden resentment of the “slow” mule team foretold what history would bear out: the tractor would stay and the mules would go. Year after year, agriculture would be adapted more and more to the technology and the processes of industry and to the rule of industrial economics*’.

And he adds:

> ‘*The tractor can work at maximum speed hour after hour without tiring. There is no longer a reason to remember the shady spots where it was good to stop and rest. Tirelessness and speed nforce a second, more perilous change in the way the boy sees the farm: Now he sees it as ground to be got over as fast as possible and, ideally, without stopping. In the midst of farming he has taken on the psychology of a traveller by interstate highway or by air. In other words, the focus of attention have shifted from the place to the technology*’.

Berry points to a fundamental shift in attention: away from soil and local ecology and towards technology. A very human shift indeed, as it was part of the ‘dream of the age’, but there was hardly anything ‘scientific’ about it.

But note that within the US the ‘dream of the age’ had decidedly political overtones, hardly less than in the USSR, for a critical stance towards all of this technological ‘progress’ was soon out of the question. I refer especially to the post-war McCarthy period, with its witch hunt of critical minds. At best, critical evaluation of ‘technology transfer’ to agriculture was equated with Amish beliefs. Yet, this very ‘transfer’ was a chief part the Marshall Plan…

Note in this connection that the critical role of Mannheim’s ‘*freischwebende Intelligenz*’ (as stressed in his ‘*Ideologie und Utopie*’) had never been broadly acceptable to the financiers of colleges and universities in the US. For faculties the danger of being accused of ‘un-American activities’ had always loomed large, and became acute especially after the war (e.g. Snethlage 1950 p. 603). As a matter of fact, faculty members got effectively curtailed by the (college/ university) administration, that maintained its supervision also after Cold War years (Lewis 1988).

> When against e.g. Snethlage's express advice – he speaks of the 'social incompetence' of the US - social sciences in Europe increasingly tried to emulate their equivalents in the US (e.g. Knegtmans 2003), they in fact copied the lack of a truly critical attitude towards High Modernity that was common at the other side of the ocean.

It will be clear by now that Schuffelen, being a child of his time, would have needed more than just 'scientific abilities' to be able to transcend his times. Furthermore, we need not doubt his idealism: he belonged to a generation that was convinced that it had the means to 'transcend' the old world and build a new one. After all, it was obvious that the US was the most 'modern' nation, capable of the most powerful industrial production. It was this nations that had won the war and now led the way with its wonderful technology…

4.3. 'The scientific basis for input-intensive cereal production is seriously flawed'

Note that the 'organic ways' that had been recommended so strongly before - in e.g. Haselhoff 1928 and Prince et al. 1941- implied

(a) mixed farming plus use of rotations with nitrogen fixing crops
(b) labor- and (local) knowledge-intensive manuring practices
(c) crop varieties adapted to the specific 'organic ways' available.

These were not really the kind of approaches enabling specialization, scale enlargement and intensification by means of fossil energy use. So when the dream of the industrialization of agriculture caught the fancy of policy makers, the 'organic ways' simply did not fit in anymore. With a fervent faith in 'constructibility' as the driving power, the dream filled everybody with great enthusiasm to enter the new era and re-design agriculture 'the industrial way'. The 'organic ways' were deemed outdated, irrelevant for the new world.

It was only after the phase of idealism had passed that people started wondering if this enthusiasm, even if regarded as highly commendable for a long time, had actually resulted in a tunnel vision. For indeed in the meantime problems had accumulated. Mineral-N proved to increase the mineralization of soil nitrogen, leading to losses higher than expected (expected from the fertilizer, plus experiments with soil nitrogen-only): the 'priming effect'. Both denitrification (losses as nitrogen gas) and nitrate leaching proved unmanageable, shattering the dream of 'constructability' that had motivated e.g. modeling researchers.

> **The priming effect and soil deterioration:** As to this 'priming effect' of mineral fertilizer on an accelerated mineralization of soil-N cp. Dormaar 1975; Kudeyarov 1992; Glendining et al. 1996; Ghosh & Kashyap 2003. For a clear example from litter studies see Vestgarden 2001. Sinsabaugh et al. 2002 provide a link to the changes in extra-cellular enzymatic activity on fertilization. Neff, Townsend, et al. 2002 demonstrate '*the acceleration of the turn-over of a broad range of plant compounds*' upon mineral-N fertilization in the soils of their study, concluding '*that relatively unaltered plant carbon resides in these soils for years to decades and then disappears as a direct or indirect result of fertilization*'.
> But that means that this mineral fertilization – in spite of extra plant growth – induces 'soil mining', the depletion of soil capital. Before them, Hempfling & Schulten 1991 found already: '*Intensive landwirtschaftliche Bewirtschaftung führt zu reduziertem Einbau pflanz-licher Ausgangsmaterialien in die organische Bodensubstanz. Dieser reduzierte Einbau vermindert die Fähigkeit des Bodens mineralischen Stickstoff mikrobiel zu immobilisieren und in verfügbarer Form zu konservieren*'.

Very recently Kahn c.s., scientists connected with the Morrow Plots (‘*the world’s oldest experimental site under continuous corn*’) arrived at ‘*an inexorable conclusion*’ which is worth quoting in extenso (Mulvaney, Khan & Ellsworth 2009, Conclusions):

> ‘*There is a prevailing view that global food and fiber production will continue to expand because of modern agricultural management systems with improved cultivars and intensive chemical inputs dominated by synthetic ammoniacal ferilizers. The use of these fertilizers has led to concerns regarding water and air pollution but is generally perceived to play an essential role for sustainable agricultural productivity, not only by supplying the most important nutrient for cereal production but also by increasing the input of crop residues for building soil organic matter. The scientific soundness of the buildup concept has yet to be substantiated empirically using baseline data sets from long-term cropping experiments. The present paper and a companion study by Khan et al. (2007) provide many such data sets that encompass a variety of cereal cropping and management systems in different parts of the world. Overwhelmingly, the evidence is diametrically opposed to the buildup concept and instead corroborates a view elaborated long ago by White (1927) and Albrecht (1938) that fertilizer N depletes soil organic matter by promoting microbial C utilization and N mineralization.* ***An inexorable conclusion can be drawn: The scientific basis for input-intensive cereal production is seriously flawed*** *[emphasis mine, J.V.]. The long-term consequences of continued reliance on current production practices will be a decline in soil production that increases the need for synthetic N fertilizers, threatens food security, and exacerbates environmental degradation*’.

That the quantitative problem shades into qualitative ones is clear where they write:

> ‘*the impact of synthetic N fertilizer is more pronounced for labile soil N than for the passive N pool’. ‘The results show a substantial decline [of the labile pool] that became progressively more serious with increasing applications of synthetic N…’. ‘There are serious implications for agricultural production and sustainability because crop N uptake is often greater from the soil than fertilizer…*’.

If we surmise that the great importance of the ‘labile N pool’ for crop growth derives from the deliverance of a complex mixture of organic compounds for root uptake (cp. last Ch.), the risks of its decline become at least understandable.

There is further evidence for the qualitative deterioration of the soil organic-N with intensive use of mineral-N. In it a relative increase in heterocyclic-N is likely.

> Haider et al. 1991, in their careful study of long-term trials at Neuhof (Germany), indicated that there is a qualitative change in the soil-N due to the influence of mineral-N fertilization. It tends to produce an increasing proportion of heterocyclic-N. Thorn & Mikita 1992 show by NMR studies that reaction of ammonia with humics results chiefly in heterocyclic forms of N. Note that these forms are normally not important (Knicker & Kögel-Knabner 1998), but with high mineral-N from fertilizer blocking normal purine/pyrimidine catabolism (Brown 1995), they can attain prominence. Cp. also Kögel-Knabner et al. 1991 for plant origins of soil organic-N, Nguyen & Harvey 1998 for intricacies of its long-term soil & sediment chemical preservation, and Stankiewicz & van Bergen 1998 for a general overview. Refer to Smernik & Baldock 2005 for problems of the NMR determinations. Parallel to Hempfling & Schulten on temperate crops, Schmidt-Rohr et al. 2004 mention yield declines in intensive rice culture, with rising amounts of amide nitrogen directly bonded to aromatic rings.

Note that the notion of 'phenol accumulation' impeding anaerobic decomposition (and therefore soil N-mineralization) as a cause of declining yields in irrigated rice (e.g. Olk et al. 2006) was not corroborated when the same researchers found that '*Anaerobic decomposition had less effect on phenol enrichment for continuous cropping without N fertilizer*' (Olk et al. 2009). Within the 'old' (mineral-N) paradigm dominating USDA and IRRI, where Olk et al. are employed, it is unthinkable that mineral-N itself can be the culprit, seeing that it is 'the only natural N-nutrient'. Impeded mineral-N delivery by soil organic matter remains as the only thinkable cause. But as soon as we introduce organic-chemical considerations, also in plant N-nutrition, we can do research that makes sense, e.g. from the known fact that high mineral-N blocks normal heterocyclic-N catabolism (Brown 1995, also Sims 2006).

All in all, both the quantitative and the qualitative changes, which are apparent under influence of the use of mineral-N fertilizer, are sufficient to speak of the **deterioration of soil fertility capital as caused by dominant 'fertilization' practices** (see further Ch.8).
This problem is compounded, in e.g. fertilizer-intensive rice culture, with fertilizer-induced plant structural problems, e.g. aerenchym deterioration. For mineral-N fertilizer interferes with secondary metabolism and lignification, and that, in turn, with aerenchym construction. As a result, oxygen delivery to the rhizosphere is diminished and soil microbial life strongly hampered (cp. Kundu & Ladha 1995).

In the long-term Rostock field trials (see Reuter 1991), after 20 years of intensive organic fertilization, continuation with 15 years of mineral fertilization indeed caused significant losses of soil organic-C. It was clear, however, that an important part of the soil capital acquired during the preceding decades was still present in the end. That is, complete soil fertility loss, due to a transfer to mineral fertilizer-only (except for crop residues), is the longer kept at bay, the more has been invested in the organic building of the soil capital, before 'mineral fertilization' took off (cp. also Russell 1957 p.26f. on long-term experiments at Rothamsted). **The 'success' of the Green Revolution with its 'mineral fertilization', may very well have been due to the soil capital that was invested before its advent**.

The assumption that 'industrial nutrients' are essential to soil fertility is wrong. It is an established fact that sustainable agriculture and food production require the *organic approach*. In fact mineral fertilizer's specific role became more and more uncertain, e.g. when we ponder Cooke's 1971 comparison of N-yields vs. N-application in England 1888 with that in England 1970. Paul (1975 p.265) summarized:

> '*in 1888 in all of England no more than 30 tons of fertilizer nitrogen were applied to a crop containing 270000 tons of nitrogen. In 1970, 400000 tons of nitrogen were applied and 340000 tons of nitrogen harvested. The total acreage was similar in the two years*'.

By then there were so many 'anomalies' that did not fit in with the mineral fertilizer paradigm that a full paradigm shift was clearly indicated (Norman 1946 is already critical of the paradigm). Yet it took three more decades for that shift to occur – and then only because plant nutrient experts from outside the institutional agricultural research circuit announced it.

Evidently post-war agricultural research & policy made some very rash statements about soil fertility and its essence, and it is important to take a close look at this period in history. As hinted at already, this is a history of ruptures, both in terms of established organic practices of old, and of important lines of earlier research. As we will see, it is especially a history in which we assumed we could effectively develop agriculture independent of the soil and its intricacies.

4.4. Organic nitrogen – a historic puzzle

> *'It is therefore concluded that higher plants can take up and utilize directly organic compounds present in soils before their nitrogen is mineralized by bacteria or other micro-organisms'* (Nature, 15 april 1933 p.535)

This Editor's summary of A.I.Virtanen's results - as presented by the latter early in 1933 to the Netherlands Agricultural Society in Wageningen and the Chemical Society in Zürich - testifies to the fact that researchers were open to the subject of organic plant nutrition in those days. For his researches in agricultural chemistry (e.g.Virtanen 1938) Virtanen received the 1945 Nobel price *in chemistry.* Hardly unexpected: he was always in the forefront, in applying new chemical methods (e.g. after the war electrophoresis in Ellfolk & Virtanen 1950).
A decade after the war, the leading (bio)chemists and microbiologists of the age contributed to the volume '*Biochemistry of nitrogen*' (Toivonen et al. (eds) 1955), that celebrated Virtanen's 60th birthday. In the meantime, results that were similar to Virtanen's had been found by many others (even by mainliners Ghosh & Burris 1950). But also the scene had changed greatly. By the mid-50s, research into organic nitrogen nutrition of plants had been dwarfed by the immense volume of research limiting itself to mineral N nutrition of plants. An author like Aslander 1958 was not unwilling to consider the 'organic view of plant nutrition' (Aslander 1958 p.985 f.), yet, as his extensive references show us, he was completely oblivious about Miettinen's and others' specific *biochemical* research into the matter.

Organic-N nutrition research – outside the agricultural research circuit:
Virtanen's work as to organic-N nutrition of plants was extended at the Biochemical Institute in Helsinki by J.K.Miettinen, whose 1959 publication is worth quoting (p.227): '*The present results clearly show that amino acids are taken up by plant roots without decomposition, and this assimilation may be regarded as a natural phenomenon*'. A similar report about amino- and amide-utilization by yeasts was published by Steiner in 1959, in the same publication of the Company of Biologists where we find Miettinen's. Two decades later Becker & Naider 1980 and Wolfinbarger 1980 reported about follow up research with yeasts and fungi in the 60s and 70s - as did Higgins & Payne on the same occasion about the trickle of research into amino acid and peptide utilization by plants. Steward et al.'s research into in-planta amino acids etc., as reported in the 1955 celebratory volume, linked up with Miettinen's research reported in this same volume. Especially in regard to the chromatographic and related methods used, this research was truly exploratory. For follow-up research cp. e.g. Steward & Pollard 1962 and Steward & Bidwell 1962. But note: institutional agricultural research did not adopt these methods, even though they were the advanced methods of those decades.

One reason was that the field of crop plant nutrition by then was under the spell of the 'quantitative approach' in linking yields and fertilizer use (Boguslawski 1958). So it seemed self-evident that the 'pure' fertilizers produced by the explosives industry, enabled a far more exact treatment of these matters than the organic fertilizers of the farmer of old. In fact, the goal proved elusive, the results even at the far more simple level of ion-exchange relationships breaking down as soon as something of the complexity of soil was introduced into the laboratory systems (Krishnamoorty & Overstreet 1950; that breakdown is general, e.g. in biological systems, Ninham 2006).

But then the introduction of computer modeling brought the promise of handling large data sets, and with that renewed determination to stick to the reductionist approach, enabling researchers to keep on some more decades in reducing soil to a provider of industrial nutrients. Enthusiasm for computer programming made up for the near-complete lack of data

in those years (Beek & Frissel 1973 p.2/3) and for the absence of anything approaching the soil (aggregate hierarchy) in the models (id. par. 1.3). Most noteworthy is the recognition of the absolute dominance of organic-N in soils even in Beek & Frissel's opening statement of their treatise (then considered path breaking): '*In the majority of soils most of the nitrogen is in organic compounds and only a small percentage is present in inorganic compounds*'
Note that they nevertheless combine this recognition with its complete neglect: they continue their enthusiastic modeling efforts as if the inorganics are a substitute for the soil-N. Furthermore they do accord a unique position in the N-cycle to soil microbes: they assume that all plant-derived organic-N makes its way through the soil microbial biomass, because plant roots are deemed inferior in capturing the 'N' that becomes available. In the models it is only the 'surplus N', which microbes disconnect from the organics they use for maintenance and growth, that comes available as mineral N.

This 'Mineralization-Immobilization-Thesis' then dominates the field until its carefully researched rejection by Barraclough in the 90s. **Yet note that it was known to be invalid before its introduction in the models.** Harmsen 1964 describes experiments with strips of ion exchange sheets located in soil that indicate:

> '*We … must presume that plant roots are able to compete to a certain degree with the microbes during the decomposition of organic matter containing nitrogen. The most striking results are those using a soil to which a surplus of ground cellulose or starch is added. In such soils during incubation* [incubation as performed in the tests for plant-available N] *no nitrogen is liberated, on the contrary they are characterized by a net immobilization of nitrogen. But the ion exchanger sheets still absorbed considerable, though relatively smaller, amounts of nitrogen, mainly as ammonium. An explanation herefore might be the assumption that a soil is always rather micro-heterogeneous…. This whole problem, however, requires further investigation*'.

Unfortunately enthusiasm for modeling, based on assumptions that had been proven invalid already, was greater than that for experimental soil investigations (Hillel and Phillip in 1991 exposed this situation). For in the 70s there ensued a true surge of modeling efforts, and all started from (cp. Frissel et al. 1981; van Veen & Frissel 1981):

(a) mineral-N as the only N-nutrient
(b) the MIT-hypothesis with its soil microorganisms bottleneck
(c) a soil that has no (micro) structure, heterogeneity/patchiness, etc.

Only during the 80s some of those researchers started to realize that 'soil' hardly played a role in their models, and they started probing what was missing (e.g. van Veen). Up to that point they had supposed that experiments with plants growing in solution culture were the truly scientific ones (cp. Barber 1986 p.184 f., p.195 f.). Using far higher nitrate concentrations than will occur in soils, for years those experiments seemed to fit in with agronomic research using always higher gifts of fertilizer.

On models in (soil) science:
In due time Daniel Hillel asked about the modeling efforts in soil science '*have we gone beyond the bounds of scientific constraint and have we been, in fact, creating fiction rather than truth?*'and then stressed the explorative character of true modeling with the words '*After all, our aim is not to wrap up all current knowledge in a final sealed package, but rather to discover the missing facts and assemble them into an expanding perception of the real system*' (Hillel 1987 p.38, 41). And in '91 he warned:
'*we now seem to suffer from … a profusion of theories and models quickly published without experimental validation*' (Hillel 1991 p.33) and added:

'*The dilemma is that, while the costs of computing have been falling, the costs of experimentation … have been rising. Consequently, more of us … have tended to view the processes of nature through the peculiarly idealized and neatly ordered perspective of the computer, thus turning our back on the exasperatingly ill-behaved and messy real world*'. And his colleague Philip (1991 p.97) stressed: '*Beyond the professionals sit the decision-makers. … Like the rest of us, they enjoy good news, and the news the messengers bring is that models, sanctified by the authority of the computer, will solve their problems. Decision-makers can thus join the throng of those ducking personal responsibility: their decisions are forced upon them by the pronouncements of the computer. Conversely, it is not unknown for an unscrupulous decisionmaker to seek out models and modelers that give the answers he wants. This, of course, requires modelers skilled in adjusting the model to yield the desired result; there is no place for the unpredictable output of the less skilled*'.

In fact also the 'microorganisms' in the models were not even a faint reflection of microbial life in the soil. Yet the fact remains that, up to the very present, all of the 'advanced' models used for legal and advisory purposes, are based on these improbable N-suppositions (a & b above) and on a structureless soil (c). Recently Johan Bouma emphasized that you can't thus build the roof if the building lacks foundations (Bouma 2006 p.23):

> '*Prominent simulation models for crop growth have very detailed sub-modules for plant physiology but a very simple soil component…. This creates unbalanced models…. This is also true for other comprehensive environmental models with rudimentary soil submodels*'.

Indeed Darrah & Roose (2001 p.363/4) admitted as to '*ecosys*' ('*The most complete mechanistic model to include both local and plant-scale processes*') that correspondence between observed and predicted variables '*was much better for plant variables than for soil variables*'.

In their short description of *ecosys* Darrah and Roose inform us that it '*includes a Barber-Cushman [diffusion to a cylindrical root] mineral nutrient submodel, a mineralization by microbial biomass submodel, and explicitly models mycorrhizae and root growth* [cylinder-like]. *It also includes a whole-plant growth model based on the functional equilibrium concept*'. Heterogeneity came only recently into focus (l.c. Pt.V) and is not part of the model. Though the enthusiasm for 'total' modeling efforts had cooled down considerably already in the 90s (e.g. Whitmore 1993) and we now see a rise of 'exploratory modeling' (see text), main-line modelers like those of *ecosys* still hold on. To that end they e.g. shy away from the experimental fractionation approach to the soil that already is obligatory since the 90s (e.g. Chotte et al. 1993, Feller 1993, Elliott et al. 1993).

As to the origins of this peculiar situation, we see that in the 40s it was especially the focus on the concept of 'soil-free crop growing' (Hoagland 1940, Davidson 1946, Robbins 1946) and its sequel in hybrid corn breeding, that made research turn away from soil fertility as some-thing inherent to soil, and use a black box delivering mineral nutrients instead. Still in the context of those years, in which soil aggregation was in focus after the Dust Bowl had demonstrated the catastrophic result of neglecting it, such turning-away from the soil had little to commend it (e.g. McCalla 1945, Martin 1945).

As a result of this factual neglect, in his 1963 '*Nitrogen metabolism in plants*', McKee could refer only to a very limited body of new research on soil and plant organic-N. If we consider the period from the 1960s to the 1990s, his was the only textbook that expounded organic nitrogen nutrition (Ch.6). His overview proves that chemically defined research into organic plant nutrition in the nineteenth century immediately followed the growth of organic chemistry. The near-complete disregard of the subject is apparently something of the post

World War II decades. Note that McKee was from Australia, where within CSIRO organic-N plant nutrition never was denied (cp. Rovira, Bowen & Foster 1983 p.80).

> **On the neglect of soil microbiology:** Note that the parlance of 'microbial mineralization' is in stark contrast with main-line's post-war neglect of soil microbiological studies, with the near-complete neglect of Winogradsky's researches the most bizarre example (his Œuvres Complètes 1949 is an eye-opener). Note that the recent growth of soil microbiological research - as linked to its judicious use of molecular biological methods - is up to the very present independent of the institutional agricultural research circuit. The same holds good for the growing attention, in Europe and the US, for mycorrhizae (root fungi), which helps us discern something of the soil micro-biological world's richness. For the surprising (non-MIT) way these researchers now picture the mycorrhiza's role in 'buffering' N-imissions into the soil, cp. Aber et al. 1998.

The government limitation of post war agricultural research, to research centering on mineral fertilizer, is only part of the story. For among the researchers themselves there was a strong preference for research that was not involved with the soil, this uniquely local entity. The real thing, so it was supposed by the vast majority of researchers, was research leading up to results 'applicable anywhere'. There were those who pointed to the inherent limitations of science & technology – e.g. Kohnstamm, Dippel, Polanyi, Chargaff – but their voices got muted by the greatly amplified propaganda of the covenant of experts and politicians in post-war technocracy. Only after an all-embracing technocracy had been institutionalized – in education, standardized methods, laws, research infrastructure, as well as in specific management structures - the caustic prose of e.g. science historian/philosopher Paul Feyerabend and science sociologist Bruno Latour made breaches in this technocracy's walls.

> An example of this prose (Latour 1984 p.247, 254/5):
> *'"Tournés vers la nature?" Que feraient-ils? Regardez-les plûtot, penchés sur des yeux d'écriture, à l'intérieur de leurs laboratoires, et se parlant entre eux, n'obéissant à d'autres principes de réalité que ceux qu'ils ont élus et ne créant de référent "externe" qu'à l'intérieur de leur monde'. 'Scolie: le prétendu mystère de "l'adequatio rei et intellectus" est toujours l'extension d'un laboratoire….'.*
> *'Prouvez-moi que cette substance, efficace à Paris, l'est aussi dans la banlieue de Ouagadougou. – Mais, pourquoi faire, puisque c'est une loi universelle?... – Je ne veux pas croire, mais le voir de mes yeux, hic et nunc. – Attendez que je construise un laboratoire et je vous le prouverai…'. 'Quelques années et millions de dollars plus tard, je vois de mes yeux dans le laboratoire flamant neuf, la preuve que je demandais. Je m'incline et me déplace de quelques kilomètres: "Prouvez-moi", dis-je, etc.'*

Our post-war High-Modernist society was technocratic to the bone and in it the omni-present complexity of the biological as well as the social world was there 'to be conquered'. There is no denying that post-war governments enabled the accelerating growth of research institutes specifically for that conquest. But there was more to it than finances or law: the strong faith in the 'limitless progress' of science & technology had convinced the great majority of the researchers of the constructability of nature & society. And exactly that made them hardly receptive to the signals that, coming from the world outside their institutions, pointed to the irreducible complexities of reality (both human and ecological reality).

> **Acknowledging limits:**
> Yet the essential limits had been visible all the time, and if not duly acknowledged the result always was and is bad S & T. For example, the analytical chemist is bound to indicate explicitly the compound & matrix dependency of his methods, cp. Sepher et al. 2006:

'…*quantitative analysis of isoflavones .. in rat plasma…*', and Delmonte & Rader 2006 p.1138, '*The majority of the methods reported in the literature have been optimized for the analysis of isoflavones in specific products and are not appropriate for the analysis of isoflavones in different matrixes*'.
The researcher who is silent about the matrix dependency of his results and about the systematic errors of his methods may be a great help in extending technocracy, but then only by allowing his laboratory 'results' to be imposed on a reality that, though suffering from it, still escapes both researcher and policy maker. In the same vein, the biochemist who does not duly acknowledge that he is 'studying life by killing it' (Chargaff), and who does not subject his artificial results to its complex living context, projects his artificial laboratory concepts & methods on a living reality he, in fact, refuses to explore in-depth. Horobin 1982 shows us, in an exemplary fashion, how to re-position the laboratory (histochemical) result in its context in cell and tissue.

4.5. A re-start for organic-N

So turning again to organic-N nutrition, it stands to reason that the recent re-direction of research into the subject came from the small band of researchers doing research into wild plants and their ecologies. That is, from researchers who, within their discipline, were aware of many of the real-life complexities of plant nutrition and ecology and among whom it was customary to discuss these complexities. But note that their main argument is both profound and simple (Aerts & Chapin III 2000, p.10):

> '***since N is typically transformed from insoluble organic N to soluble organic N to ammonium to nitrate, with some uptake of these forms by plants and/ or microbes at each step, the supply rate in any soil must be in the order: soluble organic N greater than ammonium greater than nitrate. Thus the potential of plants to absorb soluble organic N may be much more important than previously appreciated***'.

The impetus for change came especially from research into Arctic, Boreal and Alpine plants. Here also those plants that were not symbiotic with mycorrhizae (= root fungi) proved to be using organic N. As far as such use was known from mycorrhizal plants, it was depicted as something of poor soils with a poor nutrient status and therefore not relevant to high-nutrient agriculture. But now, these instances of non-mycorrhizal plants using organic-N (preferentially even) made it logical to re-consider organic-N nutrition.

From wild plants to organic-N nutrition:
For this strain of research see e.g.: Chapin III, Mollanen & Kielland 1993; Kielland 1994; Näsholm et al. 1998; Aerts & Chapin III 2000; Näsholm & Persson 2001; Aerts 2002; Henry & Jefferies 2002, 2003; Nordin, Schmidt & Shaver 2004.
Then Näsholm, Huss-Danell & Högberg 2000 took a close look at agriculturally important plant species, as did Yamagata et al. (2001). Lipson & Näsholm 2001 extended the approach to terrestrial eco-systems at large. But note that an awareness of the ability of plants to use organic-N had existed all the time with researchers conversant with wild plants and their ecologies – cp. Handley & Scrimgeour 1997. (For a related example of ecological research severing the bonds with mainline concepts & methods, see Monson et al. 2006).

Plant residues and soil organic matter deliver nitrogen compounds not so much by mineralization, but by 'organicisation' (Aerts 2002). Mineral N is formed only at the end of a long process, at the start of which soil organic matter is 'cut in pieces' (enzymatically, partly also abiotically) with e.g. soluble organic fragments as a result. Any plant that can intercept such

N-containing fragments at earlier stages is clearly at an advantage. Schimel & Bennett 2004 expound (after giving an exposition of the transition from the old to the new paradigm):

> '***Given the limitations of inorganic-N based approaches to assessing N-availability … it would make sense to move toward a technique based on measuring depolymerization [of organic soil materials]***'.

It was evident before the war already (e.g. Sadasivan & Sreenivasan 1939) that there is usually quite some organic-N available in the different strata of a soil. At present it is no longer in doubt that it is usually the predominant form of soluble N in soil and surface water.
Having recovered the concept of organic-N plant nutrition, we at once surmise that it can help researchers link up with proven farmers' practices of old, like manure preparation, legume based rotation and composting. The recent discovery of root-secretion of proteases in crop and wild species (Godlewski & Adamczyk 2007) significantly adds to our understanding of these traditional practices. There is no longer any doubt that roots of common plants, even when not assisted by microbial organisms, can use proteins as nitrogen source (Koga et al. 2001, Paungfoo-Longhienne et al. 2008). We are at last ready to ask the question of the plant's access to the great reservoir of protein-like soil organic-N.

Natural organic-N tendency. Organic N is dominant even in most rivers of a country as the USA, with its high nitrate leaching from agricultural sources (Scott et al. 2007). In boreal streams (Stepanauskas, Laudon & Jørgensen 2001), in the Amazon Basin (Aufdenkampe et al. 2001), in many forests streams and forest soils (Brookshire et al. 2005; Yu et al 2002), in wetlands (Craft & Chiang 2002) dissolved organic N dominates by far. So much so that van Breemen 2003 speaks of a 'natural organic tendency' and ascribes the high nitrate in surface waters that is often seen in industrial countries to human induced **disturbance** (also Stepanauskas 2002).
This 'natural organic tendency' had definitely been indicated repeatedly – e.g. Bhuiyan 1949, Bremner 1952 and Nommik 1967 pointed at aspects of it. The recent recognition of 'organic N' opened up horizons that had been obscured too long, and exploded the simple N-cycle concepts that had become dominant after the war (Chapman, Williams & Hawkins 2001; Smolander, Kitunen & Mälkönen 2001; Hagedorn, Bucher & Schleppi 2001). The recent discovery of atmospheric organic N deposition was helpful also, in widening our horizons, and in making our standard models more questionable (Neff, Holland, et al. 2002).

Conversely, the neglect of all of those established organic practices of the farmer by post-war research and policy is a historical puzzle. Virtanen in his book (1938) treated one of those organic practices in some depth, the practice based on the delivery of organic-N by legumes (and nitrogen fixers at large), that then is used by co-occurring non-fixers. His choice of the subject was obvious, because farmers everywhere had profited from planting fixers with non-fixers for centuries.

Virtanen's book (1938) was a milestone. He reported about
(1) organic-N nutrition of plants (2) vitamins in foods and feeds, and (3) nutrient cooperation of nitrogen fixing plants and non-fixers. We will return to these subjects shortly, but note that, in regard to transfer from fixers to non-fixers - see Virtanen et al. 1937, Wilson & Wyss 1937, Rovira 1956, Brimecombe et al. 2001 (par.4.I.D) - also the role of mycorrhizae (q.v.) has been demonstrated repeatedly - e.g. van Kessel et al. 1985, Frey & Schuepp 1993.

We see his and similar research continued after the war (Virtanen 1957, Delver & Post 1968) - only to be discontinued in the 60s. Given the decisive importance of the mixed systems indicated in traditional agriculture, this discontinuation is puzzling indeed. The more so because the budget for agricultural research by then was far higher than in earlier decades. But note: that research was financed by the government, that now entirely placed it within the confines of the newly created agricultural research circuit, which worked within the mineral-N paradigm…

> **Growing together of fixer and non-fixer:**
> Recent studies – e.g. Evans et al. 1992, Reynolds et al. 1994, Warembourg et al 1997 – re-establish the great value of this array of practices. Intercropping of beans with maize, for ages the foundation of food provision for the common man in Middle and South America, has proved to be a stable system (Rezende & Ramalho 1994), yet was discarded by the post-war Rockefeller research project that led to the 'Green Revolution'. Main-line agricultural research, following the Rockefeller research, forgot all of the millennia of rotations and intercropping with legumes etc. See e.g. Ambrosoli 1997 for an extensive (historic) overview of those subjects.
> The recent revival of research in agro-forestry brought the subject in focus again (e.g. Ogol et al. 1999). Clover-grass swards are an impressive example from temperate climates, as recent research re-discovered (Høgh-Jensen & Schjoerring 2001, Rogers et al. 2001. Consult Noble et al. 1998 for research in a widely heard objection, concerning 'acidification'. For decades main-line research caused these swards' demise, by focusing on abundant use of fertilizer – and lost the ability to maintain stable swards (e.g. Cuttle et al. 1992).
> Conversely, the need to minimize nitrate leaching led some to the rediscovery of sustainable sward management thanks to '*sustaining a large proportion of N-deficient grass in grass-clover swards*' (Parsons et al. 1991). Frequently cut swards receiving no nitrate, maintain high nitrogen fixing activity, while nitrate fertilization largely means the end of it (Davidson et al. 1990). The grasses commonly recommended for their 'productivity', grow vigorously due to high nitrogen fertilizer input, and will subsequently displace the disabled legumes (Tivy 1990 Ch.14).

Among the subjects that Virtanen was interested in, were also vitamins in foods & feeds (and food & feed quality in general); in fact Virtanen was a prime authority on vitamins. In the 40s a broader recognition wins ground that not all is well with nitrogen fertilizer use, e.g. Wittwer et al. 1945 giving '*evidence of an inverse relationship between the concentration of vitamin C in plant tissue and nitrogen supplied as fertilizer*'. So contrary to the general opinion, the proponents of 'organic agriculture' (= traditional agriculture at large) were on the right track after the war, when stressing the connection between food, health and agricultural methods.

Now we are recovering perspectives on several of the proven farmers' practices of old, we start wondering if our much-touted increases in 'productivity', which are inextricably bound up with 'sinking costs', were not largely obtained by 'driving roughshod over the soil'. Consider e.g. that the rate of soil erosion 'industrial' agriculture is an order of magnitude too high, even in temperate regions where it is hardly noticed (Pimentel et al. 1995, 1997).

The 'organic approach' in farming for sure is definitely more labor intensive than the 'industrial' version, but this is because it aims at a sustainable maintenance and extension of resources, in e.g. (1) upgrading soil structure (2) integrating with the local ecology (and diversifying it for the purpose) (3) closing nutrient cycles (4) breeding crop varieties that fit in with the local human and natural ecology. It is also far more knowledge intensive than 'industrial' agriculture, because the farmer needs expertise in each of these different realms. 'Industrial' agriculture, on the other hand, provides no substitutes, but simply skips all of this high-level labor…

4.6. Soil in/out of focus

When after the war institutional agricultural research grew with leaps and bounds, in the USA and Europe, '*the focus … was on the agricultural use of soils and how production could be maximized … focussing on fertilizer amendments and soil management*' (Northcliff 2006). Significantly, farmers and researchers noted '*structural damage to soil and associated decrease in productivity*', as is evident from e.g. the 1970 Strutt report in the UK, and yet for the next decade-and-a-half (l.c.) '*relatively little mention was made of the need to consider soil as part of the environmental system, and sustainability concerns were on maintaining yield, not the soil system*'.

Most tropical countries followed suit. But Australia was an exception. There the destruction done to the soil by irresponsible management was only too clear. In the words of a leading politician in 1949 (quoted by McKenzie 2006): '*we could not have made a bigger mess of the soil of the country if it's destruction had been carried out under supervision*'. Farmers in Australia were not in a hurry to use mineral-N fertilizer. It was already apparent that an increase in wheat yields, due to fertilizer, was at the expense of its quality (premium grade hard wheat), and caused the sales price to fall considerably. The government was willing to finance soil research, without the obligate link with industrial fertilizer that was typical of research elsewhere. As a result soil research, e.g. rhizosphere (= root zone soil) research, could come of age in Australia **institutionally**. It was within the institutional environment of CSIRO-Autralia that space for in-situ rhizosphere studies was granted, yet, elsewhere institutional environments were not that favorable. Japan with its land reform/farmers liberation under McArthur was a partial exception: for a time attention got re-focused to small farmer and soil (Hewes 1950, cp. Tsuzuki 1964).

Rhizosphere (related) studies:
After Hiltner's thorough rhizosphere studies of around 1900, significant research on the relations between plant roots and rhizosphere organisms gained ground in the Interbellum Starkey was well known, e.g. 1929, 1937; Loehwing gave a review in 1937 in which he stressed the importance of root exudates. Rhizosphere studies are continued after the war, though piecemeally: Katznelson 1946, Schmidt 1951, Schmidt & Starkey 1951.
Note further that we learn from Wallace & Lochhead that that epiphytic microorganisms had been researched for half a century at least (root-associated as well as seed- and leaf-associated microorganisms). Yet, institutionalized agricultural research marginalized rhizosphere-related studies after the war. Exception was CSIRO-Australia, with eminent researchers like Rovira, Emerson, Foster, Oades, Newman, Ladd, Skjemstad. Cp. e.g. Rovira 1956, 1965, 1969, Rovira & McDougall 1967, Rovira & Davey 1974, Forster & Rovira 1976, 1978, Rovira, Foster & Martin 1979, Rovira, Bowen & Foster 1983, Foster, Rovira & Cock 1983, Rovira, Elliott & Cook 1990, Bowen & Rovira 1991, 1999.

In Europe, even when established research institutions managed to keep political interference at bay and did some serious soil research, results were not absorbed by agricultural education at large, and not integrated in agricultural policy (France: Berthelin, Chenu, a.o.). In countries where high-ranking officials strictly supervised the new, institutionalized research focusing on agriculture's 'industrialization', with leading experts from those new institutions at their side, research focusing on the soil was, of necessity, confined to a very small group of academic researchers. In the Netherlands, Jongerius' and Bal's micromorphological research (Jongerius 1957, Bal 1973) was noticed, but not applied by mainliners. Only recently the importance of rhizosphere processes received broader recognition again (e.g. Keister & Cregan (eds) 1991), yet most attention to the subject is outside Europe (e.g. Giri et al. 2005).

A closer look at the post-war research projects that were oblivious to the exchanges of organics and (largely) to the microbiology of the rhizosphere, shows us that they all stayed rigidly within the mineral nutrition paradigm.

> To select one example of the many: Pommer (1982), in his comparison of old and new varieties of winter wheat, limits himself to mineral-N nutrition of the plants, in spite of the fact that the old landraces that he is comparing with the new varieties, had been bred and used with stable manures. Within the limits of this mineral nutrition paradigm he arrives at the superior character of rooting of the new varieties. When, in a side track to his own research, he discovers that such an old variety in a long-term trial, during which it has been manured the old way for many years, shows superior rooting, he does not reconsider his earlier results. The research paradigm he has internalized (from education and workplace) by then, leaves him no conceptual space for it!
> For within that paradigm plant nutrition equates 'essentially' with mineral nutrition, so stable manures bring no new qualities (within the paradigm they are just slow and undependable providers of mineral nutrients). And so the deviant result is meaningless to him: it can only be considered an anomaly. Pommer's research illustrates that e.g. agriculture-related plant physiology more often than not operated within mainline agricultural research's narrow paradigm. This lack of conceptual space was at least as important as any institutional rigidity limiting researchers.

A fate similar to that of rhizosphere research befell soil (micro)aggregate studies. This is the more remarkable because in the US, they received a real impetus, just before World War II, from the immense erosion problems ('Dust Bowl'), with their concomitant displacement of a great many small farmers/tenants (as vividly depicted in John Steinbeck's '*The Grapes of Wrath*').
In both Europe and the US, the pre- and post-war soil (micro)morphological studies of Kubiena (e.g. Kubiena 1938) had achieved enough fame to induce similar research by others. Yet nearly everywhere this remained only a trickle of academic research, financed because it was expected that, some day, methods would be developed that would fit in with centralized research and policies, and would be of some help in prescribing standards and protocols. But this research revealed the – locally unique - hierarchical heterogeneity of soils instead…

> **Research ruptured:**
> **c**p. for the serious start in the US Redlich 1940, Martin & Waksman 1940, Elson & Lutz 1940, Peterson 1940, Elson 1940 - all linked with Bennett 1940 – and Martin & Waksman 1941, Martin 1945, McCalla 1944, 1945. Most of this research disappears after the war – as does the attention for a healthy small farming sector in the US. In 1940 Bennett (p.446) could write still enthusiastically that
> '*Many who formerly devoted almost all their lands and their energies to the production of undependable cash crops like wheat are now supplying more of their home needs from the products of their own acres. They are developing gardens, raising poultry, increasing their livestock herds, and bringing idle lands into productive use. The net result of erosion control and water conservation work on farm after farm has been a better diversified, more self-sufficient, and more generally profitable type of agriculture*".
> But after the war US agricultural policy is dictated by the large landowner and, with its focus on the 'industrialization' of agriculture, discards e.g. legume based rotations, as well as the advanced research connected with it (e.g. Greaves & Bracken 1946). This rupture in research focus and agricultural policy, is something rooted in the war years and in no way the result of some 'evolution' (though more often than not it is presented as just that).

After the war only some of the research on soil organic matter, connected with e.g. erosion, is continued, Allison et al. 1949, Pinck et al. 1950. An older researcher like Allison maintains an awareness of the complexities of soil: note his attention to research into organics-clay interactions (e.g. that of Ensminger & Gieseking 1939, 1941). But as to USDA-directed research at large, this is completely absorbed in its 'industrialization project', with its various specialisms tailor-made for the purpose.

As the US example was followed after the war by most countries the world over, the attention for soil and small farmer soon waned. There were historical reasons for this surprising change, but note that there was no 'agricultural logic' in them: they were essentially political in character. So we had better take a closer look at that what these policies had discarded: the soil. A small number of high-quality research projects – from outside the mineral fertilizer dominated circuit - can be very useful in this respect. The next section starts with one of those research projects, and later there will be suffiocient reason to refer to those remarkable studies time and again.

4.7. Soil (and policy) paradigms

Kilbertus 1980 describes how a soil microaggregate (some tens of micrometers in diameter)

> '*was serially cut on the ultramicrotome and the resulting 450 sections examined in the transmission electron microscope*'.

It was great to discover the existence of this and other such amazing research projects, early in de 1990s. These pictures of the micro world of the soil are evidently of decisive value for education, research and policy.

For **exquisite pictures** and descriptions see: Kilbertus 1980, Foster & Rovira 1976, 1978, Foster, Rovira & Cock 1983, Emerson, Foster & Oades 1986, Foster 1985, 1988. For excellent electron micrographs of clay microstructures, range from some tens of nanometers to some ten micrometers, see Tessier 1991. Note that Foster c.s. could already build on some good pre-war research, e.g. Rossi's c.s. (overview in Rossi et al. 1936). On the aggregate hierarchy, see e.g. Oades 1990 and Oades & Waters 1991. It is dynamic, and varies with soil type and locality (cp. Jocteur Monrozier et al. 1991 and Skjemstad et al. 1994 for some well-researched examples).

And indeed, they are at the base of widely used notions of the complex hierarchical aggregate structure of soils (e.g. Tisdall & Oades 1982, Oades 1990). An aggregate hierarchy which, with its (micro)compartimentation, its physically shielded (micro)aggregates, and its (micro)pores, 'rules over' biotic and abiotic processes in the soil. A hierarchy that displays dynamics dependent upon e.g. earthworms and fungi, but out of reach of man - except that he can establish favorable conditions for the soil biota to perform their dynamic roles in the soil. Soils have their origin not in their present components: they are essentially natural bodies, not mixtures, and all of the biotic and abiotic processes that affected them in their past, co-determine them in the present. In short: soils are essentially local & historical entities (Phillips 2001), and we need that local history to understand their present character.

As Cadotte et al. 2005 stress for ecological communities: local processes and history interact, so that the 'components' that can be discerned in the present tell only part of the story of a specific community and its complexity.

An analysis of its parts is not sufficient. Likewise, as to the future of a soil, there is more than can be extrapolated from an analysis of its components. As the farmer is an active participant in connecting local soil and ecology, and has his own specific way of developing local agriculture, there is no substitute for his role in opening up the future of local agriculture. The history of an agricultural soil is also determined by **the farmer**, who **is in fact 'making soil history'** (in e.g. restoring a degraded soil) **in a locally specific way that is not accessible to distant institutes**.

In post-war decades soils were conceived of, by distant institutes, not as historical entities but as 'mixtures', and subsequently treated as such. As van Veen & Kuikman (1990 p.230) remind us, '*Traditionally, soil physical conditions have been studied in terms of macroscopically measurable properties, which were considered to be characteristic of substantial volumes of soil. Similarly, the biology of a soil has usually been described as being homogeneously distributed over large volumes of soil*'.
In spite of the fact that people had been aware of its heterogeneity and structural complexity a long time, soil was conceptually and methodically approached as a semi-technical material meeting quite narrow specifications. 'Standard' soil physical experiments, for example, were performed on sieved, dried, mixed and repacked 'soils' (nickname 'lab dust'). In the same vein 'standard' chemical tests used sieved, dried and mixed 'soil material'. As a result it came to many researchers as a shock when e.g. certain pesticides were found in groundwater under certain soils. They had not expected this at all, because, based on their experiments with 'soil material', they were certain that these pesticides would be absorbed. As Wierenga 1987 stated it (referring to the US esp. - quoted by Andreux et al. 1995 p.384): '*..although soil scientists have studied pesticides in soil for at least two decades, this has not prevented movement of some pesticides to the groundwater*'.

As to the approach to biotic processes in the soil, concepts and methods more often than not viewed the soil as (well mixed) material plus microbes plus soil solution. Consider the opening sentence of the 'Laboratory procedures' of Collins & Allison's 2002 publication on 'nitrogen mineralization', certainly one of the better publications about the subject: '*After drying, the soils were sieved to pass a 1.65 mm mesh screen, and duplicate 15 g samples of soil were combined with equal weights of 20-mesh quartz sand and mixed thoroughly*'.
Though the questionable character of this approach was evident even long before Foster's (e.g. 1983, 1986, 1988) and similar research, it is still standard in mainline agricultural research literature, in spite of the fact that it had been clear for a long time already that the current paradigm could not harbor the real soil's complexities.
Research was indeed in need of a real paradigm shift. For decades technical and laboratory concepts and methods had been imposed on the soil, approaching it as just another semi-technical material, without consideration of the very limited scope of those concepts and methods.

Research and paradigm change:
Jackson 1995, for example, referring to apparently equivalent research of Stotzky, Filip and Kubitsa on growth enhancement of soil microbes by clay minerals, which were still leading to conflicting results, concluded (1995 p.168): '*The apparent disagreement between the observations … does not necessarily mean that one set of data is 'right' and the others are 'wrong'. Probably all the results are valid within the limitations of the experimental design..*'
As a matter of fact, some researchers were always closer to their object of investigation, and quite conscious of its non-technical character, e.g. some of the researchers focusing at the interactions of clay minerals or humics with pesticides.

That is the reason that the results of e.g. Andreux et al. 1995 or Jackson 1995 can be placed, with some care, within the dynamic hierarchy of the real soil. (In fact their colleagues Schnitzer, Haider, Chenu and Monreal et al., who met with them at the same symposium, did already do that with regard to their own research). Likewise, the work of a researcher like Stotzky is rather easily thus re-positioned (Stotzky 1986 is an admirable example; note it is part of an Australian publication, again).
Evidently one cannot just discard research because it was executed under another paradigm: what is needed is a re-evaluation as well as an effort to re-position such research. Actually, such a re-evaluation can reveal, in specific cases, that certain concepts and methods that have been used under the old paradigm, largely derived from its prescribed ways of perception, and not from the physical reality that perception was supposed to depict. A paradigm change rarely allows for the measure of continuity that distinguishes chemistry's shift from phlogiston to oxygen at the end of the 18th century (Hooykaas 1952, 1971 Ch.VII)!

As to our subject, the 'discovery' of the historic-dynamic hierarchy of the soil, did explode the postwar paradigm, that much is clear so far. Schimel & Bennett 2004 summarize some urgent questions, 'invisible' under the old paradigm, that now claim attention:

1) *How are the biotic processes of depolymerization, mineralization, microbial uptake, and root uptake linked?*
2) *How important are physical and spatial processes which occur at the microsite scale, in regulating macroscale characteristics of ecosystem N cycling?*
3) *How important are roots and mycorrhizae in creating high- or low-N microsites and in mediating the biochemical/biological processes and their linkages?*

These and similar questions will require our attention in the ensuing sections.

Yet, it was not just the spatial and related qualities that were 'invisible', conceptually and methodically, under the old paradigm. For this a-historic paradigm had no place for historic qualities of *components* of soils either. In chemical terms, the paradigm neglected the multi-dimensional space of kinetically based soil species, and not only that, but when focusing on description in thermodynamic terms, it left out the conditions for such an approach. Most typically, it took the thermodynamically stable compounds as the only 'real thing', and was not aware that these so-called pure compounds reflect the (highly specific) laboratory circumstances of their preparation and maintenance (!), and not those of the soil.

4.8. The strange course of soil clays

Soil clays, for example, just like soil minerals in general (Kittrick 1977), more often than not got conceptually transformed by focusing on a supposed (but rarely present) solid-phase and solution equilibrium (cp. Ch.1). That equilibrium then seemed to allow these solids' effective replacement by soil solution concentrations of components.

Next, soil minerals got further 'generalized' by focusing on a (purportedly) broad and general description of their surface and colloid chemistry (e.g. in Gast 1977), that in effect drove out a truly empirical, descriptive approach. Similar use of illegimately generalizing theories and descriptions – giving the impression of 'final theory' where none was to be had - received an in-depth critical discussion in Ninham 1999 & 2006.

Specific ion adsorption was largely passed over, although its importance in soil clay/colloid systems (cp. Visser 1993c Ch.6&7) was evident a long time from e.g. **potassium and ammonium fixation** and delivery in soils. Selectivity of ion fixation is one of the factors entering into local specificity of the clay components of soils. As to ammonium/potassium fixation in soil clays, the complexities of this process are evident a long time (Wear & White 1951, Allison et al. 1951, Ruymbeke 1964), and guarantee that fixation and delivery forego some easy calculation (Allison et al. 1953, Barshad & Kishk 1970, Ross 1971, Lumbanjara & Evangelou 1994, Springob 1999).
Biotic factors are important in the **historic development** of soil clays, as is apparent from e.g. long-term agricultural management, Velde et al. 2003. History shows up also where clay complexes are not reconstitutable from their components (e.g. Moum et al. 1973). As to the present, biotic factors are mostly decisive in cycling of ammonium and potassium (Marzadori management by the farmer, of fixation & delivery as locally specific soil processes. Note that e.g. corn can itself obtain 'unavailable' K from soil interlayer minerals (Velde & Peck 2002). With the border between biotic and abiotic processes of fixation always locally specific and rather fluent (Trehan 1996), and clay minerals and humics participating in various ways, regularities obtained with simple systems in the laboratory are hardly relevant (Lumbanjara & Evangelou 1994). Developments in NMR-spectroscopy are a help in discerning more of the dynamics and complexity of K in e.g. soil clay minerals (Lambert, Prost & Smith 1992).

As is evident from e.g. Harter 1977, the generalizing descriptions, lacking the historical perspective, supplanted effective description of interactions in real soil. In this post-war context, in which premature generalization was rampant, the distinctly local character of e.g. soil (organo)clays could hardly come to the fore: their locally specific history that plays such an important role in the great diversity that we find in the real soil.

The generalizing descriptions were of little help anyway, in characterizing e.g. the known clay-humics complexes (Schnitzer & Kodama 1977), or in describing the K-release from soil minerals to plants (Huang 1977). Note that practical minded researchers like van Ruymbeke (1964) found a way out by employing a descriptive technique that was flexible with regard to the current theory.

Still, some limited place was accorded to soil clay studies, from the expectation that some day they would be of a generalizing help in soil taxonomy (Rothamsted). From the same expectations, also soil (micro)morphological investigations received some place in those decades (Wageningen). Now this '*ideal of pedology, geomorphology, …. , to develop generalizations that may be applied in situations where detailed examinations are not feasible*' proved simply not true to life (Philips 2001 p,.265). Quite to the contrary: it was shown that '*a detailed examination of a specific soil*' is always necessary. Yet, within the ruling paradigm there was a clear tendency to 'generalize specifics away', and to focus at equilibrium treatments, thus neglecting the infinite number of chemical possibilities stemming from the historic-kinetic origins of local soil (organo)clays.

In short, in real soils there is a dazzling diversity, and the efforts to pass it over with the help of 'generalizing' approaches was what greatly puzzled me *as a chemist*, from my first acquaintance with them. After all, from a chemical-kinetic point of view such diversity was simply to be expected:

nowhere in the soil (in-situ) we find the laboratory technicians that we need to engage if we want to prepare and maintain – at great effort – those solids in thermodynamic equilibrium (also in the solid phase) that the paradigm puts at center stage.

It was this plain neglect of the *chemical expectation* of local specifics, microcomplexity, and history that made me first wonder about the post-war paradigm (see further Visser 1993b).

An important process in soils is the intercalation of organic compounds between the thin layers of (soil) layer silicates. This process, that has obvious implications for rhizosphere processes and plant nutrition, has been evident for decades, and would normally have attracted great attention from agricultural researchers. The more so because chemically well-defined model systems have been published (e.g. intercalated cryptands and crown-ethers allowing other compound classes to enter interlayer space in turn). Complicated organomineral interlayer clays can be constructed, with compounds that are unlikely when just starting from the known properties of layer silicates sec.
Intercalation of e.g. humica and aliphatics has been researched – but mainline agricultural research hardly took notice. Likewise, it took no notice of research into the intercalation of e.g. ureum and amino acids. And all this in spite of the fact that Mortland (1970) had brought the subject of clay-organics to mainline's attention in its own Advances in Agronomy. In Europe and the US a trickle of soil clay research was financed, for a time, from the expectation that it would help in devising a system of soil classification that would allow a reductionist approach to soil. When it helped to demonstrate the non-reducible individuality of soils instead, interest waned (for Rothamsted see Loveland et al. 1999).

> Early studies on **organics intercalation in clays:**
> MacEwan 1960, Mortland 1970 (a review), Brindley & Tsunashima 1972. Some model clay studies: Casal & Ruiz-Hitzky 1986, Casal et al. 1994, Aranda et al. 1994. Intercalation of humics and aliphatics in soil clays: Schnitzer, Ripmeester & Kodama 1988 and Skjemstad et al. 1993. Specifics of aliphatics intercalation in soil clay minerals: Theng, Churchman & Newman 1986, Schnitzer & Schulten 1989, Schulten & Schnitzer 1990, Theng, Tate & Becker-Heidmann 1992, Schulten, Leinweber & Theng 1996. Intercalated aliphatics can make up the oldest part of the organic matter in a soil and can then be used for dating purposes.
> For organo-clay complexes with e.g. protein-like material see Perez-Rodriguez, Weiss & Lagaly 1977, Perez-Rodriguez & Maqueda 1991, Skjemstad et al. 1993. Cp. also studies on microbacterial (de)stabilization of micro-aggregates in soils (e.g. Chenu 1995).
> Intercalation of ureum (as fertilizer, or from urine) or of amino acids: Rausell-Colom & Salvador 1971, Berlinger 1985, Hedges & Hare 1987, Zhang et al. 1990. Related research helpful in probing the deterioration of organic-N under influence of high fertilizer applications: Vansant & Uytterhoeven 1973, Yariv & Heller-Kalai 1975. Theng 1973 & 1979 – one more impressive specimen of research from the Australian continent - is a true mine of information on clay organics, yet was & is neglected by main line agricultural research outside Australia and New Zealand.

The lack of interest from mainline agricultural research made most clay researchers focus on industrial instead of agricultural application (Jasmund & Lagaly (Hb) 1993). With Lagaly 1984 published in the Phil. Transactions of the Royal Society, it was hardly possible to miss out on the importance of clay-organics research also for agriculture, but still the neglect continued.

> Despite Lagaly 1986 & 1993, or Tennakoon et al.'s 1983 presentation of relevant methods, mainline research did without organo-clays. Mortland 1970 and the 1993 Schwertmann & Niederbudde publications focuss specifically at soil clays, yet received scant attention. So far there are only a few agriculture-related research groups that are engaged in clay studies.

It is the judicious combination/sequencing of processes that opens up perspectives that do not follow from a consideration of properties of constituents. In a pregnant way 'the judicious

sequencing of processes' amounts to 'making history': by entering kinetics, a chemist prevents the de-historizing rule of equilibrium thermodynamics.
This is no pleading for 'anthropomorphisms' in science: kinetic phenomena are abundant also outside the sphere of human influence. E.g. Harrison (1993 §4.3) asks the question '*Division of plant cells: is control kinetic, thermodynamic, or mechanical?*' and then from (especially) the path dependency of the final state proves that it is kinetic. That suffices to demonstrate that **just looking at 'equilibrium' and at the thermodynamically most stable compounds is a quaint way of studying this ever 'instable' phenomenon: life…**

'*Far-from-equilibrium thermodynamics of open systems is the physicochemical foundation of the dynamics and functions of biochemical processes inside living cells*' (Qian 2007 Summary Point 1).
Note that also structural-based specificity in living cells needs 'kinetic proofreading' to combat intrinsic noise: high fidelity is not attainable without the expense of cellular free energy. Disregard for this general requirement is still common in science texts, inadvertently suggesting that equilibrium thermodynamics is applicable where it is not. This disregard has far stretching consequences, in science education, in agricultural production modeling, etc.

Untill now agricultural research at large stayed aloof, even from soil clay research focusing at the acidification of soils and surface waters and at the role of agricultural N-emissions in it. Forest decline is an important consequence of this acidification: in spite of the current silence about the subject in politics and the media, soil deterioration under forests, with its great consequences for forest health etc., is widespread (Aber et al. 1998). Thorough soil & clay mineralogical research has been performed – and leaves no doubt about policy's obligation to act.

Acidification, the forest, the soil: One result of soil acidification: aluminium mobilized in soil and absorbed in grass cut for hay, a 'Chemical Time Bomb' (Blake at al. 1994), given its distressing health consequences. The important role of 'industrial' agriculture's N-emissions in the intensive acidification during the past decades is well documented (Blake et al. 1999, Semhi et al. 2000); they are also threatening large tracts of agricultural soil (e.g. Brahy et al. 2000). German researchers have documented the soil pedological and clay mineralogical consequences of acidification: Burckhardt 1988, Rampazzo & Blum 1992 a & b, Völkel & Niller 1993, Niller & Völker 1994, Volkel 1996, Frank 1994 (cp. also his thesis), Lang 2000. In regard to acidification/buffering see van Breemen et al. 1983, 1984. For forest deteriorat-ion at large see e.g. Wulff et al. 1996, Näsholm 1998, Gerstenberger 2000, Nakaji et al. 2001.

Our road transport technology, with its bizarre energy efficiency and air pollution, can rightly be called a 'dirty technology'. In the same vein, also post-war high-energy agriculture is greatly energy intensive and polluting. Together these two post-war technologies cause huge N-emissions, with especially the agricultural emissions leading to widespread soil and water acidification, and in high speed consuming our acid-buffering soil resources. To call such an agriculture 'highly productive' is at best self-deception, and yet that is exactly what we see US and European agricultural policy makers do.

There is one more subject where consideration of clay mineralogy is of direct relevance for policy, and that is the loss of K-bearing minerals as a consequence of industrial fertilizer use, most notably in irrigated rice. We have a parallel here with the loss of soil organic-N that is induced by our copious quantities of mineral fertilizer (see 4.1). In fact the subject could have caught the attention a long time ago, because aspects of the redox changes of soil clays under irrigation had been investigated already in the 60s, as had been the interactions of organics with K-bearing minerals. Significantly, K from manure, or from the organic matter cycle in general, is not equivalent to mineral fertilizer potassium, in interactions with soil clays, and not leading to the K-losses indicated. This once more disproves the reductionism that is at the heart of our industrial agriculture – and illustrates the sustainability of organic vs. industrial agriculture.

For the **loss of K-bearing minerals** consequent upon mineral fertilizer use, see Li et al. 2003. Redox changes under irrigation: Barshad & Kishk 1970, Favre at al. 2004, 2005, 2006. Interactions of organics with K-bearing minerals: Berthelin et al. 1979, Robert et al. 1979, Robert & Berthelin 1986. Non-equivalence of K from manures with K from fertilizer, in interaction with clays: Pernes-Debuyser et al. 2003.
For the irreversibility of structural changes at complete reduction to Fe(II) – that could be part of the soil fettility loss that we met in 4.1 - see Fialips et al. 2002. Conversely, **direct access to K** in (detrital) primary minerals and soil clays is of great importance for sustainable agriculture: Robert & Berthelin 1986, Hinsinger et al. 1991, Zubilaga & Conti 1995, Tice et al. 1995, Velde & Peck 2002. For the superior K-building properties of manure vs. fertilizer see Perenes-Debuyser et al. 2003; Mosser-Ruck et al. 2000 give some information on the dissatisfactory mineral K-building by fertilizer. Note that ammonium-fertilizer blocks K-release, Springob 1999.

Considering all of this neglect, it is not too much to state that **industrial agriculture could announce its triumphs because it was completely negligent of soil.**

Intermezzo: a chemistry viewpoint

What makes 'thermodynamic' approaches like those of Gast 1977 and of Harter 1977 differ from their chemical equivalents? What comes to mind first is, that in chemistry dominantly thermodynamic approaches have proved valuable in e.g. the high-temperature realm, where soils and soil life do not belong. But besides that evident kind of differences I think it useful to make a broader comparison still.

Consider a chemical publications of a sufficient level, Weissberger (ed) 1956, a prominent volume of its days available to Gast c.s. In a specific way, its authors stay close to their subject, in a flexible interplay of specific examples and relevant theory, in which the limits of the latter ever are evident. So A.L.Jones writes about his subject, organic liquids mixture separation by thermal diffusion in the liquid phase, after a concise historical-practical overview that includes renown researchers like Debye and Kramer: '*In the absence of adequate theory, it has been necessary for investigators to determine the range of applicability of the phenomenon by direct experiment*' (Jones 1956 p.5).

As to the theories that *are* being used, for an outsider there is often something eclectic in the choices made, that yet disappears if one realizes that authors start from the awareness that all of the various theories have their ever-present limits, known and unknown. When a method has a clear physical character - e.g. barrier separations in vapor phase as they became known from uranium enrichment, Kammermeyer 1956 - an author will be interested in any good mathematical treatment available, but never from the illusion that with it everything has been said. The practical focus of it all makes that even a subject as near to the notion of pure compounds as 'Crystallization and recrystallization' (Tipson 1956) abounds with practical examples – that show at once that '*experiment is always richer than theory*' (van Riessen).

As to analytical or preparative chemistry, here the descriptive element often is more important than anything else – as in Feigl's 1960 standard treatment of '*Tüpfelanalyse*', and in the fundamental multi-volume '*Gmelin's Handbuch der anorganischen Chemie*'. For a famous treatise embodying the approach indicated see Bock's '*Methoden der analytischen Chemie*' (Verlag Chemie, 1974-1984).

4.9. Generalizations and soil tests

Premature generalizations in effect hinder the perception of the omni-present diversity and complexity of human and natural reality. That is an important reason why introduction and use of any concept or method cannot do without an outline of their inherent conditions and limits. But note that the accelerated growth of post-war institutional research was intended to make the 'dream of the age' come true, that is, the construction nature and society from centralized research. With the whole of 'reality' supposedly subject to that endeavour, the interest in exploring limits waned. We entered the decades of grand generalizations. In a poignant way, the excessive expectations stood for intrinsically bad science. After all, e.g. the physicist or chemist who does not continually probe the limits of his concepts and methods, simply does shoddy work.

'Reality' is always made up of particulars (philosophically an ancient subject, for sure!). Even fruits or plants are always individual entities, also from a biochemical perspective (Trewavas 1999). Likewise humics are locally specific (Malcolm & McCarthy 1991), as is the soil micro-structure in its dependence on local plant growth (Babel & Krebs 1991). Indeed with soils historical contingency is so great, that '*a detailed examination of a specific soil is necessary*' and the ideal '*to develop generalizations that may be applied in situations where detailed examinations are not feasible*' is simply not realistic (Phillips 2001 p.365).

> The historic character of soils was evident a long time from the great variety of 'anthropo-soils' that was known from countries where human interference had built sustainable forms of agriculture, from the carefully inundated soils of the lower Nile, to the 'Plaggenböden' in e.g. Germany and the Netherlands. Yet, American soil classification had no notion of those anthropo-soils: '*Il est évident que le système morphologique [Américain] est né sur un continent, où la culture est récente et où le facteur homme – dans son sens pédologique – n'a encore été que peu actif*' (Edelman 1954 p.126). This is one more reason why it was an ill-fated development that the USA could put itself in a leading position in agronomy - just consider the first decades of '*Advances in Agronomy*' – although it was ignorant of carefull soil husbandry, and of sustainable agriculture at large.

There was not some 'scientific necessity', in the post-war decades, that of itself was leading to premature generalizations in agronomy and soil science. In chemistry, the difference between thermodynamic and kinetic approaches, had been an established fact for a long time, especially in analysis and synthesis. Even older is the conviction that *pure compound preparation is an art*. Moreover, the development of concepts in coordination chemistry, for the description of hierarchically organized complexes, predated the First World War (cp. Kaufmann's 1968, 1976, 1978 historical introduction and anthology). So the specifics-neglecting approach to soil was **essentially regressive**: it left established possibilities for description and characterization unused. For sure we had not some evolutionary development here, but a peculiar choice of research policy and practice, one that needs a historic explanation.

Much of the generalisation and pseudo-technical approach which characterizes post-war decades still dominates education, extension and policy. But by now, independent research strives in earnest to work within the conceptual framework of complexity and hierarchy. When e.g. Beare et al. 1995 choose for a title of their publication '*A hierarchical approach to evaluating the significance of soil biodiversity to biochemical cycling*', they mean exactly what they write: we need a hierarchical description if we want to approach the phenomena and not distort them completely. Reductionism has nothing to commend, because it collapses the soil world and suffocates soil life, both conceptually and operationally.

Note that, instead, a 'hierarchical approach' could have guided post-war agricultural research and policy all the way! Pre-war researches of e.g. Rossi and of Kubiena had already established the necessity for a hierarchical approach in soil science, and in coordination chemistry the description of hierarchically organized complexes was standard by then.

Present description of the **hierarchical complexity of soils** owes much to Foster & Rovira 1976, Hattori & Hattori 1977, Kilbertus 1980, Tisdall & Oades 1982, Foster 1983, 1985, 1988, Paul 1984, Emerson, Foster & Oades 1986, McKeague et al. 1986, Oades 1990. Van Veen, Ladd & Frissel 1984 indeed focus on the intra-aggregate position of most of soil microbial life, yet then approach it still as a soil component that can be modeled by fitting e.g. some kinetic constant(s).
It is only with e.g. van Veen & Kuikman 1990 that the **irreducibly-local specificity** of the (micro)aggregate systems in soils seems to dawn on them. Since then soil structural studies have been looked at in a better perspective, with e.g. soil-C dynamics as bound up with (micro) aggregates. Cp. Oades & Waters 1991, Waters & Oades 1991, Golchin et al. 1994a, 1994b, Jocteur Monrozier 1991, Chotte et al. 1992, Ladd, Foster & Skjemstad 1993, Skjemstad et al. 1994, Baere et al. 1994a, 1994b, Puget et al. 1995, Magid, Gorissen & Giller 1996, Balesdent 1996, Chotte et al. 1998, Schulten & Leinweber 1999, Six et al. 2000, Wilson, Paul & Harwood 2001.

It was a grave choice, after the war, to revert to reductionism, for it left no space for the perception of the reality of the soil and the farmer. Confident they were building a new world, researcher and policy maker in our epoch of High Modernity in fact collapsed Agriculture into Flatland.

Acknowledging soil as a hierarchical, and locally-unique, (micro)aggregate system, means that in the study of local C-, N- and P-cycles the application of physical fractionation methods is simply necessary (Christensen 1992, 2001). Yet it also means, that such a fractionation is never sufficient in itself - e.g. for research into aggregate and nutrient dynamics (Magid, Gorissen & Giller 1996).

But note that modeling research as a rule still sticks to the 'old' generalizations. That is first because versatile researchers, able to combine experimental methods with modeling, have become rare (Hillel 1991). And it is because policy makers expect models to be increasingly 'efficient': their High Modernist frame of reference has no conceptual space for the local specificity of soils requiring the actual application of non-generalizing methods. As Six et al. demonstrate, not even fractionation procedures can be standardized across soils (Six et al. 2000). Note that the necessity to adapt methods to the specific material at hand is well known to the analytical chemist, but then as part of a tradition that was in place before post-war High Modernism could re-define research…

Gradually we arrive at a conceptual and methodic framework that is helpful in understanding the ‘organic’ methods of the traditional farmer. Yet nutrient models that are currently still considered as advanced, embody approaches not in line with the complexity indicated, and their pools and fluxes are, as a rule, not directly linked to empirical entities like the soil aggregate fractions obtainable with fractionation methods (Buyanovsky et al. 1994, Hassink 1995). Increasingly researchers **conceptually** acknowledge the essentially (micro)structural character of soil, applying this fact to the interpretation of their results (especially the problematic ones). Yet in many of their **methods** they still stick to the old paradigm (e.g. Watson et al. 2000, Collins & Allinson 2002). The situation is a grave one, because **agricultural advice and policy got used to the ‘old’ models without considering the extent of their lack of foundation in soil reality**.

The gap between research that acknowledges complexity, and the host of **extractive soil tests** (e.g. mineral nutrient tests) which are still ‘standard’ e.g. for fertilizer recommendations, is always more apparent. Only a few researchers using such extractive tests are evaluating them as to their (lack of) validity, from the point of view of soil as a hierarchical (micro) aggregate system. In ‘standard works’ like Schroth et al. 2003 that evaluation is not yet endeavoured, the soil test are introduced ‘matter-of-factly’ instead (the single reference to light-particular-organics fractionation does not change that fact).

> Yet, all the time it was known that such **N-mineralization tests** offered some crude indication of yield potential for non-fertilized soils, but none for fertilized ones. E.g. Black, Nelson & Pritchett 1946 had a reasonable fit between the yields for unfertilized wheat and ‘mineralizable N’ when the latter value was high, but the relation became very variable when ‘mineralizable N’ was low. Significantly, yield increases due to industrial fertilizer were inversely related to the soil’s own N-supply as indicated by N-mineralization, with actual **yield suppression** quite a common phenomenon.
> Rockefeller researchers in Mexico (e.g. Colwell 1946 p.339) were familiar with those facts. So there was no reason after the war to focus exclusively on industrial fertilizer (in Australia researchers indeed did not do so).
> When after the war easier chemical methods for wider N-compound characterization became available, chromatographic and eletrophoretic methods first of all, agricultural research could have taken advantage of them. And yet it stuck to its pre-war methods…

As the analytical methods were cumbersome and the reagents in short supply, it is at least understandable that pre-war research tried to use some more easily applicable biological methods, and/or of some crude chemical ones. However, **their inadequacy was common knowledge**, e.g. in the case of the ‘mineralization’ tests for plant available N, in which, after some period of incubation, ammonium and nitrate are extracted and determined.

Van der Paauw was very open about these inadequacies, when he voiced the question: ‘*Is the assessment of the nitrogen need of a crop a gamble?*’ (quoted in Harmsen 1964). Collins & Allinson (2002), in their grassland research, arrive at a 198 weeks incubation period, so it is evident that ‘standard’ tests, that employ far shorter times, are doubtful at best. Wrily these authors conclude about grasslands ‘*They seem to defy modeling in a typical sense*’ (p.309). But then, with such tests, that are limited to the consideration of two of the end points of the multi-level process that starts from the N that is organically bound in plant & microbial biomass and in humics, it is sure that most of the information that is available (and needed) is missed. Investigations of denitrification and N-oxide formation - not part of the ‘standard package’ for e.g. advisory purposes – indicate the same complexities and, again, the lack of information from ‘mineralization’ tests. That these tests are unsatisfactory is, in fact, best proved by their history ….

The concept of hierarchically structured soils shows clearly that some specific compound in a soil extract, as well as compound classes that only are determined by extraction methods, can originate within widely differing soil (micro)aggregates (Anderson et al. 1991). In other words: such extractive tests are disregarding the hierarchical complexity of soils. That is a chief reason why

> ‘*After decades of searching for a rapid method to estimate the N mineralization capacity of soil, there is still no consistent recommendation*’ (Wang et al. 2001, 368; reason for these authors to apply soil fractionation to arrive at something better).

By not taking into account the heterogeneity and patchiness of nutrients in soils, those tests miss out on most of the real-life nutrient acquisition by roots and mycorrhizae also. Reason for recent research to focuss at a ‘*microbial scale of resolution*’ (Dighton et al. 2001). Conversely, the value of research neglecting that scale will as a rule be questionable. That disregard for hierarchical complexity is generally apparent with extractive chemical tests (e.g. for soil organic matter). As a result, none of those tests, as applied to whole soils, gives us information about the in-soil C-dynamics (Balesdent 1996).

To add to the complexity: even where fractionation is attempted, the very methods used disturb the system and its components, and can only give hints as to its dynamics anyway. Take as an example the interaction of easily available compounds from plant remains with the soil system. This in-situ interaction evidently depends on the (local-historic) dynamics within the hierarchical system. In regard to these dynamics, the knowledge of the fractions that are obtained with physical fractionation, can offer us some hints, but not more than that. For the fractionation procedures inevitably occasion an (unknown) re-distribution of exactly the easily available compounds. That is, the very methods used to gather informed about the (micro)hierarchy, cause us to loose the information that we need for the reconstruction of the interactions of the easily available compounds with this hierarchy (Magid et al. 1996).

> By now we recognize that **in regard to the soil system a reductionist research program will not do** because:
> 1) in studying the system we are bound to disturb it (there is no ‘detached observer’ here);
> 2) a specific soil is determined by its total history (it is not just a function of its present components); and
> 3) it is a living entity, with its energies and materials in a specific way in a state of constant flux (and this situation ‘far from equilibrium’ is a precondition of life).

From the account of the methods used, it will be clear that the researcher will know other things from a local soil than the peasant farming it, but also that this knowledge is not of a superior order. Both peasant and researcher face a soil that they can know only very partially, and then only by careful in-situ scrutiny. **The pretence of post-war High-Modernity, that central research could replace the traditional farmer, evidently was just that, arrogant pretension. Policies that have been built based on this pretence are bound to cause havoc**. One cannot help but wonder what caused research and policy to miss out on real-life soil, in those turbulent post-war decades, with so much pertinent information available.

4.10. Shock therapy for physiological nutrient studies

A first reason was self-suggestion: it was considered self-evident, in those decades, that the application of methods supposedly conceived after the technical and the laboratory sciences guaranteed progress everywhere. It was simply inconceivable that there was something wrong with that startingpoint: problems were there to be solved! Michael Polanyi, Paul Weiss and Barry Commoner pointed to the essential differences between the 'restricted' world of the laboratory and the 'unrestricted' world outside (Pantin 1965), but their warnings were not heeded. Especially where agricultural research was under close government direction, as in the USA and in the Netherlands, the pseudo-technical image of the soil would rule supreme until very recently (and in models up to the present).

Technocracy rampant in agricultural policy:
in the Netherlands, all of the agriculture-related research was brought under the direction of the Department of Agriculture in 1948 (Maat 2003). Next, the bizarre figure of high officials directing agricultural science, was accepted for half a century (e.g. Verkaik 1972). It is not difficult to recognize the ancient theme of the sacral(ized) king bestowing fertility and other blessings on his prostrate people…
When Schultz stated (1964 p.3) '*The man who farms as his forefathers did cannot produce much food no matter how rich the land or how hard he works. The farmer who has access to and knows how to use what science knows about soils, plants, animals, and machines can produce an abundance of food though the land be poor*', this was considered self-evident, in post-war technocracy.
For sure, historically it was completely flawed (van der Ploeg 1986), but, due to the general neglect of history, for some decades technocracy was 'self-evident', even in the judgement of the courts (Saltzstein 1994, on the USA). In short, with all balance gone, there was nothing to stop an all-out implementation of 'industrial agriculture'.

With researchers thus pre-occupied with a 'soil' that was expected to conform to laboratory approaches, the local specifics of soils and farmers could be ignored. They could be involved again when the centrally devised methods had to be introduced locally by the extension agent. Note that the phenomenal growth of agribusiness and food chains was also dependent on this centralized image, in which the local soil and the farmer were conceived of as playing second fiddle to the 'scientific' approach. This approach was first (deemed) the terrain of centralized public research, pretty soon to be 'privatized' by the agro-concern.

Some remarks on the broader framework: More broadly still, when the dominant doctrine is that (scientific) knowledge flows from taking an unrelated, distant stance, who will perceive in time that something is amiss? For decades at a stretch, Polanyi's a.o. observation that the living sciences belong to the artisanal realm, where 'tacit knowledge' and discipline-inherent standards are in need of hands-on transfer, was heeded only by some outstanding academics like Marjorie Greene and Alasdair MacIntyre. Then the newer sociology of science (Lynch, Knorr-Cetina, a.o.) rediscovered this artisanal character of science, only to experience a great deal of angry opposition from researchers and institutions that prided themselves exactly with this 'power to rule from a distance' (note they got financed from the presumption of its existence). Note that this type of research has a very short memory – forgetting not only about the everyday stumbling-along that characterizes science just like other human activities, but even about e.g. the rain making projects of the Johnson age…

Yet, e.g. the structural traits of good agricultural soil, its 'crumb structure', have been known for ages, as have a host of methods to cultivate its **essentially local character**. Paying close attention to soil structure as a local (historical) entitity, and to subjects dealing with the (micro)biological aspects of this soil, would have been the logical, ‘evolutionary’, line of research for post-war agriculture. But with history not that ‘logical’, research policy in most countries became dominated by a rather inert and static, de-historized and de-localized, concept of soil, a ‘soil’ conforming to ‘knowledge & action from a distance’. Powerful bureaucracies opted for this imaginary soil concept and took care of its petrification, in research and policy (remember Mannheim’s warnings).

As indicated before, because plant physiological research is frequently somehow connected with agricultural research, its dominant focus on mineral-N uptake is hardly a surprise (consult e.g. Runge 1983 to get a taste of the limited scope also of advanced research of those years). Still, plant physiologists outside the institutional bounds of main-line agricultural research, had some freedom to pursue non-standard questions. And so we see D.L.Jones c.s. in the 90s (re)discovering the resorption of exudated organic compounds, and subsequently taking a closer look at organic-N nutrition of plants. Others took a close look at ‘*recycling of N from plants to soil during the growing season*’ (Jimenes et al. 2002), and reviewed its significant consequences for practice and for models. Acknowledging that the plant actively participates in ‘its’ soil, researchers no longer felt at ease with models that looked upon soils and plants as passive recipients of our mineral nutrients…

But plant physiology received real ‘shock therapy’ from the application of molecular biological techniques. As in many other disciplines, these were originally applied to facilitate more ‘powerful’ ways of plant manipulation. But one of the most remarkable results proved to be the discovery of a plethora of amino-acid and peptide-transporters (e.g. Glass et al. 2001; cp. Waterworth & Bray 2006 for a thought p[rovoking review), which reduced the likelihood of their integral ‘manipulation’ down to about zero. At the same time the subject of peptide-nutrition of plants got re-introduced. Peptide transporters, like amino acid transporters, have been detected in roots also (Williams & Miller 2001). In Arabidopsis, the focus of much molecular biological plant genetics, as of 2004 more than 50 peptide/nitrate transporters and some 9 oligopeptide transporters had been identified (Lalonde, Wipf & Frommer 2004; cp. Lease & Walker 2006). This proves that former research had simply missed most of the complexity of the N-nutrition and N-transport of plants.

Peptide or nitrate? Not only has the existence of peptide transporters been demonstrated beyond doubt, but they proved in fact to be identical to the ‘low affinity nitrate transporters’ that had been found earlier (Rentsch, Boorer & Frommer 1998; cp. also Tsay et al. 2007). But then, the low affinity nitrate transport system is the one that seems to dominate maize genotypes selected to respond to high fertilizer input, as distinct from the inducible high-affinity nitrate transporter characterizing genotypes selected for low N input, e.g. traditional varieties (Quaggiotti et al 2003). In other words, with the fertilizer responsive varieties the high nitrate blocks the peptide transporters. So, is this entrance of nitrate ‘through the peptide door’ really ‘natural’ or is it, perhaps, more disruptive than constructive?

No specifying power, embarrassing results:
The crude methods that most researchers used to determine plant N-compounds, e.g. the Bradford protein determination (Copeland 1994), did not allow any real discrimination. The ‘Bradford’ misses out on e.g. peptides up till nona/ deca-peptides (Kruger 2002), yet, di- and tri-peptides (and maybe higher ones) are well repres-ented in nitrogen uptake and transport of plants (e.g. Waterworth & Bray 2006). Yet, when researchers chose to model plant N-nutrition, they used the results of research with methods like the ‘Bradford’ that were blind to chief aspects of the N-nutrition of plants.

Had they limited themselves to results obtained with real discriminating/specifying power, like the chromatographic methods that Miettinen used for analysis, they would easily have spotted the questionable character of their models. But as it was, they chose research using methods that simply were unfit to open up the reality of plant N-nutrition, and it could not become manifest that they arrived at a neat model due to extreme simplifications. So when Jones et al. (2009) in the most recent volume on modelling of plant N-uptake reviewed the sudden growth in recognition of organic-N uptake, they stressed that this far wider frame simply defies our modeling efforts (and asked, in effect, for a moratorium on modeling).

Peptide transport in organisms is not a new subject. E.g., Mellander offered some very interesting studies of '*The nutritional significance of some peptides*' (1954). Yet, research in the field came largely to a stand still, only to be rescinded after some decades. An important recent result is that the net, intestinal uptake of amino acids from partial enzyme hydrolysates, is faster and more balanced than the uptake from an equivalent free amino acid mixture (Grimble 1994 p.422, refs.).

Evidently the reductionist approach, that had dominated research for decades, and had considered it a matter of course to do the 'basic research' with amino-acids and their mixtures only, was also flawed in this context. And note that this was always the case: before the war already, Waksman had shown that most soil-N is bound, peptide-like, in soil organics, so that it becomes available to plants first in mixtures of peptide-like fragments and only after ongoing hydrolysis in amino-acid mixtures.

Peptides in (plant and animal) nutrition:
Boyd 1995, referring to Matthews' 1991 overview, points to this stand still in peptide research. For recent research see Grimble 1994, Boyd 1995, Parker et al. 1995, Leibach & Ganapathy 1996, Grimble & Backwell (eds) 1998, Webb Jr. 2000, Brandsch & Brandsch 2003. One aspect of this slump is that nutrition research, both animal and plant nutrition, for some decades turned away from chemical speciation with the help of chromatographic, electrophoretic a.o. methods, in spite of the fact that this speciation had been introduced after the war by Virtanen a.o.
That choice for crude, non-discriminating methods instead of speciating ones, and that in spite of the latter's wide availability, was noted by others also, e.g. Reeves III & Francis 1998. It is another aspect of the 'enclosure' of agricultural research, and much of nutrition research, in a narrow paradigm that needs a historical and institutional explanation.
And there are more of such aspects. One pertains to another field of research in which Virtanen, up to an old age, was a leading investigator: that of maintenance of ruminants of a reasonable level of productivity on a non-protein-N diet (see Virtanen 1969). The loss of such research, the discontinuation of chemically discriminating methods, and the neglect of peptide uptake studies, might all be aspects of the same 'enclosure' - with similar institutional backgrounds.

Research that could well become of decisive importance for plant nutrient studies is the research into the intestinal and renal peptide transport system that is responsible for the uptake of di- and tri-peptides especially (Terada et al. 2005; Brandsch et al. 2008; Gilbert et al. 2008; Quandeel et al. 2009). This transport system recently proved to have a very broad, yet stereo-selective, substrate sel-ectivity. The number of substrates for this 'PEPT1 transporter' is likely to be greater than 10.000 (Ganapathy et al. 2001 p.384), very different from the number of substrates for the amino acid transporters that have been identified to date, and it forces us to reconsider the fundamental concepts of organic compound uptake and transport by organisms. (Substrate specificity of PEPT1: Doring et al. 1998b, Daniel 2004, Rubio-Aliaga & Daniel 2008).

In regard to plant research, the recent recognition of peptide transporters was an important reason for researchers to re-focus on organic-N nutrition of crop plants (Yamagata et al. 2001, Miranda et al. 2003, Persson et al. 2006).

Peptide pransporters in plants incl. roots: Stacey et al. 2002, Stacey et al. 2006, Osawa et al. 2006, Waterworth & Bray 2006, rentsch, Schmidt & Tegeder 2007, Komarova et al. 2008.

The growing awareness of in-planta peptide transport coincided with a renewed attention for in-planta N-cycling, another aspect neglected by most models of crop N-nutrition. Stated positively: research can now study plant and soil as active participants instead of as docile subjects (their role according to the centrally devised protocols). Evidently organisms - and we have no reason to exclude plants - are far more versatile than we have ever thought. We quite likely missed out on what could very well be some chief modes of interaction of plants with the rhizosphere. We are in for a very different paradigm, one that allows us to see far more, and makes us far more modest about our capabilities.

On **root exudation & (re)uptake of organic compounds** see e.g. Jones & Darrah 1992, 1993a & b, 1996, Owens & Jones 2001, Dilkes, Jones & Farrar 2004, Nardi et al. 2005, Jones, Nguyen & Finlay 2009 (uncertainties defy modeling). In spite of research like Mellander 1954, research into peptide uptake and transport stayed at a low ebb for decades. As to micro-organisms there was more of a steady progress; for an interesting study in this field see Smid 1991, and consult Payne & Smith 1994 for the progress made in the 1980s.

4.11. Mycorrhizae

Another line of research leading to the recent paradigm shift, concentrated on the N-nutrition of mycorrhizal plants. Now most plant species are normally living in symbiosis with ‘root fungi’, mycorrhizae. The arbuscular mycorrhizae is the most frequent type, and offers to the plant a great extension of its root system (up to 50m of mycorrhizal hyphae per gram of soil).
It was Frank, the first researcher to study those symbioses with ‘root fungi’ extensively, who, in a 1888 publication, assigned the ability to mycorrhizae to transfer organic N from e.g. litter to the plant. After World War II, Melin c.s. established conclusive proof of such transport, with the help of labeled nitrogen, for a specific group of mycorrhizae. In later decades, it was first of all Read and his colleagues, who documented organic-N acquisition (also with extra-cellular proteinase), and its transfer to the host, by mycorrhizae. This transfer of nitrogen to their host plants was definitely demonstrated by recent reserach.

Mycorrhizae in focus:
Consult Smith & Read 1997 for the incidence of mycorrhizae and for the length of hyphae indicated. For Melin’s proof of mycorrhizal N-transport see Melin 1953, Melin & Nilsson 1953. For short overviews of the work of Read c.s. see Allen 1991 p.112 f. and Ahmad & Hellebust § VI. The mycorrhizal N-acquisition and -transport is no more disputed (cp. Leigh et al. 2009); it is greatly extended by way of mycorrhizal networks interconnecting different plants – Giovanetti et al. 2004, Simard & Durall 2004 (review). For a wider array of mycor-rhiza studies important for agriculture cp. Crititcal Reviews in Biotechnology 15(3/4) (1995). For more on mycorrhizal N-transport see e.g. Govindarajulu et al. 2005.

Yet this hardly influenced main-line agricultural research. To the contrary, during its post-war accelerated institutional growth this research circuit paid no attention to the mycorrhizal symbiosis, in spite of e.g. Melin's review (1953).

> And yet, its great importance for agricultural crops could easily be traced:
> - Magrou stressed it in his excellent contribution to the First International Congress of Soil Science in 1927 (as did Rayner 1927)
> - Smith 1948 and Harley 1952 in their contributions to the Annual Review of Microbiology pointed to this connection again (and Smith referred to a 1944 publication of Magrou in the *Comptes Rendues* of the French Academy).

For half a century, main-line breeding and crop nutrition research projected a world in which mycorrhizae could conveniently be forgotten. And indeed, the practices of 'industrial' agriculture disrupted these symbioses…
Still, when the symbiosis is not disrupted, most plants by far are mycorrhizal, and have always been so, from the very start of terrestrial plants, as we know from the fossil record. Moreover, it became very clear that plants in widely different ecosystems are mycorrhizal.

> Meyer 1966 and Björkman 1970 show us that post-war agricultural research missed out on a long list of pre-war records, also about the close geological connection between terrestrial plants and mycor-rhizae. See Morton 2000, Brundret 2002, and for some striking examples from the fossil records Remy et al. 1994, Kovacs et al. 2003.
> Some of the more recent publications focussing on the agricultural importance of mycor-rhizae are: Mikola (ed) 1980 Pt.V, Mejstrik (ed) 1989, Barea et al. 1993, Wright & Millner 1994, Barea & Jeffries 1995, Hooker & Black 1995, Schreiner & Bethlenfalvay 1995, Brundrett et al. 1996, Raman & Mahadevan 1996, Harrier & Watson 2003.

> **Mycorrhizae in biocontrol, and general literature:**
> Josef & Sivaprasad 2000, Sharma & Adholeya 2000, Whips 2004, Demir & Akkopru 2007 are some of the array of publications on the role of mycorrhizae in biocontrol of soilborne plant diseases. Publications of general importance: Allen 1991, Smith & Read 1992/ 1997, Read et al (eds) 1992, Varma 1995, Varma & Hock (eds) 1995, Harrisson 1997a & b, Mackenzie & McIntosh 1999, Hodge 2000a & b, van der Heijden & Sanders (eds) 2003, Leake et al. 2004. See Brundrett, Murase & Kendrick 1990 for material (also) on specificity; cp. also the review Smith & Smith 1997. For diversity ánd specificity in a grassland ecosystem cp. Vandenkoornhuyse et al. 2002.

From the accounts in Harley 1959/1969 it is evident that

(a) there was great methodological progress from the 1920s to the 1940s (as exemplified in the work of Jones vs. that of Bjørkman, with the latter and that of e.g. Nicholson in the 50s and 60s being definitive in character)
(b) the mycorrhization of agriculturally/horticulturally important plant species was consistently demonstrated (e.g. the work of Mosse)
(c) as was the mycorrhizal condition of plants in all of the different ecosystems investigated (e.g. the work of Boullard)
(d) the contribution to disease resistance was known from striking examples (e.g. resistance of mycorrhizal *Pinus* sp. to *Phytophtora cinnamomi*, as demonstrated by Marx & Davey)
(e) diversity of mycorrhizae in a soil, and the health of the plant stand they support, had been demonstrated as positively connected (from Melin in the 20s to Gobl in the 60s; van der Heijden et al 1997a/b re-emphasizes of the same relations)

(f) genetic variation, local variability, and adaption to local circumstances of mycorrhizae, were known to be quite important (Mosse, Linnemann; Eason et al. 1999 is an example of contemporary research)
(g) high mineral nutrient concentrations, especially from mineral fertilizer additions (esp. nitrate), were known to be detrimental to well-functioning mycorrhization (consistently demonstrated before and after the war; for a recent account see Burrows & Pfleger 2002).

The neglect of mycorrhizae, that was nearly complete with post-war institutional agricultural research in Europe and the USA, did not stem from an evaluation of the relative importance of mycorrhizae, but from the exclusive focus on mineral fertilizers, and from the wider research goal to construct an industrial agriculture. For years only the Organic Farming Movement (e.g. Sir Albert Howard) paid attention to mycorrhizae in crops. But true to its calling, the constructs of mainline research were developed in a 'laboratory setting' where inert substrates plus mineral fertilizer solution substituted local soils and circumstances. In other words, they had their incommensurability with real-life ecosystems built-in, a.o. because these are always locally specific. So we wonder if the ongoing absence of the subject of mycorrhizae from mainline agricultural research journals maybe indicates that mainline's constructs are inferior from the point of view of mycorrhizal symbioses and their use in agriculture?

Note especially that mycorrhizae have a close relation with diversity and productivity in real ecosystems:

> '*Mycorrhizal fungal diversity determines plant biodiversity, ecosystem variability and productivity*' (van der Heijden 1998a & b).

The implications of this relation are many, but until now, mainline agricultural research and policy did not respond.

> Their focus at genetically manipulated crops is certainly not part of the response that we need. GM crop constructs like *Bt*-corn are thwarting mycorrhizal symbioses (Castaldini et al. 2005), maybe because chitin-decomposing fungi are stimulated (Icoz et al. 2008 p.651). The agroconcerns, that promote the genetically manipulated crops, promote reductionist research approaches, because their existence depends on 'power wielded from the center'. That causes the symbioses of plants and mycorrhizae, that are always local, to be out of their reach.

As with organic-N nutrition, also the re-discovery of mycorrhization came from outside the mainline agricultural research circuit, in Europe and the USA. In post-war decades, mycorrhizae studies had been kept alive in at least some forestry schools (e.g. Meyer 1966), with important concepts developed there. One of these is, that of the closure of nutrient cycles in the soil is due to to mycorrhizae, and is opposed to less tight 'cycles' where mineralization dominates (leading to leaching, denitrification, etc, Vogt et al. 1991). When it became increasingly evident that mycorrhization and organic-N nutrition of plants are coupled, it became evident also why mycorrhizae are so important in closing nutrient cycles. In ecosystems, mycorrhizae are mosttimes preponderant, and mineral-N concentrations are mostly low. That is an important reason why the relevance of mineral-N nutrition for ecosystems is increasingly questioned (e.g. Leake at al. 2004).

> **Mycorrhizae in ecosystems:** Authors of general treatises (e.g. Allen 1991) also discuss occurrence and roles of mycorrhizae in ecosystems. Specific examples: Eason et al. 1999 (grass lands), Herman 2000, Medina et al. 2004 (deserts/desertified regions), Ananda & Shridar 2002, Sengupta & Chaudhury 2002 (mangroves), Kowalchuk et al. 2002 (sand dunes), Harrington & Mitchell 2002 (karst).

Mycorrhizae with **large spores**, often the most valuable ones, increase with plant diversity in grass lands (Burrows & Pfleger 2002), and so are doubly at a loss in our high-fertilizer, low-species grasslands. Maybe it is a loss that is general to our ‘industrial’ agriculture, Dodd & Jeffries 1986, Hooker & Black 1995 VII.B, Scervino et al.2005. For mycorrhizae in the in revegetation of desertifying regions (in e.g. Southern Europe) see Azcón & Barea 1997, Requena et al. 2001, Palenzuela et al. 2002, Medina et al. 2004. Due to the ecosystem adaptation of mycorrhizae, the focus must be on ‘native mycorrhizal potential’ (Caravaca et al. 2003; for impressive examples, see Caravaca et al. 2004), See Kaldorf et al. 1999, Hildebrandt et al. 1999, Enkhtuya et al. 2001, Dodd et al. 2002 for mycorrhizae in restoring ‘man-made deserts’.

Mycorrhizae play a primary role in the disclosure of minerals for nutrients. This role was re-discovered recently, as was their role in the direct acquisition of organic nutrients from ‘inert’ soil organic matter (Olsson et al. 2002 and refs.), and is now again a subject of active research. Besides this *intensity* of exploration its *spatial extent* is important: mycorrhizae allow their host plants a wide extent of soil exploration. They are, as a rule, valuable partners of nitrogen fixing plants: there is a ‘symbiotic triad’ plant/N-fixers/mycorrhizae (it is common with e.g. clovers). The importance for agriculture is evident – and enables us to reconsider the place of the fertilizer industry in agriculture and food production.

As a rule, the abundant use of fertilizer, introduced by post-war main-line agricultural research, disables both mycorrhizal and nitrogen fixation symbioses. Even our atmospheric nitrogen deposition disturbs mycorrhizal organic-N use (Lilleskov et al. 2002, Erland & Taylor 2002/2003). Due to its neglect of mycorrhizae, and its careless approach to biological nitrogen fixation, mainline research refused the farmer access to nutrients present in soils and in on-farm materials, and thus made the excessive use of fertilizer a ‘necessity’. In regard to biological nitrogen fixation, the renown Winogradsky had publicly warned about these negative developments, at two international congresses in 1927 (Winogradsky 1927 a & b).

In spite of those warnings, **it is exactly the replacement of freely accessible local nutrient capital by expensive industrial fertilizer from abroad that we see after the war**. In important ways, it was a forced development: it was forced on the farmer, history leaves no doubt about that. And yet, when problems like nutrient leaching ensued, the researchers and policy makers concerned pinned the responsibility solely on the farmer.

Intermezzo: comparing agricultural and transport policies

Apparently the same logic applies here as in our high-modernist transport economy, that was equally promoted by government. For although the (technically) obligate reaction to transport problems would be **minimalization of transport**, ours was to strive for its unbridled expansion. When the problems became unmanageable, the doctrine of the technocrats encouraged us to do the wrong thing: constructing more and broader roads.

Likewise, we responded to the problems of high-fertilizer agriculture, that showed up in the 50s and 60s, with increasing the application of fertilizer still. Apparantly from the ‘certainty’ that problems would soon be solved. As to the need for a paradigm shift in plant nutrition, this was denied vehemently (Schuffelen c.s.), and received no room institutionally. The same story as with car and road transport.

Car transport as another paradigm promoted by government:
Jane Jacobs became widely known for uncovering the lack of foundation in doctrines of the transport discipline (that had become part of post-war technocracy). Cp. '*Science abandoned*' in Jacobs 2004 for an account, and see her famous '*The death and life of great American cities*' (1961). See also Schumacher's succinct remarks (e.g. Schumacher 1975).
Surely there are many problems with the transport discipline's paradigm: Al-Wattari 1980, Ling 1990, Hassan & Duncan 1994, Koshar 2001, Roth 2004, Chick 2006 treat some of the decisive historical issues; Commoner 1972, Connelly & Perlman 1975, Melman 1975, Wouk 2001 and Wells & Orsato 2005 give a critical analysis of 'car and oil technology'; Flink 1990 and Ling 1990 integrate many issues in their historical works; Pearce et al. 1993, Maddison et al. 1996, Gudmundsson & Höjer 1996, Low & Gleeson (eds) 2003 and Tolley & Turnton (eds) 2001 again treat other issues.

Continuation

Evidently, neither in agriculture nor in transport sticking to the wrong doctrine won't help us in getting down to earth again. But
re-integrating mycorrhizae in breeding and agriculture is a first and major step in getting down to earth again with our food & feed economy. It is a major step both in re-opening the farmer's access to rich soil resources, and in pushing back our post-war high-modernist project of industrial agriculture.

Crops depend on mycorrhizae for explorative abilities, cp. Martin, Perotto & Bonfante 2001.
'Mineral processing': Choudhary et al. 1995, Whitelaw 2000, Landeweert et al. 2001, Lower et al. 2005. 'Processing' of organics, Finlay et al. 1991, Hodge, Campbell & Fitter 2001, Jentschke et al. 2001, Lilleskov et al. 2002, Olsson et al. 2002, Cairney & Meharg 2002.
Note that the organic pollutant remediation capabilities of mycorrhizae - e.g. Meharg & Cairney 2000 – are a related subject.
'Triadic' symbioses, those of mycorrhizae with nitrogen fixing microorganisms and plants, as well as with other plant growth enhancing bacteria and plants, are able to build free resources for the farmer in the present:
see Barea & Azcón-Aguilar 1983, Berthlenfalvay 1992, Elgala et al. 1995, Biro et al. 2000, Muthukumar & Udaiyan 2000, Tsilli-Michael et al. 2000, Feng et al. 2003, Porcel et al. 2003, Goigouchea 2005, and esp. Barea et al. 2005.
As to the disabling influence of post-war breeding and practice, see Azcón & Ocampo 1981, Erland & Taylor 2002/2003 p.181 f., Jansa et al. 2006 p.94 f. Vance 2001 is one of the authors reminding agricultural research of the need of a return to the 'free' resources.

Before we leave the subject of mycorrhizae (temporarily only), a few more remarks still on the roles of mycorrhizae in soil aggregation.
Some of those roles, especially the role of the great length of hyphae in aggregation, have been studied for decades (Tisdall & Oades 1980, Miller & Jastrow 1990). Still quite a surprise was the discovery that a specific mycorrhizal protein exudate often plays such an important role in (short term) aggregation (Rillig et al. 2002).
But if we learned one thing, it is the great diversity of soil processes (biotic and abiotic) that contribute to the different aspects of aggregation. The microbiota accompanying mycor-rhizae contribute significantly (Rillig et al. 2005), as do soil microorganisms in general (Chotte 2005).
And, the aggregating ability of mycorrhizae is closely related to rhizodeposition by roots and mycorrhizae (Jones 2004).
There is a richness here that implies a large measure of redundancy, and it is that redundancy that makes the utter neglect of e.g. soil aggregation by industrial agriculture a 'possibility'.

For now even a grave neglect does not lead immediately to a collapse. Yet soil clay deterioration under declining forest, as well as organic-N deterioration and K-mineral loss under irrigated rice, are signals that we cannot just go on consuming buffer capacities.
And of course: the soil sealing and compaction that is widely experienced in mechanized agriculture indicates that, in regard to soil aggregation, we transgressed some important limits, right at the start of our 'industrial' agriculture. From that time on we also disposed of most of our farmers. **So did we, in fact, take the land away from those who alone could maintain and build the soil resources?**

4.12. Industrial agriculture?

At this point some objections are definitely being voiced:

> '*Is not our time, with its great advances in nanotechnology, perfectly able to develop a high-tech agriculture too? And are we not in fact experiencing accelerating developments towards such a precision-agriculture?*'

Here is not the place to enter into an analysis of nanotechnologies. Still the reader is well advised to take note of the fact that most of the references to those technologies have a slogan-like character. For sure, there are critical analyses by independent experts, but their accounts were swamped by publications that at times seem to preach some 'nano-religion'. As it is, even at the most mundane level, that of the aging of CDs and the like, we find that comprehensive analyses are scarce, let alone that such analyses would come to the notice of policy makers or of the broader public.

So much is clear, that some suggestive analogy will not do, to answer the grave question that confronted us at the close of the last paragraph. At the heart of this suggestive analogy between nanotechnology and precision agriculture, there is this emphasis on the supposed superiority of post-war technology, as compared to anything in crafts or agriculture of former times. That means, that a disproval of the proposed use of nanotechnology as 'Dutch drops' will not do, for then the argument will shift to some other specimen of our supposedly superior technology. Evidently we need a more general approach: it is only by taking a close look at *industrial versus agricultural modes of production* that we can hope to find a more final answer to the objection.

> **Analysis of technology:** Most technology analyses focus not so much at technology itself as well as at technocracy. The reasons for that focus are made explicit in e.g. Jacques Ellul's works but our question pertains to technology 'from the inside'. Here van Riessen's analyses in his monumental '*Filosofie en techniek*' (1949), further developed and applied in e.g. Schuurman 1972 and Strijbos 1988, is of value, as is (the wider ranging) Vanderburg 2000. These authors start from a close acquaintance with technology-as-it-is, where limits are always specified, e.g. the mechanical technology that is the subject of Kals et al. (red) 1996, Matek et al. 1996, Tschätsch 1997. Technological design-from-respect (for humans and ecology) is the focus of van Eijnatten (red) 1996 (cp. Pacey 2001). Important from a distinctly technical point of view is also Schumacher (e.g. 1973, 1979) and his school. Other valuable studies are e.g. Ihde 1983, Higgs et al. (ed) 2000, and Doppelt 2001. Cp. Schumacher (l.c.) and Thompson 2000 for links to agriculture.

In industry, as opposed to artisanal production, we always are dependent on keeping variation and heterogeneity of our energy & materials flows within narrow and closely specified bounds. That is the other side of a choice for a high-flow technology: our 'industrial productivity' cannot be attained, but within a very elaborate institutional and resource framework, that guarantees the high flows of rather uniform materials and energy. Without that ‘organized uniformity’ the production process would have to be adapted to fluctuating (and often low) supplies of energy, and to each individual sample of materials. Industry would immediately loose out to artisanal production, where the craftsman with his expertise can shortcut the elaborate procedures that our knowledge-poor (!) industries need to apply when faced with shifts in energy and/or material.

Of course some variation and heterogeneity will always be present, also in industrial flows: in industry there is a continual need to monitor materials and energy flows só that containing the variation, by coupling forward or backward, becomes possible. Significantly, the materials or energy characteristics to be sensed relate to the factory process, and only in a distant way to the original feed stream’s ecological origins, or to the public’s use of the end product, or to the creative level of the laborer’s task. But note that the process requirements as narrowly conceived are at one with these other three aspects: only these four together decide about the quality of the technical process.

Technology with a human face:
Fornallaz (ETH-Zürich) quotes from his teacher prof. Eichelberg’s farewell address (1960): ‘*Das Ziel der Technik …. muss, ausser technisch und ausser wirtschaftlich, auf den Menschen hin gelegen sein. Und dabei geht es … letztlich um die Bewährung unserem Menschsein gegenüber’. ‘Wohl aber gehört zu unserem Menschsein unabdingbar das Recht auf Sinn der Arbeit’.*
‘Eine Arbeit ist dann sinnvoll, wenn sie die beiden Grundelemente und zugleich die bauenden Potenzen allen technischen Schaffens in sich vereinigt: ***persönlich schöpferische Initiative*** *und* ***gemeinschaftlich helfendes Zusammenwirken****. Die beiden Pole der Kultur, Persönlichkeit und Gemeinschaft, finden ihre Erfüllung auf dem Boden der Technik als* ***Schöpfung*** *und* ***Dienst’*** (Fornallaz 1975 S.254/5).
Linking with e.g. energy in industry, Schumacher (1975 S.151-3) explains: ‘*Grossindustrie mit Massenproduktion ist sozusagen geronnenes arabisches Öl; sie hat also keine Zukunft. Anstelle von Massen-produktion brauchen wir Produktion durch die Massen. Diese ist nur durch dezentralisierte, weit verstreute Kleinindustrie möglich, und nur diese hat Zukunft*’.
‘*Das übergeordnete Ziel unserer Bestrebungen ist die* ***Reintegration des schöpferischen Menschen in den Produktionsprozess****. Wenn man die technische Entwicklung der letzten hundert Jahre betrachtet, stellt man fest, dass der schöpferische Mensch den Produktionsprozess entfremdet worden ist*’.

That only an integrative approach makes ‘good technology’ is acknowledged in e.g. designs for green & clean production. For sure, it was always a temptation to discard the wider technical requirements and just focus on the narrow ones (Vanderburg 2000 gives a thorough analysis; van Eijnatten et al. (red) 1996 present some alternatives). Within that narrow focus, a negative question is guiding design and development: how to enhance production without regard to the comprehensive quality requirements. And it is here that abortive technologies are born, technologies without the due regard for labor, ecology, user and community.

The criminal disregard for labor during the Industrial Revolution so-called (‘*carried by child labor*’, Schwartz 1993) reminds us of the fact that the maintenance of (technical) quality requires the efforts of all concerned, not the least those of governments. Only after too much time and suffering some balance was attained – in the sense of Karl Polanyi 1944 – but then post-war High Modernism choose for a new round of productivity enhancement, narrowly conceived. In most countries, governments wanted an accelerated growth of e.g. energy production, and of agriculture. The government itself relieved the ‘expert’ of the maintenance of comprehensive quality standards and induced him to focuss on the ‘material’ aspects only. Technical education experienced a government-pushed accelerated growth, so it is hardly an accident that the new shift of ‘experts’ was educated within a narrow design paradigm. After that, only another extended series of painful experiences could breach the walls of this new system of ‘enhanced production’.

The problem is not that industry is ‘not natural’. The production of ceramics, the burning of chalk and the refining of ores are ancient processes indeed, and always were high-energy processes that of necessity were isolated from the laboring human. The first round of up-scaling saved energy, but was also quite demanding already in terms of materials and isolation. When further upscaling meant the introduction of flow instead of batch processes, design and construction became far more demanding still. These steeply increasing demands as a rule **make the responsible technical scale of a process a rather modest one – as long as full technical standards are maintained** (Schumacher’s starting point for man-and-ecology adapted technology). But when government itself wants a forced scale-up, as with war production, or puts its weight behind such aims in an industry, as in much of post-war ‘productivity enhancement’, then, by limiting design requirements, large parts of the increasing costs can be wished-away. By allowing or demanding their non-consideration, those increasing costs can be ‘externalized’: shifted to the laborer, to the ecology, to the user, to society at large. Though such costs often are quite evident, they are ill discernable within an institutional-industrial framework that sanctions their non-consideration...

Bottle feeding vs breast feeding is a well-known example of governments allowing the non-consideration of true quality requirements. The industry attained its growth by denying both the specific mother-child contact, and the specific (non-industrial) character of mother’s milk. When governments, from the fast growth of the industry, concluded to its progressive character, this greatly inferior technology could prolong the harm done.
In a way the situation in post-war agriculture was worse still, for here (as in e.g. transport) the government was lavishly providing direct and indirect subsidies to have it ‘industrialized’. With these subsidies, and within an accounting system only looking at momentary money flows, the bad designs did not show up, and the growth of ‘industrial’ agriculture could be out of balance from the start. E.g., soil compaction by tractors was evident early on in post-war decades, but this environmental cost was not considered in the all-out promotion of mechanical traction. That is one of the reasons that an agro-industry and an agribusiness could grow from a questionable ‘enhanced production’: from a price level that obliged the farmer to forego on careful soil management.

With at least the historic possibility of an unbalanced growth of post-war agriculture indicated, let us look once more to this period from an industrial point of view. Assume for the moment a green-and-clean industrial process. Because it derives its high volume from high flows and/or high energy it needs

(1) constant flows of materials and energy of narrow specification

(2) distantiation and isolation, both because its high-intensity flows are life threatening, and because leaking must be prevented

(3) monitoring of the streams with sensors that in turn will feed forward/backward regulation.

In fact none of these characteristics will do in agriculture. Flows of energy and nutrients are high in ‘industrial’ agriculture, but not because its photosynthesis is improved (if anything, half a century of breeding brought a slight decrease). So it tries to attain its higher yields by irrigation (boosting evapotranspiration/unit biomass), by high fertilizer gifts (its nutrient streams debilitating the ecosystem), and by high planting/seeding densities (that have to make up for the limited growing period of its monocultures). Its attempts to apply ‘industrial’ means translate in high levels of stress on the ecosystem: its ‘environmental loading’ is far above a critical threshold where it is no longer compatible with the long-term stability of this ecosystem (cp. Giampietro 1997).

Still, the industrial process/factory was indeed presented as the one-and-only model for a ‘modern economy’, and those requirements of industry were back-projected on agriculture. Then large-scale processing required big quantities of uniform materials fit for high-speed processing. And process development itself started from the assumption that agricultural materials can be equated with the feed streams elsewhere in industry.

4.13. Failed designs

Significantly, the notion of individuality of plants, fruits and grains will easily be lost from sight, only to show up unexpectedly during processing. Then it will need close attention to the ‘feed stream’ by operators with practical skills, with which they make up for design and management failures.
A pathetic example of the loss of this acquaintance (with the individuality of natural products and materials) is the effort of Nelson c.s. (from the USDA Agricultural Research Service) to substitute subjective quality sensing of watermelons with measurements with the help of dielectric spectroscopy (Nelson et al. 2007). Nelson’s publication starts with down playing ‘subjective quality standards’, in spite of the fact that anybody watching e.g. a Southern or Eastern European farmer or buyer tapping a water melon soon discovers that their expertise is quite solid. At the end of their publication, Nelson c.s. have to admit of ‘*the poor individual correlations of dielectric properties with soluble solids [mostly sugars] contents*’, yet this does not bring them to the recognition of irreducible individuality. Instead of a reconsideration of their approach, we read: ‘*Further studies are needed to determine whether practically useful techniques can be developed for reliable prediction of watermelon quality from dielectric properties of the melons*’.

When Nelson c.s. next focus on peanuts (cp. Kandala & Nelson 2007), the batch dependence of their calibration values is immediately apparent, yet not treated by them. Let alone that they would try to make a link with the cultivation-, location- and time-specific origins of those batches, or investigate individual variation within them.
Long experience in monitoring systems with biological components that are far more uniform than e.g. melons, e.g. dissolved oxygen measurement in medical fluids or waste-water, has led to the insight that ‘*uncertainty forms an intrinsic part of the measurement result*’ (Jalukse & Leito 2007). Here close attention to the various sources of uncertainty is standard practice. Even in those more simple systems there is nothing that can compare with the monitoring that is applicable to e.g. machines in industry (e.g. Hoffmann et al. 2007).
Those manifold technical experiences notwithstanding, Kandala and Nelson confidently close their article with ‘*The RF impedance measurement method provides a basis for the development of a practical instrument that can measure moisture content of in-shell peanuts*’. From their ‘industrial approach’ they project a uniformity that never existed.

The summit of this denial of real-life individuality from within USDA’s Agricultural Research Service is Trabelsi & Nelson’s ‘*microwave moisture sensing technique for grain and seed*’ (Trabelsi & Nelson 2007a, 2007b). All the individuality of species and grains, that is always connected with their culture-, location- and time-specific origins, is passed over. In discord with e.g. Lisovsky 2007 (cp. also Yagihara 2007 and refs.) even the difference between free and matrix restricted water is no more considered, although the latter is ever matrix specific (and often in need of a fractal geometry approach). Compared with Lisovsky's careful '*The specified methods…are effective at rather small variations of density…and a rather narrow range of moisture content*', discard of limits by Trabelski & Nelson is a token of sloppy technology. Their display of technological optimism is not to be mistaken for a proof of their methods. It is better to consider if not much of this kind of main-line research, in spite of its impressive institutionalization, has lost connections with the real life of plants, farmers and agriculture.

The post-war half-century brought us artifacts like Nelson's, because the institutional framework was sanctioned by the government, and because it next opened the economy to the growth of impressive agroconcerns. The artifactual research weighs heavily on agriculture and food production, because it only 'knows' of an agriculture dominated by industry. Consider the following example from a publication about the application of growth retardants (Grossmann 1992): '*The growth behaviour and yield formation of crop plants are governed by their genetic potential, climatic conditions, and the supply of nutrients*'.
The apodictic style makes that this statement is easily taken for a fact, yet none of the decisive contributions to plant growth and yields that were indicated in the present chapter, is so much as mentioned. Researchers like Grossmann seem completely encaged in the 'industrial paradigm', with real-life soil, plant growth and agriculture mostly outside that cage. They hardly notice that it is their 'industrial package' that makes many of the most notable problems. Hoffmann (1992) announces that with mepiquat chloride '*A 20 to 30% reduction of shoot height can be expected under conditions of intensive fertilization and irrigation*', yet is oblivious to the possibility that such application is part of a 'technological fix', a flight forward from the excessive (weak tissue) vegetative growth that is the result of the application of large quantities of fertilizer.

Researchers like Grossmann and Hofmann do all their best to implement a truly industrial agriculture, as when Hoffmann stresses (1992 p.803/4): '*A uniform ripeness is very important for the mechanical "once-over" harvesting and eliminates the necessity of a second picking. Part of this synchrony can be induced with mepiquat chloride treatment at the early stages of plant development. Additionally, treatment with ethephon a few days before harvesting strongly promotes the opening of all mature balls*'. Here soil, ecology, local knowledge and rural community are no longer in view and the only thing that counts is the drive for 'industrialization'. In the same vein, we see in post-war breeding such a focus at exactly the 'industrial' requirements: Yield, Uniformity and Processing (its YUP-bias).

As to Processing, take milling as an example. Though the slow-stream milling of old allowed for quite variable feed streams, because the miller could easily adapt his mill to a new batch of grain, high-speed grain milling demands truly narrow specifications, because now heat dissipation, stickiness, etc need to stay within narrow bounds, or the milling process is endangered. Back-projecting its 'needs', that follow from a design that is doubtfull when considered from the character of its feed streams, e.g. the wheat variaties that still are 'acceptable' are reduced to (big quantities of) a few varieties. The miller could rather easily adapt his milling process to a specific batch of the many farmers' varieties of his region, but the high-speed mill is at a loss with the small quantities of those varieties and their specific qualities, and so 'needs' one of the YUP-varieties, in big quantities. Generally presented as progress, it is in fact a bad adaption of a large-scale agro-industry to its feed streams, and translates into a strong push for just a few crop varieties. From a soil and ecology informed point of view, there is something bizarre in it all (cp. e.g. Browning 1998).

Note that the all-out mechanization of agriculture is discordant, in important ways, with its industrial equivalent: just think of investing huge financial capital in machines that stand idle for extensive periods of the year. The post-war parallel of this ‘idle mechanization’ is not to be found in industry, but in the all-out introduction of cars. For that also was an investment in machines standing idle most of the time, and also this ‘mechanization’ was part of the all-out transition to a cheap-oil economy in which our post-war society lost its energetic senses. As to agriculture, a direct result of this transition was the direct and indirect subsidies for road transport that enabled our food processing & distribution giants to establish themselves.
As to the promotion of machines in agriculture, many farmers in Europe were induced to make these ‘idle’ investments as a result of government regulations and subsidies (both direct and indirect). Their ‘enhanced production’ then marginalized other farmers and was used by governments first, and by processors and distributors next, to lower prices.

As a matter of fact it did not take long for the ‘modernizers’ to become loosers, with the interests on the loans for all of their idle machines etc. outdoing their gains. Ikerd 2008 Ch.16 tells what he saw as an agricultural economist in the US:

> ‘*Many farmers had borrowed heavily at record high interest rates* [in the 2nd half of the 70s, after financial markets had been ‘liberalized’] *to expand production to meet booming export demand during the 1970s, only to see exports dry up, commodity prices plummet, and record farm profits turn into disastrous farm losses. … I discovered that the farmers who were in the biggest financial difficulty had been doing the things that the agricultural establishment, including myself and my colleagues, had been telling them they should do’. … ‘Many family farmers had not followed the advice of us so-called experts. They were not overly specialized; they had maintained some diversity of enterprises, and some enterprises were still profitable. They had minimized their dependence on costly chemical inputs and farm equipment, so their cost-price squeeze wasn’t quite so tight. They had not bought land to expand their operations, so their debts were more manageable. The farmers we economists had branded as laggards – resisters of new technologies and new ideas – were at least coping with one of the most severe economic farm crises of the century*’.

Next, look at this matter of process insulation, to prevent the leaking of energy and/or materials. Such prevention was well known in traditional mixed farming with its great extent of nutrient cycling, where it depended on the integration of a diversity of on-farm and community processes. With the adverse kind of R & D reaching the farming sector, these processes were first dis-connected and then individually ‘optimized’. The government and its experts were congratulating themselves with the results - till it became apparent that the whole design process had been wrong. This showed up, when the leaks from the newly designed system proved big and beyond repair. The vain effort to contain nitrate leaching (Hack-ten Broeke 2000), after earlier denials of its existence (e.g. Schuffelen 1974 and refs.), offers a pathetic example. At least as pathetic is the effort to contain pests and diseases by covering vast stretches of plants and soil with pesticides, with nearly all of it missing its target, yet prone to pollute soil and water, and to incite fast evolution of resistance. The serious health consequences for most of the applicants is an integral part of this ‘dirty technology’, comparable to the health effects of dirty designs in industry.

In industry, the subject of bad versus good design was increasingly acknowledged, with quite

some encouraging examples of ‘green & clean’ technologies as a result (see Tundo & Anastas 2000 for examples from industrial chemistry, Stevens & Verhé 2004 for non-food renewables). Still we know, especially from the move of much rich-country production to low-regulation districts elsewhere (e.g. Hesselberg 2000, Braun & Dietsche 2008), that ‘**dirty technologies’ (of all kinds) are still prominent because technical standards are being flouted**.
In regard to ‘industrial’ agriculture, its inherently dirty character was recognized only recently. For a time the introduction of some ‘precision agriculture’ was presented as a solution, and so the need for a thorough re-design is only now being acknowledged. Note that, for the moment, we experience also here a flight to low-regulation districts far away: witness the fast growth of e.g. vegetable imports in Europe from Egypt, Kenya, Ethiopia.

4.14. Precision agriculture?

Still, that does not change the fact that there is a measure of recognition within mainline agricultural research of the urgency of re-design (e.g. because of nitrate leaching and other fertilizer losses). But as distinct from industrial technology at large, where re-design is exploring a vast universe of possibilities, mainline agricultural research until now stuck to its earlier paradigm. Confidently it advised government regulators about the solutions it envisaged – e.g. liquid manure injection - ultimately to be confronted with the fact that soil and plant reality did not conform to them. As a result much of the earlier self-confidence has disappeared – and yet research and policy are still far from abandoning the post-war paradigm. They especially still cling to the notion of ‘precision agriculture’ as something that will allow a continuation of the post-war path chosen, but then with some high-tech fine-regulation. Automatic fine-regulation in industry is the envied example.

Now industrial feed streams ever are pre-treated so as to stay within narrow specifications. Taking mining and ore processing as example, we see that because of entropic relations we are sure we have no ‘clean technology’ here. So we choose for containment and restoration: there is inevitably much waste but we can at least contain it, prevent pollutants etc. from leaching, and by careful management induce the great amount of waste produced (that is itself a consequence of the partly concentrated materials obtained) to re-develop into e.g. some more stable soil and ecosystem. In a pregnant way our industrial system is dependent on a careful management that embodies the hope to induce a process of natural regeneration that will regenerate our deserts of waste. A ‘higher order’ regeneration in which especially biotic processes that are able to fine-tune to (micro)local circumstances with the help of natural energy & materials inputs will revitalize the ‘desert’ we created. As to us, all here depends on early containment and great efforts in revegetation and revitalization. And from the start it is needed to contain also e.g. the mining & processing efforts themselves: when growing too large, both containment and regeneration will no longer be possible, and the waste desert constructed will adversely influence a far greater region still.

What this short excurs teaches us is that our industrial system, instead of supplanting nature, is completely dependent on natural regeneration. Its many costs are far greater than we are ready to acknowledge – and the many regions that got greatly polluted because of careless mining and processing testify to the ill results of our refusals to invest care for the initiation of regeneration. And we see: the whole idea to supplant nature with our supposedly ever more advanced constructs is a chimera. **A sustainable society exists only there where natural regeneration is undoing our industrial system’s gross waste.** Once we understand that much, we also see that there are perfect reasons to consider other options for production than our industrial ones. There is no good reason to stay within the post-war industrial paradigm.

As to agriculture, its non-industrial character is soon apparent. At the most fundamental level, that of soil, agriculture is essentially not working with industrial feed streams and so cannot apply industrial-type fine-regulation. Instead it is for its sustainability completely dependent on the inherent fine-regulation of in-situ soil processes. I will now seek to adstruct this important subject with a systematic exposition of some key publications.

1. Soil is irreducibly variable & heterogenous, both spatially and temporally**.**

(A) Jackson & Caldwell (1993) analysed 362 soil samples within a narrow local range and found the ammonium and the nitrate concentrations spread over three orders of magnitude.

(B) Parkin (1993) reviewed the spatial variability of microbial processes in soil and concluded that '*The high spatial variability exhibited by many microbial processes, in many cases, precludes precise quantification*' (l.c. p.409).

(C) A decade later Nunan et al. (2002 p.304) write: '*This study strongly suggests that investigations into the effects of variables on natural bacterial ecosystems in arable soils shoul be carried out at the 0,1 to 1 m scale and/or at the micrometer scale*', that is at scales unfathomable to industrial agriculture.

(D) Amador et al. (2000) analysed fine-scale variability of properties in an old-field soil and found all variable, the biological still more than the physical. The form of the spatial variaton was always specific for the property studied, also at small distance. Implying that even if one property would lend itself to some form of 'monitoring' (as in industry), such is of no help to our knowledge of the variation of the other properties.

(E) Yang, Hu & Bu (2006) analysed the microscale variability of redox potential in a surface soil. Also this proves to vary with both location and scale, with e.g. the application of the means to represent unsampled neighborhoods: evidently a procedure that is not valid. And then, the measurements have always some artificial character themselves. Yang c.s. discovered that geostatistic treatment of results did not help in finding any viable proposals for simplification. (See Teichert et al. 2000 for a.o. aspects of method).

(F) Omonode & Vyn (2006) found apparent electrical conductivity only weakly related to e.g. nutrient characterstics in a field soil (as is evident from e.g. the point clouds even in their log-log fig.2). And the weak relations that díd show up were not stable - they changed both spatially and temporally. In short, this conductivity offers no prospects for monitoring e.g. soil fertility characteristics.

(G) Sauer & Meek (2003) made a close analysis, both classical and geostatistical, of the micro-local phosphorus availability (as conceived in common protocols). They found that the sampling density and analyses required for meaningful investigation '*represent an approach that is cost-prohibitive under current economic conditions*' (p.835). Also, the real micro-local variability is so great that the common fertilizer or slurry applicators cannot be thus finetuned.

2. There are no perpectives for sensor use in agriculture**.**

(A) Jabro et al. (2006a) found that correlations between most physical soil properties – candidates for monitoring during farming operations - were non-significant.

(B) They found especially (id. 2006b) that those properties, that at least would give some chance for sensor-use to wit electrical conductivity and cone index (soil penetrability), had a correlation of about zero (cp. their fig.9).

(C) Logsdon (2006) in her thorough study of soil conductivity and permittivity spectra demonstrated that as to electrical conductivity (a) sample-to-sample uncertainty is big, and (b) incomplete contact is a problem for some samples even in the laboratory. So there is no reason to expect satisfactory results of routine measurements in the field.

(D) Omonode & Vyn (2006) as indicated found that electrical conductivity offers no hope in connecting this with nutrient characteristics in the field.

3. Other ways of plant & soil monitoring offer no escape**.**

(A) Borges & Mallarion (1997) in their close research of both the plant and the soil K&P status in eight different fields with varied history of fertilization found (a) the 'plant nutrient status' is not a plant property with some well defined range, there is a high variabil-ity for plant dry weight and P&K content instead (b) the plant characteristics indicated show a variable correlation with the local soil nutrient status. The uncritical assumption of the existence of such a plant or soil nutrient status that is easily measurable is at the root of much of our futile agricultural research.

(B) Robertson et al. (1997), meeting the same lack of correlations, stated:

'*We were also surprised by the lack of overall correspondence between plant productivity and a host of soil properties. That any given combination of measured soil properties could explain no more than 46% (r=0,68; Table 7) of the variation in plant productivity across the site – despite the fact that productivity varied by more than an order of magnitude – defies conventional wisdom and bodes poorly for efforts to base site-specific agronomic management strategies on isopleths of soil properties*'.

(C) And Borges & Mallarion state (l.c. p.852):

'*The results for these fields suggest, however, that the spatial structure of the variability for nutrient concentration or uptake is site specific and nutrient specific, and that the optimal sampling scheme and optimal separation distance between sample positions would vary markedly among fields and among directions within fields. Specific recommendations for these fields probably would be of little value for other fields*'.

> The reader will wonder why all of this variability and lack of correlation was not discovered much earlier. Part of the answer is that research and advice for decades were locked within a kind of circular reasoning. Just consider Baer 1965 p.160/1:
> '*Two standard methods are employed for determining whether extra nitrogen is needed by plants or can be used by them to advantage. One is to test the growing tissues of the plants colorimetrically for nitrate. If nitrate is present in the plant juices in appreciable amounts, lack of nitrogen is not the primary limiting factor in the plant's growth. The other method of testing for nitrogen deficiency is to apply some quickly available source of the element, preferably a nitrate, and note the effects*' . It is all very human indeed: 'standard methods' contructed from wishful thinking.

4. There is no escape in geostatistics either**.**

(A) Borges & Mallarion continue:

'*A significant implication of the results is that theoretical models commonly included in geostatistical packages would not describe appropriately many semivariograms shown in this study because they cannot account for periodic trends. This problem was observed for soil properties … and suggests blind selection of a best-fitting model often not appropriate*'.

(B) Likewise Stenger et al (2002; research station Scheyern) conclude:

'*Due to the low spatial correlation and the temporal instability of distribution patterns, nitrate-N semivariograms and resulting maps are not a useful basis for variable-rate fertilizer applications*'.

(C) By then other efforts to find a way out, e.g. by using the micro-local yields of the preceding harvest as a guide, had proven ineffective too (Maidl et al. 1999, Kravchenko & Robertson 2007) - and were widely known as such (den Biggelaar et al. 2001 p.39).

5. Soil and soil fertility are essentially non-reducible**.**

(A) Goodlass et al. (2002 p.720) as to proposals to regulate N-fertilizer concluded:

'*The study showed that some of the inputs currently used to determine N requirement need to be revised and additional terms added. However, even the best estimates have poor predictive precision*'.

Now note that for decades there was no discussion about the supposition that 'N' was most important in determining yields. Yet when serious research indeed made a prolonged effort to attain 'precision agriculture', the supposition proved unfounded.

(B) In fact soil fertility **conceptually** exceeds our laboratory and industrial methods. Just consider the conclusions of Cox et al. (2008 p.550), an example of painstaking research:

'*any management plan incorporating the findings of this study would not be addressing the cause or causes of most of the yield variability*' and '*most of the yield variation was not explained by the soil and terrain variables measured*'.

Our reductionist approaches simply do not work, here in the field (also Kravchenko & Robertson 2007) . Soil and soil fertility indeed have this hierarchically-complex character where properties at systems level cannot be derived from the 'parts', but are what already Polanyi c.s. called 'emergent properties', non-reducible to the core.

(C) Note that Bradfield (1946), right at the start of the post-war boom in institutional agri-cultural research, after expressing his contentment with the introduction of improved statist-ical methods, objected against their indications of complexity *not* being followed:

'*As I study the results of many of these experiments, I often have the feeling that many of our research workers feel that their job is done when they complete their analysis of variance and establish the odds of significance for their experiment. This is often done even in cases where the variation in yield due to factors which are uncontrolled or are unknown is of the same order of magnitude as the factor or factors being studied*'.

'*I am always impressed by the fragmentary nature of a high percentage of the contributions. Each paper may, and probably does, contribute its grain of truth to our immense storehouse, but it is becoming increasingly difficult to trace the relationship of these various fragments to each other. The classical approach to agronomic problems was to keep all factors of the environment constant, except the one under study, and to vary it systematically. Even a superficial analysis of the cycle of changes which go to make up the environment of the growing plant will indicate that this classical approach is highly artificial*'. One more of Bradfield's injunctions (l.c.) that received no follow-up in the booming agricultural research circuit of post-war decades.

We summarize:

1. Research focusing at methods in which plant production is connected with soil 'material' characteristics thanks to the application of some 'sensor', industry-like, brought no results.

2. As could have been evident to all from the start, soils proved to defer any industrial description in which materials (flows) are actively contained within narrow technical limits. Instead the dynamic-biotic and irreducibly hierarchical & heterogenous (spatially ánd temporally) character of soil became amply evident.

3. Any real-life effort at 'guiding' starts with the recognition of the irreducible complexity of soils & soil life, next to search for locality- and plant-specific ways of stimulating the soil's modes of sensing & regulation. Expressed in another way: sustainable soil health and plant production depend on caring stimulation at all the hierarchical levels of soil and soil life.

Note that the **individuality and heterogeneity** of soils are part of the given natural system in which everything has this same character. Likewise plants, though morphologically distinct at the species level, are always individual entities, both as a whole and as to their 'parts'.

> As to those 'parts', Weyers & Lawson's (1997) study of stomata offers us a well-researched example of the heterogeneity and individuality of plant parts. At the physiological and bio-chemical level we see this heterogeneity and individuality repeated, e.g. Watt & Creswell 1986, Ievinsh & Ozola 1998, Kirschbaum-Titze et al. 2002, and esp. Trewavas 1999. But note that post-war physiological research mostkly started from the silent assumption that each set of plants consists of identical individuals… Still, heterogeneity/variation is very important in e.g. inducing '*dramatically decreased pupation rates*' of herbivores, Shelton 2004. As to the connection between soil heterogeneity and plant part heterogeneity see Orians, Ardón & Mohammad 2002.

With individuality and heterogeneity characteristics of the natural system everywhere – and including its fine-tuned methods of regulation (e.g. Trewavas 1999) - industrial uniformity is quite likely an expression of our helplessness in the face of so much individuality, and only in a very restricted sense an asset. And of course, within the crafts from the dawn of history man has found ways to deal with this natural individuality. **It is not craft-like agriculture that has something to explain, but the post-war industry-like version.**

In soils we meet individuality/heterogeneity everywhere, not just in the concentrations of mineral constituents. Soils are always 'local individuals' and biotic in important aspects, active in e.g. (co)determining nutrient concentrations & availability in their own specific way, instead of 'waiting for our orders'. As Davidson & Hackler's (1994) demonstrate: '*the concentration of ammonium in bulk soils may be a consequence of characteristics of the soil and its microbial populations rather than a driver of the microbial processes*'. Yet we tried to stand the world on its head, from our proud supposition that our fertilizer gift is the driver of it all. We lost sight of real-life soils, and of the expertise of the farmer of old, the insight that one 'needs to be there' to get acquainted with a soil and its fertility. We got used instead to suppose an 'indifferent' background for our experiments and theories, and did not face the individuality and heterogeneity of soils.

> **Heterogeneity** is everywhere in soil and in its constituents. As to humics, see Preston & Newman 1992 and Christl et al. 2000. For (soil) clay components see e.g. Barrow 1993, Bank et al. 2001 (p.101), Drits 2003. The micro-character of this heterogeneity influences soil properties in a decisive way, e.g. soil water sorptivity, Hallett et al. 2004. Where often a thin surface layer on e.g. mineral particles suffices to give a dramatic change in properties, there is no reason at all to expect a close correlation between e.g. chemical analytic results of bulk soil and decisive soil properties. Note that in itself the validity of such arguments was evident in the 1940s too: it was the ruling paradigm that prevented their consideration.

We neglected heterogeneity completely, so that even ecological models (e.g. Vitousek & Field (1999) about interaction of N-fixing plants with non-fixers) assumed '*that ecological interactions were occurring in a spatially homogeneous environment*' (Jenerette & Wu 2004).

Indeed, it was not just main-line agriculture that tried to stand the world on its head. This same general assumption, that given the right 'properties' to 'monitor' we can rule it over from a distance, thanks to our well-designed protocols, characterizes our post-war technocratic governments and the research promoted by them.
But note these were our governments indeed: nearly all of us shared their faith in the constructability of nature and society…

In regard to the specific assumptions of mainline post-war agricultural research, we noted that all its efforts at 'industrializing' agriculture started from a conceptualization of soils as mixtures of solid phases mostly in equilibrium with the soil solution. That conceptualization then suggests off-site characterization and prescription. Yet there never was a basis for it - as was demonstrated once more by Göttlein & Stanjek (1996 p.635): '*there are very limited possibilities for predicting soil solution concentrations from characteristics of the solid phase*'. That means that **also the relation to soil and plant of a big part of 'standard' soil tests and agricultural advisory practices now is uncertain at best.**

> Note that with the rejection of this conceptualization also the whole gamut of soil chemical & physical theories that had been constructed from this (equilibrium mixture) supposition was laid to rest. Think here of the pre-war constructs of Mitscherlich c.s., then after the war of the equilibrium-based soil chemical/mineralogical constructs of Lindsay c.s. as well as of the soil physical constructs of Bolt c.s., as far as all of these equated the soil's irreducibly-own character with that of some laboratory mixture of pure substances.

The search for an industry-type of 'precision agriculture' has proven in vain. We are left with the historic fact that e.g. the closure of nutrient cycles as practiced in e.g. mixed farming is not attainable for 'industrial agriculture'. Its character of a 'dirty technology' cannot be mended, and it is only right if it is abandoned and replaced by an agriculture that has careful soil husbandry by the farmer at its center.

4.15. Heterogeneity & patchiness: assets for plant and farmer

It is startling that even a very simple model of soil structure/heterogeneity suffices to arrive at a completely other picture than the one mainline agricultural research & advice is working from. Davidson & Hackler (1994) just introduced a tortuosity factor between 0 and 1 as a very simple way to allow for the effect of soil structure on the diffusion of nutrients in soil, with 0 indicating complete independence of the bulk and the microsite ammonium concentrations, and 1 their equality (no heterogeneity). They then modeled nitrification in specific sites as dependent on the local ammonium supply and found the commonly measured bulk ammonium concentration '*a consequence of characteristics of the soil and its microbial populations rather than driver of the microbial processes*' (p.1452). In other words, the relations between bulk ammonium concentration and nitrification that had been intimated before proved to be pure artifacts.

Similarly while only acknowledging the spatial distribution of microbial 'N-hotspots' in their model – straw as sink, clover as a source – Korsaeth et al. (2001) showed that plant roots were very active players in their own right, e.g. able to '*increase the net microbial N-mineralization in soil, not by stimulating the microbial activity but by successfully inflicting N-starvation in soil microorganisms*' (p.224). Once more we see that the introduction of even a very simple spatial model suffices to promote the plant from a passive player waiting for the microbes to deliver its nutrients (the picture in the mainline paradigm) into an actor able to 'rule over' the microbes. All thanks to a patchiness of organic residues in the soil.

And then: the interactive dynamics of plants and microbes makes all the difference even in an originally quite homogeneous soil. Baker et al. (1997) focused at the feedback dynamics of individual plants with their soil microbial community: even in homogeneous soil positive interactions can lead to patchwise different soil communities, while negative feedback leads to the maintenance of above ground species diversity. Altogether the modeled interactions of plants with the microbial soil community '*may create selective forces which run counter to those from differences in the mineral soil*' (p.569). The mineral soil proves subservient to the biotic actors. Once more the reductionist approach of mainline research & advice proves mistaken.

Note that the authors quoted use explorative modeling: because of the unknown complexity of the soil they focus at models that look at just some of the interactions that can be imagined. As a rule they focus at some potential possibilities that they intimate from field research to be realistic, yet unacknowledged in standard theory. This helps them to explore possibilities - as distinct from the kind of modeling that is looking for deterministic results that allow protocol construction (as expected by e.g. policy makers).

We'll look at one more of those explorative modeling efforts, those of Kinze & Harte (1998). They introduced in their model Winogradsky's pre-war division of soil microbes in **strategic** (slow uptake of min.-N) and **tactical** (fast uptake). By further acknowledging different 'N-reservoirs' with simple relations between them, they arrived at remarkable results some of which deserve quotation:

'*Not only do plants have higher biomass and higher inorganic nitrogen uptake (and therefore growth) rates when they coexist with strategists rather than tacticians, but the simulations also show that average plant assimilation rates in the presence of strategists can be as much as double that in the presence of tacticians*' (p.850)

'*if plants actively exsude organic matter to 'feed' the microbial communities at the root sides where the most inorganic nitrogen is available to them (consistent with the presence of strategic, or at least less voracious, micro-organisms at these sites), then the fitness of these strategic communities could increase even further and they may be able to proliferate throughout the system…*' (p.851)

Quite in contrast, in a homogeneous environment the strategic microbial types will be driven out by the tactical types within a few years (p.842). Surely a soil is always heterogeneous, yet a dominant provision with mineral fertilizer will greatly enhance its homogenization as to nutrient supply. **The result then is a loss of exactly those microbes that can drive optimal plant nutrient uptake and growth.**

Soils are patch-like anyway because organic and mineral particles and aggregates are always present. Mainline research deflected attention even from this unmistakable patchiness with its double simplification:
(**1**) plants use only mineral nutrients from soil solution
(**2**) bulk soil solution concentrations approach the true concentrations everywhere in soil.
As we saw repeatedly, (**2**) is plainly not true: only non-disturbing micro-sampling of the soil solution will do to find true micro-local nutrient concentrations as they are sensed by the plant root (e.g. Moutonnet & Fardeau 1997). The improbability of (**1**) shines through in e.g. Loneragan's (1973 p.119) statement: '*the solution concentration of many minerals is very low in relation to the amounts absorbed by plants: large quantities of these minerals must be*

released from the solid phase during plant growth'. Lonergan's consideration of nutrient acquisition **in direct contact** is evident from his judgment (l.c. p.120): '*Enzymes on or secreted from root surfaces may also play an important role in modifying soil minerals prior to their absorption. Thus the recent demon-stration that iron is absorbed by plants in the divalent form seems to establish that reductive activity at the root surface is an essential prerequisite for iron absorption from soils*'. Lonergan's consideration of direct-contact nutrient acquisition was anything but new, so why did mainline research at large not respond that what was rather obvious?

Presently the attention for direct-contact acquisition certainly is increasing, e.g. in studies looking at such acquisition by mycorrhizae from litter/organic matter (Sinsabaugh et al. 2002) and from clay minerals (Paris et al. 1995). Direct acquisition of e.g. K from feldspates became ever more evident and also widely known (refs. in Gadd et al. 2005). Previous denial of direct acquisition was always at variance with e.g. the nutrition of desert plants, and indeed direct acquisition has been proved beautifully with such plants (Puente et al. 2004, Puente, Li & Bashan 2004). These different examples open up perspectives for agriculture too: perspectives of nutrient acquisition from in-situ and on-farm resources without the losses characterizing industrial agriculture. The primary role of farmer and local ecology in it all is evident.
But note that mainline agricultural research & advice did not yet internalize the evidence and remains silent about the perspectives. That is apparently because it is still suffering from a flywheel effect of its reductionist focus at mineral solution culture, to the neglect of soil with its clay minerals, soil (micro)aggregates, organic plant nutrition, mycorrhizae, etc.
This curious implosion of agricultural and soil reality means that much, if not most, of mainline's expert knowledge applies to a virtual reality: a far stretching re-evaluation of this body of expert knowledge is required. The institutional and policy ramifications of such a re-evaluation are clear.

Yet, the case of agriculture stands not on its own. From its dream of manageability of nature and society, as such the corollary to its dreams of a reductionist science allowing it to wield greatly increased power from the centre, post-war government everywhere confidently worked with expert-derived 'optimization of resources' regimes. We have seen these regimes collapsing everywhere in the face of reality (fisheries, large-scale hydrological works, forestries, etc). It is a comprehensive collapse that makes a fundamental renewal of policies urgent indeed (e.g. Dovers, Norton & Handmer 1996).
Still there is greater tragedy in post-war agricultural policy than anywhere, because there is no doubt that many of the experts and politicians concerned worked hard to make the world food situation safer than it had been. Their greater idealism, as compared with the present generation, explains at least part of their singular focus at reductionist S&T, yet it did not make soil and plant conform to all of this reductionism. The present world food situation as experienced in the Two Thirds World is bleak indeed, and we better scrutinize also our truly idealistic endeavours (e.g. those in FAO, for also that organisation partook in our reductionism – Ilcan & Phillips 2006).

4.16. Re-rooting agriculture and research – the task ahead

In this chapter we took a close look at concepts and methods, in context, as used by the government-induced, post-war institutional agricultural research and extension network. We saw that as to many of the established practices of pre-war farming there was hardly any affiliation. Likewise, we saw that as to soil there was a near-complete neglect that included

the marginalization (or discontinuation even) of important pieces of research. Evidently post-war policy as well as policy-related research embody some massive ruptures.
The choice for technocracy and for an all-out re-direction of production after the example of big industry was the larger framework of those ruptures. This was an a-technical choice because in it the denial of the essential limits of S & T figured prominently. Yet, a sense of progress was derived from the enthusiastic implementation of the technocratic aims, with 'history' reduced to the story of technocracy's ever widening implementation. The self-serving character of this 'history' ('Whig historiography' in the sense of Butterfield) was hardly discernable in a half century deeply committed to Cartesian efforts to escape from real history.

In regard to agriculture, even great minds are convinced of industrial agriculture's impressive progress from the steep growth in the amounts of mineral fertilizer used. That this is in essence the kind of 'historiography' just indicated is only slowly gaining recognition. Only a gross neglect of 'organic questions' – that were always central to sustainable agriculture – and the limitation of concepts and methods to such focusing at mineral fertilizer-only could refigure the confusing course of post-war agricultural history into a semblance of progress. And even then only because we excluded on the way the wide range of 'anomalies' that threatened to burst the narrow paradigm.
I deem it proved so far that agriculture and agricultural research experienced some grave ruptures after the war and that it is futile to sketch these as part of some history 'made by agriculture'. That means that we will have to take a closer look at some aspects of the wider historical frame also to arrive at a more truly historical picture. The role of World War II is an obvious candidate, as is the growth of technocracy. Note that technocracy is essentially the bureaucratic re-definition of technology in the service of policy (government and big industry power). We sensed already repeatedly the overwhelming influence of political direction – under capitalism as well as communism - in keeping post-war research and extension within narrowly defined bounds.

But note that many of the results obtained thus far are of a material character. They together indicate that as to agriculture post-war technocracy embodied an effort to dislodge agriculture from soil and reshape it into a laboratory construct that lacks the connections with the real life of soil and man on which food production does and always did depend. There are dangers like giants embodied in this effort; some of them have been indicated in this chapter.
Conversely, post-war policy and research missed out on about all of the natural resource capital that is at the disposal of the farmer, and on his essential roles in maintaining this capital and make it increase. Together the need to mend the ruptures that were caused is clear enough, as are the positive possibilities. What is in the way is our all-out institutionalization of our technocratic constructs, as fed (under both capitalism and communism) by the ideology that inspired post-war decades and that in important ways still is part of our worldview. We did a solid job in entrenching ourselves in cyberspace…

In the meantime there is no doubt about the unsustainability of our post-war constructs. First in the industrial countries and then increasingly in the rest of the world the farmer saw his existence denied – his knowledge, his craft, his land, his plant and animal varieties. The pretense under which this occurred, that the centrally directed agriculture that was introduced instead was much more powerful, proved true only from the point of view of power politics. As to its contents it is a 'dirty technology' with grave environmental and human consequences. As to its institutionalization it was introduced at immense costs and is maintained with huge public subventions – 300 billion dollars a year according to the

Millennium Ecosystem Assessment 2005. Evidently governments have perfect reasons to redirect their agricultural policies. But note that not their national accounts, but food security and the prevention of the collapse of agriculture and food production are the first ones.

Post-war technocracy was more than an effort to dislodge agriculture from soil and make it inhabit a central laboratory construct instead: it was an effort at reconstituting nature and society at large. Grotesque in important aspects it still did a historically unique job in its all-out overhaul of society and its relation to nature. One of the most untenable aspects of this overhaul is the accelerated deruralization and urbanization, with the removal of the peasant/small farmer from his land – by push-and-pull – at its center. This was 'possible' only as long as the costs were not being counted. When they are, it is clear enough that only a re-ruralization can prevent disasters.

We noted that the cost of soil deterioration is immense: it takes away the very base of food production. The cost of the rupture of the N-cycle that is incumbent on our accelerated urbanization plus industrial fertilizer use (Kawashima 2001) is likewise immense.
Re-ruralization is not just for some 'cosy living', but because our industrial agriculture is untenable and we need the small farmer's craft to maintain soils and grow food.

Now note: while the huge costs of industrial agriculture to soil and environment are increasingly recognized, the same is not yet true for its reverse, the need to re-install the small farmer. The wish to give an evolutionary interpretation to our post-war accom-plishments is an important reason here. We want to dub them progress, not to see them fall flat. And we flatter ourselves with the thought that somehow we will find a technical solution to pressing problems. But then, does this not mean that we prefer a flight forward above a truly critical re-evaluation? No doubt that is human indeed – but how can it ever induce a true solution?

As to the **possibility to re-install the small farmer**, just consider two arguments.
First there is no doubt that the impending oil shortage forces us to reconsider our transport policies and agriculture in concert (Shiyomi 2001), next to consider the advantages of the re-localization of our economies. There are good reasons for such re-localization: even a well understood globalization depends on it. And once relocalization is acknowledged as desirable, local food production by the small farmer is in view again.
Second the present food processing & distribution system in 'advanced economies' employs about the same percentage of the population as were farmers before it saw its policy-induced accelerated growth (Pimentel's argument). That means – if we for the moment leave other considerations aside - that the transition is possible macro-economically. Considering the many sides of the unsustain-ability of the present system of giant food processors & distributors, it is an urgent transition too. Note that giving back a sense of valuable labor to a host of people now locked into low-wage jobs without perspective is an important aspect of that transition.

For the moment I leave it at this indication that the re-installment of the small farmer is not just some piece of outdated romanticism. We met so many questions in this and the preceding chapters that there is no escape from executing a number of historic and scientific background studies before we can attempt to draw some more final conclusions. These background studies make up the second part of our research into post-war agriculture and the position of the small farmer. Because it is a core element of our post-war agricultural philosophy the role of industrial fertilizer, more especially a comparison of the merits of industrial N-fertilizer vs. biological N-fixation, is the first subject asking our attention.

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5.

Fertilizer -

the loss of innocence

Evaluating the fertilizer paradigm

Four decades of growth of industrial agriculture concurred with the abandonment of nearly one-third of the world's cropland because of soil degradation, with compensation largely on marginal soils (Giampietro, Bukkens & Pimentel 1999 p.358). Surely the relations here are complex, but that industrial agriculture is a prime actor in soil deterioration is not in doubt. The 'triumphs' of industrial agriculture quite evidently need a thorough evaluation. With industrial fertilizer at the center of those triumphant stories, it is well to probe its real-life influences not only on soils, but also on crops and on feeds & foods. As recently as the year 2000 a learned author like Vaclav Smil would still skip that evaluation by stating apodictically: '*As far as the nutrients are concerned, we have solved the challenge of their natural shortages by massive application of inorganic fertilizers*' (Smil 2000 p.24). His chapter 'Opportunities for higher cropping efficiences' (l.c. Ch.4) for sure is wide ranging and sober: it is not fashionable transgenics but more precise farming that we need (p.137/8). Yet then he forgets about local soil & plant resources and farmer qualities and writes '*Precision farming will thus demand a much more informed management, moving agriculture along the path already traversed by industrial production*'.

In the last chapter we escaped from this tunnel vision by an analysis of soil fertility and 'precision agriculture', as well as by taking a close look at the peasant's natural resources. The widely presumed 'need for nitrate' proved not that pressing, and with the pressure from the lid we now are in a position to take a less nervous look at the ecological and health consequences of mineral-N fertilizer. A great help is here the rise, end of the 80s, of medical 'NO' research, that soon led to similar developments in plant science. It gave reasons to doubt mineral-N fertilizer's innocence, but evaluation was hampered up till now by internal disagreements within official committees (cp. Powlson et al. 2008). Because an in-depth literature review is urgent, I offer one in the present chapter. As a positive development, the rise in ecological N_2-fixation research brought an awareness of the massive and unexpected importance of natural N_2-fixation - and of the rudimentary character of our knowledge of the diversity of micro-organisms and plant-m.o. associations involved in the process.

What then made the industrial & fertilizer paradigm so all-dominant for at least half a century – and how did this occur? Only historical research uncovering the complexity of the process is of value here. After all, there were many able people involved in paradigm building and propagation and not a few of them were true idealists. What in a way seems curious when evaluated after half a century, as a rule had some real power to convict in its own time. Only when we arrive at some sketch of the historic *possibility* not only of certain practices and convictions, but also of their relative *probability*, we are in a position to evaluate. As before the 'paper traces' of science

will prove of great value here, helping us to discern why and how concepts and methods got their standing within and outside research disciplines.
Helping us discern also if, and in what measure, researchers were true to the objects of their research. For in the field agricultural research there ever is 'substance' that sets the standard – soil, plant and farmer first of all. When and where research starts 'wishing its objects away' – neglecting soil and farmer or proposing to do without them – it is evidently below standard. In the present chapter we will focus especially at concrete aspects of soils and plants. Being a chemist, I have a professional preference for 'material arguments'. But note: whenever the reader will need some patience in digesting subjects of a rather specialist character, I promise that at the end of the exercise the reward will be some modest but true handhold in our evaluation efforts.

5.1. Pieces of a puzzle

A minor paradox in industrial agriculture's use of fertilizer is connected with the mutual repression of NH_4^+ and K^+ of their uptake by the plant, as well as NH_4^+-induced repression of nitrate uptake. The close crystal chemical similarity between NH_4^+ and K^+ is part of the explanation for their mutual repression – and this similarity is evident from the 19th century on. As to the knowledge of the nitrate repression indicated we can go back at least half a century.
Now note that these uptake repressions make us expect that higher fertilizer gifts will easily lead to enhanced leaching losses of the mineral fertilizer components – as indeed it became apparent soon after the introduction of industrial fertilizer. Any 'precision agriculture' is in need of a previous solution of this problem of mutual repression. And when proponents of industrial agriculture confidently predict precision agriculture's design without ever so much as mentioning it (e.g. de Wit 1972/1974) they evidently are not 'down to earth'.

> As to the mutual repressions indicated Forde & Clarkson 1999 in their review offer a wealth of material. Note that much of the repression paradox had been well investigated already in the 60s – cp. Fried et al. 1965, Tromp 1962. And as to the classical case of K^+, NH_4^+ similarity see Freund's 1904 overview (in the 1968 Dover reprint). The paradox is typical for industrial agriculture: when we turn to natural resources allowing continuous 'just-in-time' delivery of nutrients, it is no more there. But, for the careful use of those resources we need a farmer-centered agriculture…

Now it is reasonable to suppose that proponents of industrial agriculture like de Wit were acquainted with this phenomenon. But they evidently were noticing this fact through techno-cratic spectacles: together with their audience they were sure any obstacles would soon be conquered by S & T. Now after more than three decades have passed we know they were not. Some formidable other obstacles showed up instead (the fact that soil and fertility are not amenable to sensor-based manipulation is a huge one).
Of the really big obstacles, fertilizer 'toxicities' to plants had shown up even before 'industrial agriculture' had its way. E.g. Nelson 1946 (p.459) warns that

> '*Overabundant nitrogen … produces a large, soft, succulent growth, dark green in colour, and often susceptible to insect and fungus attack*'.

That is quite enough to be very careful indeed with fertilizer gifts – and yet all of main-line research in post-war decades was only interested in their accelerated growth.
The result was that from then on the 'toxicities', though always present, were being mentioned only in passing (Bergmann 1986 S.12, Wiese 1993 p.28):

'N-Überschuß mit nachteiligen Folgen … insbesondere auf die Qualität der Ernteprodukte, tritt meist schon auf ehe ausgesprochene Schadsymptome sichtbar werden. … Übermäßig große Blätter mit großlumigen Zellen und schwammigweichem Gewebe ohne Blattrandchlorosen und –nekrosen, die bevorzugt von Insekten, Pilzen und Bakterien befallen werden, sowie übermäßig lange Triebbildung (z.B. bei Obstbäume) sind ebenfalls Symptom für N-Überschuß'.

'Excess N promotes lush, rank, and prolonged vegetative growth. This circumstance can accentuate lodging, frost injury, and disease'.

In the meantime plant debilitation in industrial agriculture had to be 'countered' by the application of ever increasing amounts of pesticides, growth retardants, etc. In spite of all those means a change in e.g. meteorological circumstances can easily '*lead to the rapid development of NH_4 toxicity or to the accumulation of excess NO_3 in tissues*' (Fernandes & Rossiello 1995 p.125). But it is the farmer and the consumer who are confronted with those negative results, not main-line research as backed-up by government…

> A critical review of current mineral fertilizer provision in agri- and horticulture is contained in Fernandes & Rossiello 1995. Since then NO-related research brought new concepts and methods to study the many problems indicated. Apart from that the 1990s brought renewed attention to **ammonium toxicities** in plants. (As to the preceding decades research in these phenomena often was inconclusive: Golvano & Felipe 1986 even marks regress comparing with Puritch & Barker 1967). Britto & Kronzucker 2002 give a good overview of the 1990s research, and of the problems confronting earlier research (but note also e.g. Ayling 1993). Britto et al. 2001 and Kronzucker et al. 2001 in combination with Szczerba et al. 2006 show us the 'futile ion cycling' that makes us wonder about industrial agriculture's fertilizer gifts. (Note the concept was used already by Buurman in his 1991 thesis).

In fact the list of negative effects known for at least half a century is that long that systematic investigations into alternatives for the high use of mineral-N fertilizer would have been indicated. Jungk lists them for the plant (19.. Tab.2):

Symptome überhöhter Stickstoffversorgung von Pflanzen

Wachstum üppig ('mastig')	Blätter gross, dunkelgrün ('blasig')
Gewebe weich ('schwammig')	Anfälligkeit gegen Schadpfilze erhöht
Stützgewebe geschwächt	Wurzelwachstum vermindert
Standfestigkeit erniedrigt	Trockenresistenz verringert
Ausreife (Triebabschluss) verzögert	Frostresistenz eingeschränkt
Haltbarkeit verringert	Blütenansatz z.T. schwächer
Zuckergehalt (Rüben) erniedrigt	Zuckerausbeute (Zuckerrüben) beeintr.
Starkegehalt (Kartoffeln) erniedrigt	Vitamin-C-Gehalt (Gemüse) erniedrigt
Geschmack (von Obst, Gemüse, Kartoffeln) beeinträchtigt	

Yet the doctrine that ammonium and nitrate were the only N-nutrients that were (sufficiently) available to the plant made main-line research unreceptive for alternatives from the very start. As often in history, there was much here that was incidental and yet largely determinative of the post-war developments in agricultural research. For this research had its chief roots in the USA where already around the war a preoccupation with mineral nutrient solutions according to Hoagland c.s. (as applied in e.g. sand culture) had developed. This meant a fixation on the 'optimization' of the use of fertilizer concentrations that in fact were outright excessive. In Epstein's words (Epstein 1983 p.vi):

> '*In all of them, most nutrients are present in concentrations far in excess of their concentrations in typical soil solutions, to enable plants to grow for long periods by drawing on a large supply of nutrients contained in conveniently small volumes of solution. Hoagland was aware that plants can grow well at much lower concentrations in the medium, and do so in nature. But progress in developing the technology of culture media automatically maintained at predetermined, realistically low concentrations has been slow and sporadic*'.

But then, it was exactly the easy equation of soil with water culture media for crop growth (e.g. Arnon & Hoagland 1940 Summary) that made American research massively follow the Hoagland directions (e.g. Robbins 1946). What initially was a bad choice soon became an ingrained bad habit of all of the research circuit and displaced any and all other approaches. Soon main-line research was no more free, neither conceptually nor institutionally, to pursue explorative investigations outside the officially sanctioned paradigm (cp. Lockeretz 1981 p.275 for the USA situation).

A note on fashion in science:
What seems now puzzling indeed was less strange in e.g. the 60s. For by then in cell physiology and biophysics a theory formation was in full swing in which concepts and methods focusing on free diffusion in aqueous solution had the day – even though it was known also then that '*the cytoplasm has the consistency of a gel*' (Pollack & Reitz 2001, p.886; see for alternatives their a.o. contributions to Mentré (ed) 2001). Also in e.g. physical chemistry theory builders more often than not extrapolated from 'infinite dilution', to arrive at concepts and theories that got taught widely, yet never were true outside the very dilute range (Ninham 1999, 2006).
As to enzymology, this experienced something like a growth spurt in those years. That is one reason – that is of course not standing on its own – why for a time also enzymologists forgot the difference between the 'crowded' cell and artificial dilute solution, e.g. in their kinetic experiments (something hampering nitrogenase/biological nitrogen fixation studies greatly). We meet here the strong wish to 'use' methods from laboratory chemistry - on the way conceptually reducing reality around us to a system as '*claire et distincte*' as a test tube…

N fertilizer application then conditioned a precipitous decline both of symbiotic N-fixation (Vance 1998) and of effective mycorrhizal symbioses (Larsen et al. 2007), and so the HYVs and other such proud products of research executed within the post-war paradigm originated in concepts and methods that were adverse to (the careful use of) those natural nutrient resources. Recently the paradigm got breached, but that took some half-century to arrive. We now will look at some decisive episodes that brought the change.

5.2. Missing the obvious

February 1960 Griffith reported in Nature on the nitrate contents of herbage as resulting from fertilizing with ammonium sulphate. As research into nitrate accumulation in plants had a long history, it had been reported before already that nitrate-N contents from 0,07% on were toxic to the animal. Griffith pointed to the fact that such contents were being reached when fertilizing with some where between 100 and 150 kg ammonium-N pro ha. In the same year 1960 Turtschin et al. at the International Congress of Soil Science (Madison USA) reported the formation of nitro-compounds in soil organic matter from recent plant residues under the influence of ammonium sulphate fertilization.

Cp. Turtschin et al. 1960, employing 15-N. The method used was standard: after destillation of ammonia formed by caustic hydrolysation of the probe, they reduced the residue with Dewarda alloy and obtained once more a very considerable amount of ammonia. Note that there was a wider awareness of problems connected with fertilizer application. For example, during the 50s it had been shown already that urea fertilization easily led to nitrite accumulation, Court et al. 1964 (Pt.I). Research into e.g. nitrite's influence in soil and plant would have been perfectly normal.
Yet, as it was, even nitrate accumulation was allowed to linger on. In maize it reaches easily some 30% of total N (e.g. Sakala et al. 2000), and that although with careful practice it is nil (e.g. Hetier et al. 1980). The problems are manifest especially in nutrient solution-based horticulture of vegetables, that even will lead to 50% nitrate-N, and is evidently a result of interference with the malate metabolism (Blom-Zandstra 1990). Safety or waste prevention is not in sight, within this nutrient paradigm (Gruda & Schnitzler 2006).

There was, in short, ample evidence of nitrate and nitrite in plant and soil as a consequence of fertilizer, with as a result nitr(os)ation processes at work in plant and soil. Moreover simple but robust analytical methods had been developed that allowed a close investigation of those processes (e.g. chromatographic methods). Yet hardly anything of the kind did ensue.

A history of non-reception of signals: An early report on the high emissions of nitrogen dioxide from silage that result from the high fertilizer gifts of the post war period is Grayson 1956. For a recent report see Maw et al. 2002, for in-planta mechanisms see e.g. Bethke et al. 2004. For overviews of early research on nitrate/nitrite in plants from 1907 on, as culminat-ing in a.o. Schuphan's researches from the 50s till the 70s, see Sen 1974 Pt.III.A and Lorentz 1978. Sokolovsky et al. 1966 (see also Riordan et al. 1966) using tetranitromethane as an agent studied tyrosine nitration in enzymes, yet other such studies had to wait till the 90s (Oshima et al. 1990). From the 1960s on there was research into (carcinogenic) nitrosamine formation under the influence of nitrites in foods and feeds (and including the previous reduction of nitrate to nitrite). This would normally have broadened into wider nitr(os)ation research, but with nitrosamine formation largely neglected by the agricultural research circuit, 'scientific progress' got cut short once more.
But then came the recognition of the vasodilatory roles of NO at the end of the 80s (some recent studies: Busse & Fleming 2000, Vita 2002, Wyatt et al. 2004). That was the start of a real surge into NO's physiological roles, as well as into the regulatory and disturbing roles of nitr(os)ating agents in general ('nitrosative stress', Hausladen & Stamler 1999). From Zhao et al. 2002 it is clear that DNA damage – possibly from the use of feeds containing nitrate/nitrite - is present in calf thymus DNA. Since the discovery by Veleminsky & Gichner in 1966 of mutagenesis by nitrosamines in Arabidopsis, as well as known chemical mutagenesis by deamination of bases in DNA/RNA with nitrous acid (Perutz 1962 p.142f.), the need was there to take a close look at nitrate/nitrite metabolic interactions in plants and vegetable products, as well as in e.g. the ruminant organism. Yet, research had to wait for the medical interest into 'NO', and only in the present this intricate field is being mapped (see Tsikas 2006 for a thorough analytical chemistry discussion).

Returning to Griffith 1960 we learn that he also stressed that nitrate was the chief contributor to so-called 'crude protein', when the latter increased above a certain percentage. That is, 'crude protein' was a misleading term and analytical methods ought to be used instead that allowed real chemical speciation. Where they were applied, e.g. in the investigations of Schuphan stretching from the 50s to the 70s (see Hodge 1981), it was quite apparent that a rise in 'crude protein' with ever higher fertilizer input covered up important quality losses (e.g. as to essential amino acids).

In spite of such reports, main-line agricultural research continued using ‘crude protein’ not only, but also focused on increasing fertilizer use above the (too) high values indicated. In fact the Wageningen professor ‘t Hart started exactly in 1960 with experiments with high fertilizer gifts, on the Dutch Fertilizer Industry experiment station in Vaassen founded in that year (Iwema & ‘t Hart 1972/1974). His low-N gifts amounted to 200 kg/ha, his high-N gifts to 600 kg/ha, and there were no comparisons with herbage originating in pastures managed in the largely organic ways of traditional agriculture. The health problems that showed up in ruminants were considerable, especially such in connection with fertility and reproduction. And yet ‘t Hart summarizes at the end of the publication: ‘*With good farm management …. the fertilizer gift can be increased without objections*’. Puzzling as this pronouncement may be when considered from ‘t Hart’s own research as well as in its wider historic-scientific frame (e.g. Kohnke & Vestal 1948), it yet made some sense within the government-supported research framework of the post-war decades. For in that framework ammonium and nitrate were considered the only ‘natural’ N-nutrients of the plant, while organic fertilizer (with its high labor intensity) was considered old lore.

> We will see in Ch.8 that after WW II Rothamsted researcher Bremner did some solid research in soil organic matter using specifying methods like chromatography, only to abandon this approach in the mid-50s and revert to the non-specifying methods that were used by mainline agricultural research. This regress was considered progress in those years, because it allowed to substitute a focus at mineral nutrients as ‘the real thing’, instead of being ‘held up’ by the intricacies of explorative soil organic-chemical research.

Yet note that there was no question of a careful comparison of the industrial fertilizer-based herbage operations with the organic ones predominating before. **Main-line research like ‘t Hart’s solidly started from a rupture with these ‘old’ practices, not from a careful comparison of newly proposed (high-fertilizer) practices with farming practices that had proven their worth a long time**. And so ‘t Hart does not even mention studies comparing industrial fertilizer-based feeding with its traditional equivalents. Yet such studies in the meantime had culminated in Aehnelt & Hahn’s 1973 publication in the Tierärztl. Umschau: ‘*Fruchtbarkeit der Tiere – eine Möglichkeit zur biologischen Qualitätsprüfung von Futter- und Nahrungsmittel*?’. I quote Hodge’s (1981 p.219) summary:

> ‘*[they] demonstrated the comparative effects of conventionally and organically fertilized fodder on the fertility of animals. In bulls given fodder from fertilized land, considerable reductions in semen quality were found, and these effects could be reversed or prevented by using fodder from manured pastures. Similar experiments in female rabbits, fed largely on vegetables produced by conventional or organic methods, showed detrimental changes in a number of fertility characteristics in those animals in those animals fed on the conventional procedure*’.

The recent surge in medical NO-research opened up a window to likely mechanisms of the reproduction- and fertility-related problems (e.g. Olson 1999). But already beginning 1970s there existed a sizeable body of research into reproduction-related aspects of high fertilizer use. It was a body of research that got still enlarged in later years, and that Haynes (1986 p.398/99) summarized with the words

> ‘*there are numerous reports of ruminants dying of* NO_2^- *intoxication after they ingested plants containing high amounts of* NO_3^-*…. Reduced milk production in cows, increased abortion in cattle, and vitamin A deficiency have also been implicated in animals feeding on silage or pasture forage containing excessive amounts of* NO_3^-*…*’

By then nitrate accumulation research, starting especially from Schuphan's 1950s researches, had indicated the need to be careful indeed with any but low fertilizer gifts (cp. Haynes' overview, l.c. p.399 f.). Why then was for authors like 't Hart the only discernable way-forward exactly that of increasing industrial fertilizer gift, at the same time discarding the soil fertility building practices of traditional agriculture?

For sure the institutional framework of his researches clearly defines these as exercises in 'functional rationality' (*sensu* Mannheim). With industry and government here uniquely at one in post-war decades, he hardly had another choice than that of industrial product research. Yet he concurred because he himself thought this the only scientific entry to crop growth enhancement. For he adhered to this puzzling conviction of those decades that (in Haynes' words) '*Nitrate is the major form of N absorbed by plants regardless of the source of applied N…*'. Within that very limited frame of mind organic fertilizers are simply inferior nitrate suppliers, with a supply that offers no perspective to fine-tuning. The way forward was to search for ways to lessen nitrate accumulation in spite of greatly enhanced fertilizer gifts. Those 'laymen' that diasagreed just made his responsible labors unduly heavy…

The frame of mind is exemplified in another lecture in the same 1972 series in Wageningen in which also 't Hart presented his results. This is the lecture by de Wit, an author who for decades would figure as spokesman for 'industrial agriculture' (de Wit 1972/1974). He starts from an assumption of 20 kg N/ha coming available '*as a consequence of natural processes*' in agricultural soils, a value that is low compared with some of the long-term field trials well known by then; it apparently derives from 'mineralization' experiments of some sort (but de Wit does not mention the great uncertainties and generally unsatisfactory character of those experiments). From the low value indicated he derives at an upper limit of 1000 kg wheat pro ha pro year, in traditional agriculture only to be increased from the application of animal manures and from rotation with nitrogen fixing crops. Yet he is silent about the fact that in the Netherlands at about 1930 primarily with the latter 'organic' means (and a then still very limited use of mineral fertilizer) yields of 3000 kg/ha had become common already (van der Paauw 1960).

Then he refers to the '*costs of transport of organic manures*' and stresses that '*over some tens of kilometers [these] are greater already than the cost for factory production of the nitrogen and minerals contained in the manures*'. A statement that is not about farming as conceived up to then, given the fact that in small-scale mixed farming transport ever is to nearby fields. A statement above all that completely neglects soil: again this equation of 'soil' with 'industrial nutrient solution' that we met repeatedly already.
Towards the end of his lecture De Wit confidently refers to some single experiments showing '*that N-use with yields of 20.000 kg grass/ha is hardly greater than with yields that are 40% lower*'. With adaption of the fertilizer gift to the growth of the grass '*it shows up that nearly 100 percent uptake [of fertilizer] is reached*'. No less self confident he insists that aiming at a lower yield level implicates a less careful approach and so will lead to higher losses (leaching and denitrification) - a bizarre way to accuse the traditional farmer of a lack of care... Note also that the more-than-proportionate increase of losses with the increase in mineral fertilizer gifts, investigated extensively from the 1920s on (and affirmed by 15-N studies, cp. Macvicar 1957), is not even so much as mentioned.

This influential researcher certainly was aware of the problems indicated, including those of feeds, yet did not allow any doubts about the dominant paradigm. Toxic nitrate accumulation, for example, was translated by him in Mg deficiency as induced by the high K that usually is accompanying high nitrate fertilizer, to be relieved by e.g. injecting Mg salts (Black 1968 p.436/7). This seems an odd way to approach an organism, and indeed by 1962 within veterinarian research the real low-Mg-tetany had been clearly distinguished already from e.g. urea/ammonium-tetany and other forms of tetany (Hendriks 1962 p.2 f.). It probably is best to see de Wit's 're-wording' of nitrate toxicity as an expression of an extremely narrow, but broadly accepted research paradigm. After all, mainline researchers when studying the effects of feeding heavily fertilized grass on ruminants were *all* limiting themselves to this supposed 'mineral balance' (Mudd 1970 and refs.). **With those mainliners one looks in vain for toxicological or biochemical research with the methods that by then were nearly routine already**.

Note that is not just that de Wit presents a 'paradigmatic construct' - that will at times happen in any discipline. But puzzling is that neither in the example given nor in his 1972 lecture we can trace receptiveness for new phenomena and for non-paradigmatic signals. Not even for important conceptual and methodical developments in neighboring disciplines (e.g. the use of chromatographic and electrophoretic methods in clinical and toxicological research that allow valuable speciation – see Ch.8). And in that lack of receptiveness he seems at one with main-line research into the effects of high fertilizer doses on feeds and animals.

Of course, this lack of receptiveness was part and parcel of the institutional surroundings in which de Wit and colleagues were doing their research. The government and its experts were sure that industrial fertiliser made the difference between want and plenty for mankind, so the 'best minds' were asked to dedicate themselves to research focussing at its application. The younger generation of researchers was indeed *dedicating* itself to the cause, with all of their energy and intellect. They were quite unconscious of the fact that the path had been indicated from false premises.
Next the common ideals/ideology made for a 'closed research frontier'. With the goals determined by technocracy, and research dominated by its functional rationality, no time was lost in intercourse with 'traditionalists' like the farmers of old with their organic methods. There is a perfect parallel here with economists like Tinbergen who were enthusiastically supporting government in its planning efforts. Quite sure that their paradigm was superior to any 'non-modern' ones, and eager to ascertain the good foor their fellow human beings, also those economists 'lost no time' in considering 'non-progressive' options.

If we read de Wit's lecture after more then three decades we realize that it was shaky indeed – and yet it was received enthusiastically. Two things stand out in it:

(a) the near-equation of soil fertility with (industrial) mineral fertilizer nutrients
(b) the overarching aim to reduce on-farm labor by mechanization and to further structure a high-external-input agriculture.

Apparently it was De Wit's avowal of the common faith in industrialization also of agriculture that made for an enthusiastic reception of his lecture.

The net result of this adherence to a common faith - that by then had been institutionalized to the exclusion of any other options - was that any explorations outside the sanctioned paradigm were 'unbelievable', e.g. to the high officials deciding about financing agricultural research's projects. Note in this connection that mainline agricultural research was strictly managed top-down by such officials – and that this was accepted by the research circuit as entirely self-evident (e.g. Coolman 1974). As a result we had to wait for investigators elsewhere to initiate relevant research.

5.3. Nitrates, nitrites, and nitr(os)ation

After the first reports of nitrosamines in foods and feeds and their carcinogenic potential, already in the 60s the formation of such compounds from nitrite and secondary amines in gastric juice was demonstrated (cp. Sen 1974 Pt.III.C). Then within a few years many other ways were discovered in which their in-vivo formation was likely, e.g. through involvement of microbial agents (l.c. Pt.III.D and Challis & Challis 1982 p.1173). Soon the conclusion was unequivocal, as summarized by Challis & Challis in their authoritative review (l.c. p.1199):

> '*Most [N-nitrosamines] show carcinogenic action and no test species … has proven resitant to N-nitrosamine-induced cancer. These results have been thoroughly documented and it is clear that this class of compounds has many special features as a carcinogen*'.

Next there was a continuous line of chemical analytical method development to get a grip on e.g. nonvolatile nitrosamine as well as on nitrosamide determinations (cp. Sen et al. 1984). On the way some minor shocks were the discovery of nitrosamines in bacon (cp. Skrypetz et al 1984) and especially that in rubber baby-bottle nipples (cp. Havery & Fazio 1984). Still the research was largely limited to a few laboratories, and many applications of nitrosamines are only now coming under scrutiny (e.g. Raksit & Johri 2001). Main-line agricultural research as a whole did not link up, nor did standard setting for e.g. drinking water, with some decades delay in urgent measures as a result. As Vermeer & van Maanen express it in their review (2001 p.105):

> '*The main conclusion of the review is that the risk of endogenous formation of carcinogenic N-nitrosamines, as well as other adverse health effects of nitrate exposure, have to be taken more seriously into account when setting standards for nitrate*'.

Indeed within the present wider frame of burgeoning NO-research the mutagenic potential of dietary nitrate – long neglected, if not denied – was rediscovered and its carcinogenic effect at e.g. the gastroesophageal junction proven.

Nitrosation and cancers. The group of McColl did much to prove the causal connection: Iijima et al. 2002, Winter et al. 2007, Clemons et al. 2007. Note that by then cancer research had focused on the roles of NO and related active N-compounds (e.g. Lala 1998/ 1999 and other contributions in the same issue of the Cancer and Metastasis Reviews). Also the connection between colorectal cancers and the formation of N-nitrosoamines from certain meats and saucages was demonstrated anew, e.g. Silvester et al. 1997, la Vecchia et al. 1997, de Stefani 1998, Haorah 2001. For a good epidemiological review see Norat & Riboli 2001 (displaying quite some progress since Eichholzer & Gutzwiller 1998). Lagiou et al. 2002 acknowledge epidemiology's limitations and show that these do not preclude the use of preventive nutritional approaches.
Dietary polyphenols are known for their anti-oxidant and anti-nitrosative action, cp. de Bruyne et al. 1999 and Packer (ed) 2001 for some overviews, and Zakhary et al. 1994, Dion et al. 1997, and Torres y Torres & Rosazza 2001 for some examples. Also this subject profited from the surge in NO-research: after a delay of some four decades DNA damage by nitrite and its in-vivo products, and the protection offered by dietary phenols, became a research focus, e.g. Zhao et al. 2001. But note that: (a) conventional fertilizer use tends to suppress important aspects of polyphenolics formation, and (b) reductive action sometimes leads to NO that then can diffuse to e.g. enzymes and disable them.

This research brought a focus on NO's roles in fertility and reproduction, shedding new light on the long neglected influence of nitrate/nitrite on e.g. ruminants' reproductive health. But note that it came only after a delay of some four decades. The inability of mainline agricultural researchers like ‘t Hart to admit the obvious is a chief reason for that delay.

> Cp. Olson 1999 for NO’s roles in the ovary and Garbán et al. 2004 for its role in estrogen-dependent gene transcription). Only now research like that of Iannaccone 1984 (mouse embryo pre-implantation exposure to methylnitrosourea MNSU) started receiving the attention it deserved (Rieger 1997). Striking was Iannaccone’s demonstration that the 50% lethal dose for live birth, after implant of treated blastocyte was only 4,7 ng/ml, as compared with a direct 50% lethal dose of 4,2 mg/ml. That is, MNSU’s reproductory toxicity is 6 orders of magnitude higher than its direct toxicity… Next, offspring developed from exposed embryos still had thrice the crude mortality rate of unexposed mice, in spite of the fact that neither gross dys-morphogenic effects nor major histological abnormalities were associated with exposure. And so the reproductive toxicities of these nitroso-compounds became a subject of active research (e.g. Iona et al. 2002).

A burgeoning research field always delivers a number of results built largely from enthusiasm, yet, as to research into the metabolism and roles of NO and related compounds we are lucky to have some incisive publications on the (bio)chemical aspects of it all.

> **NO: some (bio)chemical and analytical publications:**
> For standard-setting publications see Wink et al. 2000, Nedospasov 2002, Miranda et al. 2005, Shapiro 2005, Thomas et al. 2006, van Faassen & Vanin 2007a/b, Vanin & van Faassen 2007, Thatcher 2007. Method development/characterizations in protein nitr(os)ation research: Ischiropoulos 1998, Aulak et al. 2001, Lehnig 2001, Cardenas & Packer (eds) 2002, Espey et al. 2002, Chen et al. 2004, Franze et al. 2004, Sala et al. 2004, Mani & Moore 2005, Radi 2005.
> See also in Packer & Cardenas (eds) 2005: e.g. Xu et al. 2005, Jaffrey 2005, Jourd’heuil et al 2005, Nicholls et al 2005, Turko & Murad 2005, Zhang et al. 2005. Most important is Tsikas’ 2006 critical review. The recent character of this surge of research does not change the fact that important aspects of it all could have been investigated in the 60s already - e.g. following up on Tschurtchin et al.’s report – because a good number of thorough chemical publications mostly from the 1950s had prepared the way (see Thatcher 2007).

And so Shapiro in his 2005 review ‘*Nitric oxide signaling in plants*’ arrives at the sober conclusion (p.385):

> ‘*The easily cited titles that fill this literature today of the form, “NO does X”, will likely give way to complex discussions of kinetic rate constants, transport phenomena, and signaling cross-talk. Evaluation of this literature will require the nuanced approach advocated here, with full attention given to artifacts, caveats, and alternative explanations*’.

Significantly, none of the nuanced publications allows for the conclusion that nitrate ‘is just a natural nutrient and so quite harmless’. For it is quite evident – and widely recognized – that the regulatory roles of ‘NO’ can be disrupted by e.g. local amounts of nitr(os)ating agents outside the limits of the regulatory systems.

Also with plants fertility is easily hampered by ‘NO’ (Shapiro 2005 Pt.H.4) and research along the ‘NO’-line is indicated. Likewise research into long standing observations like nitrate interference with iron metabolism, xylem formation and grain dormancy (e.g. on-ear sprouting) will possibly receive a new impetus from ‘NO’ research. For it is evident that the high post-war fertilizer applications are inductive to the formation of comparatively large concentrations and amounts of nitr(os)ating agents.

The discovery of regulatory roles for 'NO' makes research into their disruption an urgent matter. For if we look at medical 'NO' research, then it is clear that such disruption plays an impressive role indeed, connected as it is with important neuro-degenerative diseases like Parkinson and Alzheimer, with type 1 diabetes and rheumatoid arthritis, and with many other debilitating diseases. Evidently a real 'precision agriculture' is in need of close attention to **mineral fertilizer's possible roles in the disruption of soil, plant and animal life.**

> **Mineral-N – the end of innocence:**
> As to 'NO' and the diseases indicated cp. Weinberg et al. 2007 for arthritis; Burkhart & Kolb 2000 for type 1 diabetes; Chung et al. 2005, Ebadi et al. 2005 and Halliwell & Gutteridge 2007 for the neurodegenerative diseases. Note that in those cases close research into diet-related factors now is indicated, not the least because of the reappearance of dietary nitrate in saliva and its reduction in the oral cavity to nitrite. Earlier dismissals did stem from the non-consideration of differences between humans and experimental animals (no reappearance of nitrate in saliva with the rat) and from non-consideration of NO production when ascorbic acid 'consumes' nitrous acid in de stomach.
> For the nitrate interferences with plant life indicated cp. Barceló et al. 2002 and Gabaldón et al. 2004 for xylem formation. Refer to Simpson 1990 and Lenton 2001 §VII for on-ear sprouting (as a problem especially of industrial agriculture) and to Bethke et al. 2004b & b and Desel & Krupinska 2005 for breaking of dormancy. Some publications pointing to NO interfering with Fe uptake & metabolism: D'Autréaux et al. 2002, Murgia et al. 2004, Shapiro 2005 (Pt.IV.D), and esp. div. contributions to van Faassen & Vanin (eds) 2007. Publications like An et al. 2005 can open a window on excessive growth/weak tissues consequences of mineral-N fertilizer use.

Yet as it was, in post-war decades technological optimism was rampant everywhere, greatly influencing also e.g. experiment interpretation by established researchers. And so we see Kolenbrander interpret his lysimeter experiments into nitrate leaching in clayey soils in a generalizing way hardly warranted, in the eyes of posterity, by the limited experimental facts (Kolenbrander 1969). The renowned researcher Quispel – not from the agricultural research circuit – was careful to point it out (Quispel 1974 p.4): '*Of course this does not assure that under less controlled conditions, under other edaphic and climatic circumstances, leaching of excess nitrates might not lead to serious problems*'.
But note that even Quispel was prone to think of increasing 'control' as a way out of the problems, and so was not ready to send Kolenbrander 'back to the drawing board'.

> That we cannot afford to be complacent any longer as to industrial fertilizer's problematic roles in food and health is apparent also from the peculiar role that the build-up of resistance to nitrosative stress – as executed by the prospective host – has in the rise of multi-resistant *Staphylococcus aureus* or *Mycobacterium tuberculosis*. Here our excessive nitrate use could have accelerated the selection for increased flavohameoblobins protecting microbes against nitrosative stress exerted by e.g. activated macrophages towards microbial invaders: Richardson, Libby & Fang 2008 resp. Rhee et al. 2005, and esp. Nobre et al. 2008 and Ascenzi & Fisca 2008.

Scientific research as a whole received its immense post-war impetus from that belief in (the possibility of) 'control', so it was something uniting researchers from divergent disciplines. For a time it even united policy makers and researchers from all over the globe, giving them the taste of the 'Kingdom of Man' that seemed at hand (we need 'religious language' to describe the common enthusiasm of the age). The 1950s and 1960s abounded with prophecies about this 'Kingdom' (think of its 'limitless energy' projections) and researchers saw

themselves in the vanguard of the noble effort to bring it near. More than that, policy makers and the wider public expected them to fulfill exactly that role.
The careful search for limits, that is always at the heart of all good science and technology, was not part of that expectation. With society itself requesting its experts to be ‘out of balance’, these were indeed the decades of ‘science fiction’. Agricultural research is of course not the only discipline where we still experience the flywheel effect of the expect-ations of these high days of technocracy. (E.g. our energy and transport policies as a whole are an expression of our ongoing inability to transcend the bounds of technocracy).

As to agricultural research, the belief in ‘control’ in the 60s and 70s motivated some bright researchers to focus at nutrient modeling in agricultural research. This was the progressive research of the age, and policy makers were ready to finance this token piece of responsible research. With researchers assuming that their research started from first principles, traditional farming was hardly interesting even as a historical phenomenon. Historically farming had ever been of an ‘organic’ character: consider e.g. its great diversity of approaches in legume use (Ambrosoli 1997). This historical capital was not mined by mainline researchers, but discarded.
Virtanen for example, for whom ‘traditional farming’ had been a starting point for his researches, saw his work become incommensurable with the post-war paradigm. Post-war Finland certainly could have become a centre of renewal in agricultural research: it was a scientifically advanced nation, a country with renewed interest in people-centered agriculture (because of resettlement of the great number of displaced people, Finland 1948), and politically neutral.
Still, machine-centered agriculture with its queer concept of ‘productivity’ (Soule & Piper 1992 p.76f.) had the day. Even agroforestry had to be re-discovered and up till this day is outside the industrial paradigm of main-line research and policy.

In short, we have ample reason to characterize the 50s till 70s with their ardent faith in technocracy as a historic bottleneck, not the least as to agricultural research. For sure, every period ‘reads the facts’ with the glasses provided by its own paradigm, but this period had an extremely narrow ‘frame of mind’ that contrasts in a grotesque way with its ‘limitless’ aims.

5.4. History, again

I recall that the discovery of the mutagenic qualities of nitrosamines (e.g. Puretz 1962; see Zhao et al. 2001 for a recent account) already in the 60s led to relevant investigations also in the plant realm (Veleminsky & Gichner 1966). Mainline agricultural research did not respond, but some other research disciplines did.

Unicellular organisms being easiest for a start, in due time one of the organisms to which nitrosamines’ mutagenic qualities got purposely applied is the (N_2-fixing) cyanobacterium *Anabaena variabilis*. Here it was especially the (oxygen) sensitivity of its N_2-fixation that showed up in diverse ways in the mutants. But it was only recently that the possible derivation of nitrosamines from nitrite in the organism brought an awareness also with plant researchers (see Morot-Gaudry-Talarmain et al. 2002 p.714) that

> ‘*It is not excluded that nitrite accumulation leads to intracellular generation of reactive-nitrogen species capable of deaminating purines in DNA*’.

Here the connection with the *Anabaena* research is immediately evident: what if our post-war high-fertilizer breeding indeed helped create varieties with disrupted nitrogen fixing and mycorrhizal symbioses abilities? It is clear that not only *Anabaena var.*, but also *Nostoc muscorum*, for example, lost its nitrogen fixing ability after prolonged culture in media containing nitrate (Rogers 1987 p.43 and refs.). As to genetic disruption of legumes by nitrate, think here of the consistent efforts in post-war decades of farmers and breeders to have it both ways with legumes – the use of both industrial fertilizer and biological N_2-fixation by the crop. Here and in other plants, nitrite formation etc. in the wrong tissue at the wrong time is quite conceivable.

At the wrong time at the wrong place: The *Anabaena var.* nitrosamine mutagenesis researches indicated got started with Haury & Wolk 1978, to be continued by Gotto et al. 1979, Grillo et al. 1979, and Murry 1983. Van Baalen 1987 reviewed the researches for a broad expert public. Giller et al. 1994 (p.187) and Sessitch et al. 2002 (p.358) are pertinent about modern breeding selecting against efficient N_2-fixation association, both with legumes and with other crops.
As to possible 'mechanisms', Bethke et al. 2004a show that the apoplast in barley grains is sufficiently acidic for nitrite to be non-enzymatically converted into NO, with that membrane-diffusable compound then acting as a signal in e.g. dormancy breaking. In fact when nitrite concentrations are high enough, under influence especially of fertilizer, ascorbic acid acting as a reductant is greatly promoting NO formation – the same we find in the human intestine. Refer to Planchet et al. 2005 for another study of in-planta NO formation. Non-enzymatic acid-catalyzed nitration, after enzymatic nitrate-to-nitrite reduction, is another possibility (cp. e.g. Rousseau et al. 1997).

Considering the truly excessive mineral-N use in agri- and horticulture leading to nitrate accumulation etc. there is ample opportunity for nitr(os)ation also within reproductive tissues, if only because detoxification potential will get exhausted. And then, disruption of symbioses (or other associations) need not go the genetic or mutagenic way. Early on NO proved to be a very potent inhibitor of nitrogenase. And one does not need think only of NO produced from ammonium fertilizer as a co-product of nitrification: the Rhizobium-rice endophytic association from the long standing Egyptian rotation of berseem and rice proved to change from growth-promoting to stunting due to the endophyte causing NO accumulation where nitrate fertilizer is used.

Plant disruption by NO: As to nitrate accumulation see for example Blom-Zandstra 1990 and Grada & Schnitzler 2006 for lettuce, Sakala et al. 2000 for maize. On exhaustion of nitr(os)ation detoxification potential see e.g. Munro et al. 2007.
See Lockshin & Burris 1965 for early indictment of NO as a potent inhibitor of nitrogenase. For NO as a co-product of nitrification see Skiba, Smith & Fowler 1992. For the rice-berseem rotation see Yanni et al. 2001, for its endophyte causing stunting with mineral fertilizer see Perrine-Walker et al. 2007. The endophyte is here normally in a plant growth promoting (PGP) role that gets inverted by fertilizer. Induction of phenolics production in the plant as against fungal attack is an important aspect of this PGP (e.g. Singh et al. 2002) and also this gets thwarted by the fertilizer gifts.

This fertilizer disruption of sustainable modes of farming could have been known as such a long time: a prominent researcher like Burris in the 50s did research into NO_2 production by silage and in the 60s into NO inhibition of N_2-fixation (cp. Burris 1985). So why did he and most of his generation of agriculture-related researchers miss out on what in retrospect seems obvious enough?
Soybean growing helps us understand the confused situation. From a modest acreage it grew explosively within some decades especially in the US, recently repeated once more in the increase from 200000 ha in 1986 to some 1000000 ha in 2000 in North Dakota alone. With

effective rhizobiae (the symbiotic N_2-fixing microorganisms) as a rule lacking in the 'new' soils also because some non-indigenous variety was sown, with certain soybean varieties it proved profitable to provide mineral-N fertilizer to 'first-time' soybeans (Goos, Johnson & Carr 2001). Moreover to research *within the mineral-N paradigm* the known propensity of many legumes to use soil-N besides biological nitrogen fixation (BNF) could only mean that they used mineral-N nutrition! So US breeders and growers felt relieved from patiently developing and adapting soybean varieties (and if need be rhizobiae) to the local soil: they conceived an 'instant' system that uses both biological fixation (BNF) and mineral-N. Research followed suit, with mineral-N nutrition easily dominating research design even with this nitrogen fixer, and researchers duly pronouncing that for optimal yield of soybean it is important to use both BNF and mineral fertilizer. And so 'fertilizer-resistant' rhizobiae have to be developed – true 'expert knowledge' – but this symbiosis remains insufficient and extra mineral nutrition is 'needed'.

Soybean research designs. Soybean establishment: inoculate preceding crop as well as soybean with rhizobia, Goos et al. 2001. Establishment & enhancement: co-inoculate soybean with Bradyrhizobium and Azospirillum, Groppa et al. 1998 and Molla et al. 2001. Sufficiency of BNF is apparent also with other legumes, e.g. leguminous trees (Bala & Giller 2001). Note that mycorrhization of rhizobial soybean a.o. legumes ought to be the rule. Yet, as to Europe and the US, 'mixed' soya nutrition was the rule, following e.g. Harper 1974. The mineral fertilizer concept seemed 'logical', even here, from (1) most plant physiology taking water culture (non-symbiotic) with mineral nutrients for truly fundamental research and (2) noticing that legumes could derive part of their nitrogen from soil and interpreting it within the mineral-N paradigm as feeding on mineral nitrogen (instead of organic-N). See e.g. Eaglesham 1982, de Veau et al. 1992, Ohtake et al. 2001, Fujikake et al 2003, all research that requires disentangling of the agrobiological aspects of soybean from those imposed by the mineral fertilizer paradigm. Such disentangling could have started a long time, seeing that there is quality research like Herridge 1982 available for decades already.

But note: where *soya as a nitrogen fixer* dominated research direction, as in Döbereiner's research in Brazil, BNF proved essentially sufficient and mineral-N nutrition out of order (e.g. Nicholás et al. 2002). Both for establishment and for enhancement of the soya crop an array of BNF possibilities offer themselves to the researcher.
Johanna Döbereiner was a truly famous researcher - see her 1966 Nature publication for a widely known study on legumes – and the Brazilians are grateful indeed that she helped them establish fertilizer-free soya cultivation and continue with fertilizer-free sugar cane cultivation. Yet, research in the US and Europe did not follow her lead. To the contrary, agriculture-related research there expressed doubts about her results and those of her colleagues. But note that research in Brazil (for which see e.g. Döbereiner 1982b) and in other countries in the present is solidly in her line, with criticism of western agricultural research ever more manifest.

The diverse associations and symbioses of plants and micro-organisms require a carefull approach as a unity: development of the crop separate from its symbionts/associates disables the symbiosis/association instead of strengthening it. And yet in the sudden post-war turn towards energy- and chemicals-intensive agriculture, crop development was all of the fertilizer intensive type, with symbioses mosttimes not even coming up as an afterthought. These were decades of 'active neglect' because breeding and cropping went on, but without regard to e.g. mycorrhizal symbioses. As Triplett indicates (1996 p.32/33, ref. to Hetrick et al. 1992/3): '*Unfortunately, the current agricultural practice of heavy phosphorus fertilization has probably resulted in a massive decline in VAM infection of crop plants*'. Generally as a

result of high fertilizer gifts (esp. of N and P) both breeding and farming methods were disabling symbioses. (Discussed in e.g. Douds et al. 1993 & 1995, Hamel 1996, Boddington & Dodd 2000, Kiers et al. 2002).
BNF-symbioses and mycorrhizae being soil-bound, are always local in character, and their development requires local expertise. R&D in some far-away central institution, the type of research that had a monopoly in post-war decades, will not do. Indeed, also here the newly institutionalized research missed out on the chief ingredients of traditional farming systems that had proven their long-term sustainability.

Example: the milpa system. In the milpa system, beans, maize and squash are intercropped (e.g. in Meso-America and the Andes). The system is of such a respectable age that the **co-domestication of beans and maize** in those regions seems likely, Silva et al. 2003. The Rockefeller funded research starting in Mexico during the war neglected this a.o. sustainable systems completely – as did post-war HYV research ensuing from it that was at the center of the Green Revolution. Some researchers educated within the HYV frame but aware of the extent of inter-cropping of e.g. peas and maize tried to study the subject – e.g. Saito 1982 **-** but the results were not very satisfactory where HYVs were being used (e.g. Riggs et al. 2001). Recently the introduction of molecular genetic methods in field characterization also of BNF in the milpa system brought renewed interest, e.g. Estrada et al. 2002.

Research into symbioses in crop plants for decades was only thinly sparsed (something making Johanna Döbereiner's researches only the more notable, cp. Reis et al. 2000). With rhizobial and mycorrhizal symbioses more often than not mutually supporting, it looks like Green Revolution breeding largely severed the connections with exactly those agroecological resources that can help us in regaining a sustainable agriculture. That makes recent research in the line of Döbereiner only the more important, of course.
Still as to the revival of independent research in BNF/GP associations and symbioses there are two caveats. And that is that: (a) commercialization is of limited value here, and (b) history teaches us that premature commercialization can do much harm. For when at the end of the 19th century the function of rhizobiae was discovered, premature commercialization soon was rampant (in Germany and USA). When found out as premature, it did also the study of BNF great harm (cp. Miller & May 1991). Given the society and economy we live in, history could easily repeat itself, with loss of interest for the subject once more as a result. It is quite clear that we cannot again afford such a derailment, but we can avoid it only by fully accepting the truly local character of the natural resources that we want to study and develop. It is a local character that requires local knowledge – with the farmer at center stage.

Research into BNF associations/symbioses: For some such research into microbial BNF and/or growth promoting (GP) 'associations' with maize, wheat, rice, etc, see Elliott et al. 1979, Palus et al. 1996, Perrine et al. 2001, Yanni et al. 2001, Dobbelaere et al. 2002. Renewed interest there is also in stem nodulated legumes (in e.g. rice cropping; Becker et al. 1990, Ladha et al. 1992, James et al. 2001), after some four decades of neglect, e.g. of Johanna Ruinen's superb research into BNF in the phyllosphere (Ruinen 1956 and later), and of most of Döbereiner's research. As to the latter, think of her research into BNF associated with grasses, cp. Reis et al. 2001 (review); for early publications see e.g. Moore 1963 and Dubber 1965, for contemporary research e.g. Lovell et al. 2000 and Hamelin et al. 2002. Recently research into BNF/GP-associations in crops came fully on its own (e.g. McCully 2001) not the least because the careful application of molecular biological methods, as in Perrine et al. 2001 and Wieland et al. 2001, helped researchers regain independence from main-line breeding (at present largely governed by transnationals).

5.5 The changing BNF research culture

The 1952 Lochhead and the Thornton & Meiklejohn 1957 reviews indicate that even in the 50s European research was leading and research from the USA was in a minority position, both as to soil microbiology at large and as to agricultural and soil BNF. In this they affirm the impression that one gains when comparing Virtanen's 1948 and Wilson & Burris' 1953 reviews of biological nitrogen fixation. The comparison shows us also that Virtanen mastered a diversity of sources that Wilson and Burris - leading American BNF researchers of the day - knew second hand only (by way of Chemical Abstracts), or not at all.

> As to **soil microbiology** at large, at the First Int. Congress of Soil Science in 1927 American research as a whole is no match to that of Winogradsky and other Europeans. When consecutively Greaves c.s. (Utah) reaches solid experience with non-symbiotic BNF – cp. Greaves et al. 1940, Hervey & Greaves 1941, Jones & Greaves 1943, Greaves & Bracken 1946 – we see Wilson & Burris 1953 not even mentioning his results and Wilson 1958 simply denying them. Wilson and Burris prove not acquainted with soil microbiological methods of Europeans like Winogradsky, Rossi, Virtanen. When Wilson in '39 in spite of great effort is not able to duplicate Virtanen's results on co-culture of fixers and non-fixers – cp. Wyss & Wilson 1941- he is not willing to consider a fundamental lack of experience with both soil microbiology and traditional agriculture as a chief cause.
> Wilson and Burris miss out on e.g. Winogradsky's warning, repeated by Aso et al. 1938, not to use sugars, but organic acids (preferably dicarboxylic acids), when performing research in e.g. Azotobacters. That is an important reason that they miss out on Azotobacter's ammonia exudation - and it is from that kind of failures that Wilson arrives at his low estimate of non-symbiotic BNF (Wilson 1958). Now note that when, during the 50s, American research becomes dominant, also these sloppy methods become standard-setting: we see sugar feeding of Azotobacters the standard up till Thompson & Skerman 1979 and later – prolonging up till very recently Wilson's low estimate of non-symbiotic BNF.

Indeed except for European refugees most American researchers were hampered by minimal knowledge of foreign languages and cultures (as indicated also in Burris' autobiographical sketch, Burris 1995). World War II brought renewed and intensive contacts between Western European and North American cultures, with the credit that the Americans gained in liberating Western Europe making Western Europeans at large far more receptive than before the war. Add to this the need of desolate Europe for American economic and other assistance and we have a scenario that could well have resulted in cross-fertilization.
Yet the barriers of language and culture were bigger than envisaged by contemporaries of both sides (who at least could converse in English). Add to that the tendency of the nations of all centuries to 'copy' aspects of the culture of the victorious party and we realize that true cross-fertilization was in need of greater investment of empathy and energy (from both sides) than in fact was made.

Robert Burris when he was 80 gave us an autobiographical account of his research and that of his research teams in BNF and related subjects (Burris 1995). Easy-going it gives us an eyewitness account also of American research style in the field, and of the broader American culture in which it all was embedded. This culture, with all of its peculiarities (like any culture), also had a characteristic freedom 'to speak up'. Note e.g. that it was the uncompromising stand of the Methodist bishops in those years (cp. Muelder 1961) *as expressed time and again in typical American rhetorics* that became an important cause of the ultimate downfall of McCarthyism (the ‘witch hunt’ of supposed communists).

As to the **rhetorics of science**, there is e.g. a remarkable difference between the ‘business-like’ language of most Americans at the 1st. Int. Congress of Soil Science in 1927 and the far less ‘hurried’ or pragmatic language of the Europeans there. Of the refugee scientists (from Nazi Germany) conversant both with American and European culture some wrote also about these differences (e.g. Erwin Chargaff). But post-war technocracy supposed its S&T to be a culture-exceeding endeavour and had no sympathy for culture-conscious generalists in the human and natural sciences. In fact, while specialists operating in a historical and cultural vacuum were sought after, generalists in effect got muted - many a refugee scholar among them (e.g. Krohn 1993). Soon the simple take-over of American approaches - including their rhetoric - proved more ‘profitable’ than a process requiring mutual empathy and historical research. In not a few cases this was quite literally so if we take notice of the Marshall Plan finances, but all went surely deeper than just that. The behavioristic approaches in human science are a case in point: their introduction started as soon as the non-consideration of philosophical and historic contributions to a discipline could be presented as e.g. ‘pragmatically justified’. Some literature on rhetoric in the human and the natural sciences: Nelson et al. 1987, Bazerman 1988, Prelli 1989. (But cp. also Latour 1999).

Something of that specific ‘rhetorics’ is evident also from Burris’ autobiographical sketch. But recalling the Europe of the 40s and 50s, it is also apparent that American rhetorics then differed substantially from the kinds of rhetoric acceptable in Europe in those years (before Americanization set in and largely prevailed).

There are some painful incidents in post-war years reminding us of the substantial difference in rhetorics. These include American prosecutors not being able or willing to follow the defendant in certain trials on the continent – cp. that against von Weizsäcker, ambassador to the Vatican, that Churchill himself, following Lord Hallifax who had cooperated closely with von Weizsäcker at the end of the thirties, dubbed ‘*the mistake of the century*’. Richard von Weizsäcker, his son, gave us an extraordinarily valuable account (Weizsäcker 1997 S.112-129):
‘*Im amerikanischen Starfprozess … bekämpfen sich [Ankläger und Verteidiger] nicht nur mit Dokumenten und Argumenten, sondern jede Seite setzt ihre Mittel ein, um die Zeugen der Gegenseite als persönlich unglaubwertig zu entlarven, sie menschlich zu diskutieren. Es ist ein gnadenloser Ringkampf*’. His father had always been a reserved personality in his utteran-ces: ‘*Seinem Wesen gemäß hatte mein Vater sich stets gescheut, den Empfindungen seiner Seele frei sichtbaren Lauf zu geben, die sein Leben von Grund auf bestimmten*’. Now he was faced with an ‘American way’ that squared with his own life time ‘rhetorics’: ‘*Je mehr er empfand, daß ihm jedes Wort im Munde herumgedreht wurde, je mehr er sich von Miß-trauen und gestellten Fallen umgeben fühlte, desto mehr verschloß es im den Mund. … Familie und Freunde bemühten sich, das verletzte Verstummen meines Vaters aufzutauen. … Aber es fiel ihm schwer*’.

From Burris’ sketch it is clear that he was hardly equipped to bridge the two cultures. The more so because he even missed an entire idiom: from the start of his research career there was no link to the farmer, let alone to such farming systems that had proven their sustainability a long time. Here there was an essential difference with e.g. Virtanen’s research as exemplified in his 1938 ‘*Cattle fodder and human nutrition*’.

With the leading role of Europe in BNF research only too evident, in ’54 Burris received a Guggenheim Fellowship to link up with it. The first destination of Burris and his family was Helsinki: there was no doubt that Virtanen’s team was the most prominent research group in agriculture-related BNF. In his 1995 overview Burris gives us a nice account of the things he picked up in his next stay, Cambridge University, but about Helsinki and Virtanen we hardly learn anything, except than Burris relating that Virtanen knew eight or nine languages. Though he tells us '*After a pleasant and productive time in Helsinki, we moved on to Cambridge*' we are completely in the dark as to the 'products'.

There is only one exception: Burris relates of an incident in connection with some piece of Virtanen's research that, supposedly, proved his own research superiority. There's hardly any detail, but Burris is evidently making a joke out of Virtanen, then ends the story with '*Virtanen really was a very nice guy, and his AIV silage did wonders for the diary industry of Finland. It was just that being the only Nobel Prize winner in the country put him on such a high pedestal that others were loath to challenge him, and he even may have come to believe in his infallibility*'. But note, the 1955 celebration volume '*Biochemistry of nitrogen*' honoring Virtanen makes it abundantly clear that the foremost researchers of those years, mainly from Europe, knew Virtanen to be one of them. The book was completed in December '54, and Burris knew all about it.

The best we can say is that Burris failed in his mission: that of linking up with essential aspects of European biochemical BNF research, as well as research in related subjects. (Note that also as to organic-N plant nutrition, root exudates and rhizosphere studies Burris failed to link up with Finnish and wider European research). Quite likely the cultural chasm between Burris and Virtanen was too deep, with a period of about half a year too short to have it bridged. After all, this was Burris' first journey outside the US, with no knowledge of other languages or cultures. '*There are no sciences, only humanities*'. This dictum by the late science historian Reyer Hooykaas proves true also in this piece of BNF research history.

Very human indeed Burris c.s. next spent extra effort in the kind of research that, so they envisaged, was their strength. That was research of the industrial type, as executed in well-furnished, central laboratories. Burris & Wilson's 1957 '*Methods for measurement of nitrogen fixation*' was a milestone on this road. It made them famous thanks to their elaboration of the use of the N-15 isotope in BNF research – but at the same time consolidated the distance from farmer and farming, from its complete silence about experience with BNF in agriculture of old. Moreover its impatience with established methods like gasometric ones (repeated in e.g. Burris 1972 and 1974), that had proven their worth in the hands of researchers like Virtanen, prevented the kind of systematic comparisons that are always urgent with the introduction of a new method (cp. Bock 1985 Kap.5 for the gasometric methods).

From now on for the 'scientific' study of BNF the transition through laboratories like those of Burris would be obligate - the 'translation' of a (social or) natural phenomenon in a laboratory specimen due to method 'standardization' (John Law). Yet this was aborted research, not only because the links with others' research had either not been made or had been severed, but especially because the links with traditional farming and with ecology were missing. Note that at least ¾ of peas in Latin America are grown associated with another crop, mostly maize (Hernández-Bravo 1973). With many centuries of peasant/farmer experience in co-culture (examples in Ambrosoli 1997, Stone 2005), solid research in such BNF-based systems of necessity starts with agro-ethnographic description of those farmer practices. But as far as American BNF research is concerned, already before the war it had no direct links with those practices. With research design thus uninformed at best, its agricultural relevance was uncertain from the start.

In due time the limits of N-15 methods would show up and would enable an assessment of applications. But in the meantime BNF research would come under the spell of a quick assay – that after some decades of intensive application was proven invalid. It is the acetylene assay as developed in the 60s (the decade that saw a fast development of cell-free and reconstituted-enzyme studies of BNF). Its 'rise to power' signals the willingness of the decade to consider evident problems of assays like these – then hardly less visible than now – as things that soon would be mended. Its spirit would rule also the next decade, at the end of which we see Hardy et al. (1979 p.x) confidently declare:

> '*The modern era of biological N_2-fixation research was ushered in by advances at the biochemical level, beginning with the discovery in 1959 by Carnahan and coworkers of a reproducible method for extracting a highly active N_2-fixing enzyme system from the bacterium Clostridium pasteurianum. The 1960s saw a logarithmic increase in interest and activity in biochemical studies, so that by the end of the decade the complex nitrogenase reaction was reasonably well defined, and the enzymes … had yielded to purification and intensive study of their unique properties'.*

The optimism of the age evidently got embodied also in its research into cell-free and reconstituted-enzyme systems. For as soon as a cell-free system had been constructed that 'worked' – that is that could be forced to show some continuity in nitrogen fixation – this without further proof was taken as a copy of the in-vivo system. And this in spite of the evidently makeshift character of the in-vitro system: just consider the fact that enzyme preparations, mechanistic studies and model construction all came to depend on the use of the laboratory & industrial reductant dithionite that has no equivalent in the cell.

It did not take long for this train of research – much of it using advanced physico-chemical and enzymological methods - to discharge into an elaborate model of the mechanism of nitrogen fixation by N_2-fixing enzyme nitrogenase. For a time this model would function as a holdfast in research and education, untill recently also the doubts about this model would grow, from e.g. an awareness that some truly in-vivo reductants did better than dithionite. At present it is safe to say that leading researchers want the model 'back to the drawing board'.

5.6. A hype at the core of 'modern BNF research'

Minchin, Witty and Mytton (1994) in their definitive rejection of the acetylene reduction assay (ARA) for field-based studies of biological nitrogen fixation with legumes write (p.166):

> '*we … conclude that the usefulness of the ARA in field studies is extremely limited*'.

They point to the long known fact that

> '*there is usually a good correlation between fixed N and total dry matter and measurements of simple parameters such as dry weight, grain yield and total N will provide good indications of nitrogenase activity as well as providing agronomically useful data*'.

They stress that

> '*some of the strongest condemnations of the [ARA] come from people who have close links with laboratories in the developing world*':
> the use of the ARA brought estimates of field performance of legumes that were not only erratic, but more often than not several times below the true values, leading to depreciation of this natural resource and to a parallel overrating of industrial fertilizer.

So Minchin et al. ‘*suggest that researchers consider the merits of simple, alternative measurements such as dry weight, yield and total nitrogen*’.

Minchin et al. brought the **errors of the ARA**
to the attention of the BNF research circuit especially in Minchin et al. 1983 and Minchin et al. 1987. The error-prone character of the ARA leading to estimates that are several times too low is also illustrated in Fig.1 of Hunt & Layzell 1993. Somasegaran & Hoben 1994 (hand-book) follows Minchin et al.’s recommendation of simple methods and grant the ARA only a place in an appendix at the back, with a warning. Cp. also Morris et al. 1985 (erratic relation between ARA and 15-N assay) and Warembourg 1993 for rejection of the ARA in field studies or quantitative work.
So what was it that yet made the ARA into a hype?
In what marked the ‘final acceptance’ of the ARA by BNF researchers Burris (1972) was careful to note, as he had done at his first elaboration of the method in 1967: ‘*if one wishes to translate information from acetylene reduction to rates of N_2 fixation, it is necessary to establish the ratio between acetylene reduction and reduction of $^{15}N_2$ or production of ammonia*’. Yet he hardly mentioned other methods that had proven themselves for years already in e.g. field evaluations.
For quite some time such methods got in disrepute (but not for chemical reasons) and only recently they were rehabilitated again. When Burris in ’74 reiterates his warning to establish a conversion factor he speaks of a supposedly superior reduction of acetylene compared with nitrogen (Burris 1974 p.29), initiating others’ statements that ‘as is well known’ nitrogen fixation is less than the ARA indicates. Burris recommends the ARA with the words (l.c.) ‘*The widespread acceptance of the acetylene reduction method as a measure of biological N_2 fixation attests to the usefulness of the method. Because it is simple, inexpensive, and highly sensitive, it has been possible to make numerous measurements, particularly in the field, where earlier methods limited experimentation to a few samples*’.
This last statement signals Burris’ disjunction from the research of e.g. Virtanen and Döbereiner, and from the long-standing uses of legumes by farmers; the first statement gives us a clue how a shabby method yet could come to dominate BNF research for some decades.

Looking back, the introduction and acceptance of the ARA was not just an accident. The earlier publications on nitrogen fixation in cell-free extracts were not yet presumpteous (e.g. Nicholas & Fischer 1960). But then the discovery that a system consisting of such extracts (1), an ATP generating system (2), and a reductant for which mostly dithionite was used (3), could for a somewhat longer time be made to fix nitrogen made investigators like Burris imagine that they had the nitrogenase system in their test tubes. When then some other small molecules proved reducible in the same system they were convinced that they now had convenient laboratory means to follow the system’s performance.

Next when acetylene reduction by soybean nodules (containing the N_2 fixing bacteroids) was discovered, it soon proved possible to construct an acetylene-reducing cell-free system with this legume too. From then on enthusiasm knew no more borders and Hardy et al., from the du Pont Central Research Department, did extensive experiments to establish an acetylene reduction assay: nitrogen fixation became a chemical laboratory phenomenon. (Note that the high standing of such research departments in the 50s and 60s guaranteed respectful attention from researchers everywhere). Hardy et al. (1968 p.1186) stressed that ‘*the C_2H_2-C_4H_4 assay of N_2-fixing activity has undergone extensive development in this laboratory*’ and that as a result it had become ‘*sensitive, universal, specific, rapid, simple, economical, and quantitative*’. And they added: ‘*It is emphasized that the adoption of a consistent procedure by the various disciplines, e.g. soil science, agronomy, marine biology, plant biology, microbiology, and* *biochemistry, which will utilize this method, is essential if valid comparisons are to be made among results obtained from various sources*'.

The conviction that such a method had arrived in the ARA then made researchers use it as the reference method, if not in principle (for many of them subscribed to Burris' warning), then yet for all practical purposes. Yet, the many doubtful qualities could be evident from Hardy et al.'s publication itself (esp. p.1196 f.). What is more, the way they advertised their method ought to have caused any experienced chemist to be on the alert: such 'all-round' methods do not exist in his branch. But for a time enthusiasm overruled hesitation, with as a result that for some decades a method was used as a kind of reference that consistently did the wrong thing: even if it indicated nitrogen fixation it did so with a (severe) underestimate.

An all-round method…. Burris' 1967 publication (Proc.Nat.Acad.Sci.58('67)2071f.) on the use of the ARA in the field was received with great enthusiasm in Nature. Again we see that the admission that '*it is perhaps unwise to rely entirely on this secondary standard*' is followed by an enthusiastic '*The ARA has overriding advantages, however:…*' - Nature 217('68)219. Acetylene reduction by soybean nodules was reported by Koch & Evans 1966, of the cell-free system by Koch, Evans & Russell 1967. Note that the du Pont article (Hardy et al. 1968) was of quite extreme length for the journal Plant Physiology, indicating the great value that editors and others attached to it.
By 1972 the ARA was a 'standard method' (publication in *Methods in Enzymology*) and by 1975 Burris could already compose an insider's history about its development for the International Biological Program publication on BNF by free-living micro-organisms. By then it played an important role in estimations of BNF in diverse ecosystems and so in global budgeting – see Paul 1975 & 1978 – with also here low values as a result. The only positive aspect was that now more researchers looked at BNF than before - cp. Granhall (ed) 1978.

For sure, researchers who were well versed in field research (paddy soil, lake sediment, salt marsh grasses, tropical phyllospheres) noticed time and again that things could be greatly wrong with the ARA. The wish to have it evaluated was growing, yet, its decade long prominence made it difficult to differentiate spurious results from good ones. Of course, indicating robust analytical methods (cp. Bock 1985) as 'overtaken' hardly helped solid comparisons.

Questioning ARA. Researchers reporting faults and inconsistencies of the ARA as a rule tried to apply it in (ecologically diverse) field settings ánd used other methods to check its results. Two collective volumes with quite some examples in each are Granhall (ed) 1978 (deriving from a 1976 conference) and Gibson & Newton (eds) 1981. So it is clear enough that the ARA had quite some critics already in the 70s. We see e.g. Döbereiner & Boddy 1981 present theirs' and other results indicating low values, not alone with e.g. the ARA, but also from e.g. solution culture experiments. Quite significant is their report of very careful nitrogen balance pot studies at IRRI (Manilla) that brought the low values of current methods, esp. the ARA, to light. Yet in the same volume Knowles 1981 in his effort at evaluation is still caught in a futile effort to differentiate trustworthy from false results.

It was only with publications like Herridge 1982 (from Australia, by the way) that criticism as based on valid comparisons could start to shed light on the issue (but note that the ureide method that he used was hardly new): '*In data from our laboratory the [ARA] underestimated fixation by soybeans compared with other techniques, e.g. 50 kg/ha (C_2H_2) vs. 211 kg/ha using the ureide technique*'.

Two things especially stand out from the history of the ARA.

The ***first*** is that the 'spell of the 60s' - the prior conviction that laboratory measurement was the real thing and that phenomena 'in the field' were laboratory-reducible - had to be broken before real-life phenomena like BNF could come in view again. This 'spell of the 60s' was at the center of post-war enthusiasm (here we need the religious concept) for (re)constructing both nature and society 'guided by science and technology'. An enthusiasm that had its unreflected connections with the centralism of war and reconstruction. It is from this 'spell' that the ARA was so widely welcomed and published despite its shoddy character. Note that research that did not accord with this assumption of 'central laboratory reducibility', but emphasized looking at the phenomena 'in the field' above all, for the duration of the 'spell' was something of an anachronism.
That brings us to the ***second*** point: for a time also the research that was closest to real-life BNF (in agriculture and ecosystems) was considered secondary, if not suspect. Ruinen's superb research into phyllosphere BNF, with publications stretching from 1956 to 1975, could be discredited by van Hove 1976 from ARA measurements, apparently justifying the fact that no permanent research post had been provided for her (she became director of a highschool). Döbereiner's researches got discredited time and again from some ARA use as 'reference' (e.g. van Berkum et al. 1982). Luckily her research 'niche' was in Brazil, and European-American 'general opinion' did not reach till there.

For indeed for a time also prominent researchers like W.D.P.Stewart were under the spell of the 'laboratory approach' Stewart thought it due, for example, to grant about half of the International Biological Program publication edited by him to the ARA plus nitrogenase research (Stewart (ed) 1975). Indeed there was something very impressive with this nitro-genase research and we now turn to its role in the period of history under consideration.

5.7. Systemic doubts about in-vitro nitrogenase

As indicated, in the mid-60s enthusiasm about the just-discovered laboratory cell-free systems 'for N_2 fixation' rose immensely. Also in the mid-60s the first stable complexes of N_2 with a transition metal were discovered. I remember following the research in these complexes as a student – and noticing in due time the conclusion of well-versed researchers that these complexes were not an inroad into nitrogen fixation.
Still those were years of great hopes and some thorough research programs into chemical nitrogen fixation possibilities had their origins in them. BNF research got connected in that way with chemical research that had truly advanced methods at its disposal, so it is understandable at least that great expectations were raised. These did not materialize, but those advanced methods in the consecutive decades for sure were helpful in probing the chemical basis of the many suggestions that were being offered.
So there was a start that was promising in several ways. Yet, when we look closely at it, overall research was a mixed bag, with the total not stronger than its weakest links.

Chemical contributions to BNF studies: Postgate 1971 (p.300) refers to the discovery of transition metal dinitrogen complexes as 'providing the first sound chemical basis for proposing a direct prosthetic role for iron or molybdenum in the functioning of nitrogenase'. In a way he was right even if by then the doubts about these complexes as models had increased, for from then on research methods were applied that allowed to differentiate between experiment-derived suggestions and experimental facts. Chatt 1977, Leigh 1977 and McKenna 1980 a.o. provide the links to the strictly chemical background studies of the 70s. Schrock is one of those who already in the 70s used synthesis to explore chemical 'reasons' for BNF (cp. Peters & Mehn 2006 p.90f., and Schrock 2005 for a recent account).

Another recent example is Barney et al. 2006; see Malinak & Coucouvanis 2001 for a review. Note the link to nitrogenase functioning still has to be proven even when an exiting synthetic discovery has been made. Refer to Lawson & Smith 2002, Holland 2003, Igarashi & Seefeldt 2003, Rees et al. 2005 and Peters & Mehn 2006 for reviews covering chemical background studies plus discussion of mechanistic proposals.

Understandably, for a time preparation and purification of component enzymes asked most of the attention of the chemists/enzymologists concerned: in the section '*Nitrogen fixation*' in Methods in Enzymology Vol.24 (1972) we find the result of much hard work. With that work accomplished, it was easy to forget the step on which all depends: the close comparison of the in-vitro 're-constructed' systems and their in-vivo counterparts… And indeed: researchers skipped analyzing the differences at systems level and immediately moved on to mechanistic studies instead. From the start nitrogenase research was hampered by the unproven assumpt-ion of identity between in-vitro and in-vivo systems, while it was exactly at this system level that essential developments soon were to come in enzymology itself – bringing the risk that nitrogenase research inadvertently would live from short-lived approaches.

Such an essential change was Minton's introduction of a quantitative theory of 'molecular crowding' (the concept itself was hardly new; cp. Minton 1983 for an earlier contribution). Its results are important especially for our notion of the macromolecular cell constituents, enzymes prominent among them. I quote Ellis (2007 p.8) to give a feel:

> '*…let us consider the association of two 40 kDa monomers into a dimer. Suppose that the association constant for this oligomerization in water is 1,0. Crowding theory predicts that the value of this constant inside a cell of E.coli will be between 8 and 40, depending on what specific volume of the protein is assumed. If we suppose that the dimers can form a homotetramer, the effect is even larger – the association constant is now 10.000*'.

With in-vitro nitrogenase composed of 2Fe-proteins, each a homodimere, and the MoFe protein that is tetramere (e.g. Lawson & Smith 2002), 'molecular crowding' will change things completely compared with aqeous solution. Yet, the widely quoted mechanism for in-vitro nitrogenase functioning was developed from solution kinetic studies, '*stopped-flow spectro-photometry and rapid-quenching followed by product analysis*' (Lowe, Thornely & Smith 1985 p.232/3, Lawson & Smith 2002 p.90; Thorneley & Lowe 1983&1984, Thorneley & Deistung 1988). Re-location of this in-vitro nitrogenase in the 'crowded cell' is only now beginning.

But not yet in the standard-setting thesis of Wilson (2005), in spite of the fact that it gives an admirable overview of the present state of the kinetics-based mechanistic discussion.

Besides the associations also the wider consequences of the 'packing' of enzymes are being investigated and prove quite immense (e.g. Williams et al. 2004). Add to that the phenomenon of 'substrate channeling' of intermediates by (packed) enzyme systems (e.g. Anderson 1999), with the enzyme system forming a bit of a 'factory', the substrate being channeled along its components without intermittently being dislodged (that it is at least possible in the nitrogenase system is intimated in e.g. Wilson 2005). Here anything disrupting place and contact of the component enzymes will easily hinder the substrate channeling, and with that the enzyme 'efficiency'.

Taken together we have ample reason to suspect great differences between a 'test tube system' of protein components of an enzyme system and the in-vivo system within the cell. Certainly as to the nitrogenase system, where long before the protein structures became available (a) from experiments in dilute solution a mechanism was devised implying consecutive turns of dissociation and association of component enzymes, and (b) the process of quantum-mechanical 'tunneling' of electrons and protons was not considered (N.B. also the tunneling concept is hardly new). The way the protein components of the system are coupled decieds about the 'tunneling' of electrons or hydrogen atoms from one side of a 'whole' to the other instead of being 'delivered'. The picture of one component shipping the 'errand' to another component, that in a way is central to the 'conventional' nitrogenase model, now is question-able (Lanzilotta et al. 1996 and Johnson et al. 2000 point to cooperativity; cp. also Wilson 2005).

Enzymes in close packing: For preparations of intact nitrogenase see Eady & Smith 1979 §2.5. Consciousness that the in-vivo system is membranous/membrane-connected – cp. Oppenheim et al. 1970, Bennett 1973 p.74 – got lost in the effort to master preparation of 'purified' and reconstituted systems. Edgren & Nordlund (2004, 2005a&b) are some authors who presently re-invoke a membrane-associated system.
As to electron transfer in natural proteins (tunneling) cp. Page et al. 1999, Moser et al. 2000, Regan & Onuchic 1999. P450 electron transfer reactions: Udit et al. 2007. A tutorial review is Benniston & Harriman 2006. Recently Dance (2005, 2006, 2007, 2008a&b) started carefully exploring such possibilities with nitrogenase. For broader relations between catalytic activity and enzyme dynamics see Antoniou et al. 2006. For a review putting our subject in its plant cell context see Aon et al. 1999.
There are still other developments in enzyme theory, like the rejection of transition state theory for enzyme catalysis (with introduction of alternative concepts) in Sumi 1999, that make the re-evaluation also of much nitrogenase research inevitable. For non-partial accounts of the research that historically provided building blocks for nitrogenase mechanistic models see Eady & Smith 1979 and Smith et al. 1977.

The question of a coupling of the nitrogenase system with a hydrogenase is not resolved. Hydrogenases are commonly membrane-bound, so 'packing' could be of great consequence for coupling (and for tunneling or more general aspects still – for which cp. Curatti et al. 2005):

> '*One intriguing model postulates that the extent of 'macromolecular crowding' … will determine the tendency of intracellular macromolecules to associate with the plasma membrane and consequently their enzyme activity* (Häussinger 1996 p.204).

Intact nitrogenase as distinct from the 'reconstituted' versions is particulate, its relative stability toward O_2 '*attributed to association with membrane fragments and other proteins*' (Eady & Smith 1979 p.460). In 'reconstituted' nitrogenase to the contrary membranes etc. got 'purified away' and any interactions with them are lost.

Some information on the interrelation of nitrogenase and hydrogenase: Bothe et al. 1980 and Evans et al. 1985. Naik & Nicholas 1969 (cp. Burris 1971 p.132) reported on the transhydrogenase in the particulate nitrogenase system from *A.Vinelandii*; their report got into oblivion. The Friedrich et al. 2001 overview of hydrogenases shows that those connected with BNF are membrane-bound. Nitrogenase and hydrogenase are developmentally connected, the connection a candidate for disruption in breeding or culture (lactate repression: Gogotov et al. 1985). For multiple pathways in hydrogenase functioning see Homann's 2003 historical review.

Altogether, Peters & Mehn do not exaggerate when they write (from thorough chemical considerations, Peters & Mehn 2006 p.85; cp. also Hoffman et al. 2009 '*the summit remains distant*'):

> '*To a large extent, educated suppositions have been the dominant vehicle for conversations about key mechanistic aspects of biological nitrogen fixation*'.

And yet in a multitude of publications one specific reaction equation is presented as a short-hand for some supposedly 'generally accepted mechanism of BNF' *without* any warning about its character as an 'educated supposition'. From this 'equation' authors then 'admit' that quite an excessive energy investment by the plant is needed for BNF. From there it is only a small step to the 'conclusion' that industrial fertilizer provision is a solid advance compared with BNF….

5.8 Shorthand for a questionable model, not a reaction equation

As to BNF and its mechanism(s) McKenna 1980 wrote (p.449):

> '*The molecular mechanism of reductant-dependent ATP utilization by nitrogenase remains completely obscure at this time. Accordingly, discussions will be limited here to a brief outline of background information and some current speculations on the chemical role of ATP in nitrogenase reductions*'.

Indeed McKenna after the brief outline gives far more attention to the '*Intrinsic chemistry*' (l.c. Pt.III) and abstains from any and all suggestions as to a stoichiometry of the nitrogenase reaction(s). Now if we go to the present this sober attitude is becoming the norm again, with e.g. Rees et al. (2005 p.973/4) writing:

> '*Despite decades of study, the mechanism of biological nitrogen fixation remains enigmatic and continues to provide the chemist with scientific challenges*'. '*The overall reaction stoichiometry of the nitrogenase catalysed reaction has still not been unambiguously determined*'.

As a consequence, where formerly many an author who was less chemically-sober than McKenna used a shorthand for the 'dominant' mechanism in the form of a reaction equation:

$$N_2 + 8H^+ + 8Fd^{red} + 16ATP \rightarrow 2NH_3 + H_2 + 8Fd^{ox} + 16ADP + 16P_i \quad (I)$$

we now see the equation in retreat, with e.g. 'nATP' instead of '16ATP' to indicate the uncertainty about the energy requirement (Howard & Rees 2006; note that 'ATP' is a reactive phosphate ester whose formation requires energy and whose well-connected hydrolysis then provides energy on a spot where it is needed).

Now the nitrogenase mechanism works with the assumption that MgATP binding and hydrolyses, through e.g. some peptide conformat-ional change it induces, is connected to electron transfer from the Fe protein to the MoFe protein. Yet Igarashi & Seefeldt illustrate how variable the real experimental situation is (2003 p.361):

> '*The details of how MgATP binding or hydrolysis promotes the transfer of an electron remains one of the significant open questions in the nitrogenase mechanism. While these two events, MgATP hydrolysis and electron transfer, are linked, they are not obligatory. Both MgATP hydrolysis in the absence of electron transfer and electron transfer in the absence of MgATP hydrolysis have been demonstrated. Hydrolysis of MgATP can occur at significant rates … when the Fe protein is mixed with the MoFe protein without any source of electrons to support electron transfer (so called reductant independent ATP hydrolysis). … The independence of the electron transfer event from MgATP hydrolysis has also been shown under special circumstances*'.

That leads to the question if the 'reaction equation' ever was admissible as shorthand? To find an answer we need a look at the historical evidence.

Consulting e.g. the 1979 Eady & Smith overview we soon discover that the ATP-situation was already then known to be **more variable and uncertain still** (cp. l.c. p.434, 446, 447, 454, 457, 481) than the quote above intimates. Burris 1971 (p.134f.) documented that it from the start were specifically the in-vitro systems that had high ATP use and admitted that '*the number of ATPs required per pair of electrons transferred may be less for intact organisms* *than for extracts*'. The way the in-vitro of nitrogenase showed up as an 'ATP-ase' was (also) connected with the use of the non-physiological reductant dithionite. Yet Burris c.s. never evaluated if maybe this dithionite use was leading to artifacts.

Next in works like Burns & Hardy 1975 (Ch.4) instead of an exhaustive overview we meet a 'linearized history' that plays down (or neglects) discordant results. Their enthusiasm for model building is unmistakeable, as is the inferential character of the model proposed. Smith et al. 1977 are more careful to disclose experimental details: their model derives from extensive use of dithionite in stopped-flow spectrophotometric kinetic experiments (see also Watt 1977, Lawson & Smith 2002 §5). But then, already in preparations and purifications for in-vitro systems the role of dithionite is quite dominant (cp. contributions to San Pietro (ed) 1972, Packer & Glazer (eds) 1988). As a result the model is peculiarly dependent on a select group of dithionite-dominated preparations and experiments. Smith et al. 1977 (p.195) mention facts like dithionite hindering association of the nitrogenase components (so leading to continuous energy loss and investment to yet attain to a complex?) then next progress to their model based on dithionite use as if all was well.

> The questionable dominance of the non-physiological reductant **dithionite** in nitrogenase assays was manifest early on (Evans & Phillips 1975 p.394). Note that Burris 1975 already indicates the insufficiency of dithionate (p.345/6), yet, once more neglects to follow his own indication (as with silage NO_2 in the 50s and NO in the 60s). Likewise, Hageman & Burris 1978 point to the inferiority of dithionite cp. with flavodoxin. Dithionite proved insufficient as a reductant also with other enzymes, e.g. Schindelin et al. 2001 p.73. Note that dithionite was introduced in BNF research while neglecting (a) its effectively radical character that causes unwelcome interference with proteins and their function (b) its decidedly weak character as a reductant at physiological pH. This last a fact was known from Jellinek's 1911 publications and thoroughly covered in the 1960 edition of the most fundamental handbook of inorganic chemistry, *Gmelin's Handbuch der anorganischen Chemie*.

With both the number of discordant observations of ATP quantities and roles far bigger than generally acknowledged, and the complete dependance of the model on preparations and experiments with the non-physiological reductant dithionite (providing the SO_2^{-}• radical that is the effective reductant), the answer to our question evidently is:

> **the number of discordant observations always was so great and the dependence of the model on dithionite that big that something like (I), a 'reaction equation', chemically never was indicated.**

In fact it is widely known that the problems in the direct study of nitrogenase functioning until now proved unsurmountable (e.g. Holland 2003 p.586) and so any proposal ought to be limited to e.g. a scheme embodying a 'rationalization' of a number of experiments plus an account of all of the deviating facts. The still too common presentation of (I) as a fact (e.g. Zuberer 1998) has nothing to commend. The same is true of all those publications that pretend to summarize all of the available evidence with words that in effect suggest the same rigid reactant relations as (I) does (e.g. Gutschick 1980).

Yet, a conviction took hold that there is '*the fortunate occurrence of a virtually-universal best stoichiometry of (ultimate) reactants at the nitrogenase enzyme*' (Gutschick 1982 p.127) best expressed by some such 'reaction equation'. Evidently most researchers and their public were confident that results were on the way to unification already and that discordancy was soon to disappear. After all, the 'progress of science' was undubitable everywhere and one could be sure that present problems would soon yield to more of research.

As to the present, doubts about e.g. dithionite in nitrogenase research are definitely being expressed (e.g. Lawson & Smith 2002, Howard & Rees 2006). A recent observation is that flavodoxin hydroquinone, a natural electron donor, proves able to fully reduce the Fe protein to the all-ferrous $[Fe_4S_4]^0$ state, a fate that dithionite is not able to accomplish. (Note that flavodoxin hydroquinone hardly was a new reductant - Evans & Phillips 1975 p.393). Furthermore flavodoxin hydroquinone can pass electrons to the $[Fe_4S_4]^{2+}$ of the oxidized Fe protein while it is complexed to the MoFe protein, *without a dissociation step*. Other experiments since then have tended to affirm the conclusion that the dithionite-dominance could very well have led us astray - with the widely quoted 'mechanism' dependent on artifacts.

> **A note on the nitrogenase Fe-protein redox chemistry:**
> Experiments with Ti(III) revived the doubts about dithionite as a nitrogenase reductant (e.g. Nyborg et al. 2000). For the Fe protein with an all-ferrous cluster see Watt & Reddy 1994, Erickson et al. 1999, Strop et al. 2001 (the second publication the link to reduced ATP use with this reduced cluster). Lowery et al. 2006 bring the definitive proof that flavodoxin hydroquinone reduces the Fe protein to an all-ferrous physiological specimen.
> A thorough treatment of background chemistry (e.g. as to Fe complexes) is in Peters & Mehn 2006; cp. also Lawson & Smith 2002 (as indicated). Scott et al. 2005 and Deng & Holm 2008 develop an all-ferrous analogon to the nitrogenase Fe protein, then Chakrabarti et al 2009 prove the close similarity. Hendrich et al. 2006 take a close experimental look at a possible Fe^I/Fe^{IV} cycle for nitrogenase.
> A mechanism strictly comparable to the Thorneley-Lowe nitrogenase proposal has been invoked for a hydratase (Hans et al. 2002), but now also in this system an all-ferrous cluster proves extant (Hans, Buckel & Bill 2008) and the mechanism is likely to change.
> Note that **enzymatic H-transfers** until now are a neglected subject in nitrogenase research (for an inroad refer once more to Schindelin et al. 2001). Likewise the relation of nitrogen fixation with leghaemoglobin and oxygen transport in the fixers was up till now neglected. Minchin et al. 1986, Minchin et al. 1989, Becana et al. 1989, Kanayama & Yamamoto 1990a/b & 1991, Gordon et al. 2002 can prove useful for a start.

We come to a conclusion as to the 'cost' of BNF. With equation (I) proven 'chemically unlawful' and the nitrogenase mechanism concept in flux again, we can limit ourselves to research that consisted of truly experimental investigation in BNF's energy relations. Here we note that

(A) such experimental research already in '73 brought as a result that the energy cost of in-planta nitrate reduction is comparable to (what then was counted as) BNF's energy requirement (Minchin & Pate 1973). That is, the 'costliness' of BNF to the plant as compared to mineral fertilizer nutrition is evidently not a real issue.

(B) serious research in BNF-costing from before the dithionite-based systems development consistently decided that costs were low (e.g. Christiansen-Weniger 1923 as summarized in Stapp 1927; Wilson 1958). The 'high costs' story evidently derives from … doing as if something like (I) is a solid experimental fact (e.g. Pate et al. 1981, Gutschick 1982, Heytler et al. 1985, even Ludden 1991 in spite of his doubts about dithionite on p.374).

> **Perspectives in BNF costing research**: The title of Heytler et al.'s 1985 publication is '*In vivo* energetics…', yet, they introduce their subject with '*The currently accepted stoichiometry for N2 fixation is*' and then present (I) above, and this then makes them choose from among the widely divergent experimental researches. The more recent experiment-based research - e.g. Warembourg & Roumet 1989, Schulze et al. 1999, Voisin et al. 2003 – is not that prejudiced, still it can be wise to compare it with older studies like Wilson 1958, e.g. because the more modern ones are hampered by fertilizer-based pre-treatments and the like.

Note that most studies use e.g. glucose or mannose feeding, yet at least since 1925 it is known that this influences BNF in a negative way (Lipman & Teakle 1925). Recent research makes clear that principal carbon sources for rhizobia are C_4-dicarboxylic acids: Poole & Allaway 2000, Jeong et al. 2004. As to free-living nitrogen fixers, a.o. Winogradsky emphasized before the war the greatly distorting effect of using sugar feeding. One more point that could prove important: present day studies depend on a way of distinguishing growth from maintenance (both as part of respiration) that got questioned recently.

Already now it is evident that the carbon cost of BNF – that is the amount of photosynthate the plant must accord to BNF - '*mainly depends on the amount and possible recycling of concomitantly produced hydrogen, and on the efficiency of energy transformation into ATP and reductants*' (Schulze et al. 1999 p.625). With the non-use of hydrogen in intact systems increasingly seen as an aberration, and in-vivo ATP & reductant formation more efficient there than that in reconstituted systems, it is quite possible that the true carbon cost of BNF will not just prove comparable to e.g. the cost of in-planta nitrate reduction but indeed will prove far lower (as the pre-dithionite investigations do suggest). Yet even when we just stick to present day results we are sure that it is not warranted to speak of the 'high costs of BNF' (as in Smil 2000).

5.9 BNF rediscovered

Recent reports leave no doubt about nitrogen fixing endophytes in wild rice species. It was not difficult to infer to the existence of such endophytes: e.g. African wild rice has been harvested and consumed for more than 10.000 years and is frequently found associated with aquatic legumes so that a close investigation was indicated. But it is important to work with wild rices or land races, because modern races often are no good hosts for these endophytes, and the fertilizer gifts that are used with modern varieties disable the associations/symbioses looked for.

The rice picture is repeated with maize: nitrogen fixing *Burkholderia* sp. were found to densily colonize indigenous maize in Mexico, but 'elite' strains did poor when inoculated with bacterial endophytes previously isolated from maize. Note that research in nitrogen fixation in maize varieties (and other Graminae) is hardly new: the name of Johanna Döbereiner especially was connected with it from the 50s on. Tropical grasses and their associations with nitrogen fixing microorganisms were among her favorite research subjects, with nitrogen fixation in sugar cane a special focus.

BNF: wild rice & land races vs. HYVs. For older research cp. Balandreau et al. 1975, Diem et al. 1978. Yanni et al. 2001 relate the example of berseem-rice rotation disabled by fertilizer. Engelhardt et al. 2000 prove the inferiority of modern races in the associations/ symbioses. For the African wild rice investigations consult Chaintreuil et al. 2000, for other wild rice-endophyte research Elbeltagy et al. 2001. Baldani et al. 2000 starting from an old variety known for its good yields in poor soil with low fertilizer input demonstrated fixation; van Nieuwenhove et al. 2001 with their modern variety had more problems.
Reinhold-Hurek et al. 2006 proved colonization of rice roots by nitrogen fixing *Azoarcus* an active process and Alexandre & Bally 1999 showed that *Azospirillum lipoferum* via a two-step phenotypic switching process becomes rice rhizo-sphere competent.
IRRI research with HYVs shows that modern varieties can be quite 'resistant' to the endophytes, e.g. James et al. 2002. Tan et al. 2003 show a direct link with N-fertilizer. So when IRRI-associated researchers Dey & Datta (2002) with their overview of – mostly HYV-related – nitrogen fixation in rice focus at genetic manipulation this likely is another 'technological fix'.

For real progress we better start with (a) an evaluation of the disabling influences of half a century of high-fertilizer breeding, and (b) extensive 'agro-ethnography' that allows research to start with wild varieties and land races, and from traditional agriculture at large (as in Saleena et al. 2002).

For sure in this age of 'biofuel' the possibility to obtain alcohol from sugar cane without the investment of energy-demanding fertilizer is unique among all of the suggestions for biofuel crops! (Boddey 1995). Sugarcane related BNF-research is known from e.g. Egypt too, but in Europe and the USA neglect was main-line agricultural research' chief response. In spite of that the subject now is a focus of attention.

For a review of **Döbereiner's Brazilian program** see Baldani et al. 2002, for its follow-up elsewhere see Ruschel etv al. 1978, Lee et al. 2000. Some reviews: Boddey 1995, James & Olivares 1997. For neglected Egyptian research see Hegazi et al. 1979 in the rice symbiosis (cp. now Yanni et al. 1997, 2001, etc.). For endophytes of sugar cane see Cavalcante & Döbereiner 1988 (discovery), James et al. 1994, James et al. 2001, Lee et al. 2006. For a rotation that is related see Chen & Lee 2001. For inhibition of colonization by high N-fertilizer application see Fuentes-Ramírez et al. 1999, dos Reis Jr. 2000.
Maize references: Palus et al. 1996, Estrada et al. 2002, Riggs et al. 2001. Wheat: Larson & Neal 1978, Elliott et al.1979, VandeBroek et al.1993, Iniguez et al.2004. Grasses: Döbereiner & Day 1975, further e.g. Bergholz et al. 2001, Hurek et al. 2002, Miyamoto et al. 2004. General reviews of BNF in Graminae: Döbereiner & Boddey 1981, Reis et al. 2000.

After the recent adaption of molecular genetic methods to soil, sediment and aquatic micro-biology, exploration started in what was intimated to be largely unknown territory. And indeed, from the first decisive publication of Torsvik et al. (1990) there has been no end to surprises. To give an example: the assemblage of nitrogen fixing microorganisms associated with the roots of the salt marsh grass *Spartina alterniflora* proved not only very diverse, but also mainly to consist of unknown organisms (Lovell et al. 2000; in fact similar explorative research never led to another conclusions than that the microorganisms in soil or sediment samples are largely unknown).
As if the new tools gave renewed courage, we now are in the midst of renewed attention for soil and sediment microbiology, including further development also of methods other than molecular biological ones. One of the results is a careful exploration of possibilities to construct low-nutrient environments far closer to soil and sediment than in former experiments. These already provided a far more real entrance to soil and sediment microbial life than had been attained before (when most research was hampered by the use of methods adapted from industrial-laboratory-type of research).

For valuable **low-nutrient research** of soil and sediment microorganisms see Mazumder et al. 2000, Aagot et al. 2001, Janssen et al. 2002, Kaeberlein et al. 2002. For screening tools for free-living nitrogen fixers see e.g. Bürgmann et al. 2004. An example of perspectives opened on uncultured microorganisms (the great majority by far) thanks to the adoption of molecular biological methods: Harris et al. 2004.
Remember that it was Winogradsky (e.g. 1926, 1935, 1949) who already introduced soil-adapted methods that yet few microbiologists or soil scientists were ready or able to follow. Construction of low-nutrient environments close to soil and sediment is also likely to help the study of those centrally important microorganisms that break down complex biopolymers or humics. At the moment these microorganisms are the great unknown in soil and sediment.

For example, an old puzzle of BNF in grasses was solved: grass endophytes (e.g. *Azoarcus* sp.) proved to contribute their fixed nitrogen to the plant in an *unculturable* state

(Hurek et al. 2002). The lack of culturability of endophytic nitrogen fixers showed up with other grasses too (e.g. Miyamoto et al. 2004), so not the BNF of those microorganisms had been the problem, but our own methods and inferences.

BNF in forestry, agriculture and ecosystems see e.g. Rózycki et al. 1999. Cp. Sessitch et al. 2002 for a review of Rhizobium research. Also **non-legume BNF symbioses** are known a long time, cp. Becking 1970; for symbioses with *Frankia* see Guan et al. 1998. As with mycorrhizae, BNF symbioses/associations are present in all ecosystems. As a rule BNF symbioses are **coupled with mycorrhizal symbioses** (for revegetation efforts cp. Valdenegro et al. 2001). For evidence of broader (than ammonium) **N-compound export** from legume nodules see Day et al. 2001; also Waters et al, 1998, Rosendahl et al. 2001. For BNF-related plasmids and their mobilization see Brom et al. 2002. **Agroforestry** BNF examples: Kass et al. 1997, Bergström & Kirchmann (eds) 1997 (div. contributions), Berninger et al. 2000, Clapp 2001, Bala et al. 2003, Schroth & Sinclair (eds) 2003 (div.contrib.).
It is clear that there is an invaluable natural capital here that is not amenable to e.g. 'genetic manipulation' - note the great number of different BNF-related genes, Colebatch et al. 2002, Tesfaye et al. 2006 - but that awaits careful local development by the small farmer. Some discoveries: Bryan et al. 1996 (non-nodulating yet fixing *Leguminosae*), Rivas et al. 2003 (non-rhizobial legume symbionts), van Berkum & Eardly 2002 (unexpected symbiont). For high-altitude legume symbioses see Jacot et al. 2000. Symbioses with cacti in disturbed urban desert soil: Bashan et al. 1999. Symbiosis in soil crusts in e.g. semi-arid rangelands: Davidson et al. 2002, Belnap 2002, Yeager et al. 2004. With shrubs in arid soils: Quatrini et al. 2002.

As to legume symbionts, redifferentiation of *Rhizobium* sp. at nodule senescence into the state of free-living, growing bacteria proved true not for a minority, but for the great majority of the nodule microorganisms (Müller et al. 2001). The rhizobial life cycle evidently is adapted to the plant-soil transition v.v. Which proves that it is perfectly possible to go for its local agricultural use without some form of engineering by experts from central institutions.

It is encouraging that surprises with legumes, formerly considered a rather well known class of host plants for nitrogen-fixing *Rhizobiae* only, are not coming to an end. Likewise, there is a constant stream of surprises also with the other big class of BNF plants, those plants (e.g. *Alnus* spp.) living in symbiosis with *Frankia* spp. Both the plants that are host to *Rhizobia* and those that are host to *Alnus* are of decisive ecological and agricultural importance. The symbioses of shrubs and trees with nitrogen fixing micro-organisms, for example, are of prime importance in agroforestry, as well as in establishing vegetation on derelict land or in harsh environments. But note that nitrogen fixation proved highly active also in the root zone of common trees like pine and oak.

It is not exaggerated to speak of a **re-discovery of BNF in agriculture and eco-systems**, with a renewed appreciation of the N-delivery by legumes to the soil environment (due to exudates, root turnover, residues) and to neighboring plants (also by way of mycorrhizae) an important part of that re-discovery. Essential is here the 'recovery of wonder', as exemplified in Vance's (1998) contribution on legume BNF in agriculture. A quote (from p.511):

> '*The amount of N_2 fixed by legumes is quite amazing since the total amount of nitrogenase in the world amounts to only a few kilograms. … To replace the N_2 fixed by legumes [in agriculture] with anhydrous ammonia produced by the Haber-Bosch process would require 288 Tg of fuel and cost approximately $30 billion annually*'.
> (The estimates of BNF, in and outside agriculture, are higher now than they were in the 90s).

A re-discovery of traditional co-cultures & rotations of crop plants accompanies this re-discovery of BNF. Remember that intercropping of e.g. maize with pulses was the norm in vast stretches of Latin America and Africa, and that in Africa it was colonial administrations that only too often encouraged monoculture (obviously for export & tax reasons). It was only with the 'hybrid maize rush' of the 1950s, that the use of inorganic fertilizer became a main focus of agricultural research, also in Latin America and Afica (refer to Raussen 1998 for an account of these and other points). Before its advent, rotation and green manuring were in focus, with encouraging results obtained in the decades before 1950 (Raussen refers to a 1952 review). In the 1980s, with the IMF 'Structural Adjustment Policies' governments refrained from subvening fertilizer use, so the farmer in Africa and Latin America returned to his local, natural resources, BNF a main component (legumes figure prominently, as do improved fallows with leguminous shrubs and trees). As to BNF by non-symbiotic microorganisms, here mycorrhizae could prove the connection between non-symbiotic BNF and the plant.

In regard to the quantitative importance of BNF in ecosystems and on a global scale, we recently saw a steady rise in the estimates. A most notable feature of this steady rise is an ongoing series of discoveries in the marine environment of 'new' nitrogen-fixing organisms and their unanticipated quantitative importance. BNF proved both more general and of far greater quantitative importance than had been envisaged within the mineral fertilizer paradigm of the post-war half century. In effect we now know that the huge 'needs' for industrial fertilizer that have been projected time and again largely derived from an essential undervaluation of BNF. The consequences of the re-discovery for agriculture and agricultural policy are immense, so we will return to this central aspect of our research in the final chapter.

More on BNF rediscovery: Kallar grass - a favorite research subject of Johanna Döbereiner – was shown to actively stimulate free N2-fixers' N-compound delivery by organics exudation, Mahmood et al. 2002; nitrate abolishes the process. Plant uptake of the N-compounds plus low oxygen levels in the soil guarantee continued nitrogenase gene expression in the non-symbiotic fixer *Azospirillum brasilense,* Fadel-Picheth et al. 1999 (cp. earlier studies like Anderson 1958). For a broader Azospirillum review see Steenhoudt & Vanderleyden 2000.
As to the broader study of bacterial endophytes, after decades of comparative neglect this has now become obligate, see Rosenbluth & Martínez-Romero for a review. Refer to Kan et al. 2002 for total below-ground nitrogen contribution of legumes, for exudates by leguminous trees Uselman et al. 1999, for mycorrhizal recovery of N-compounds from legume crop residues for succeeding maize see Paré et al. 2000, for mycorrhizal translocation of soil N-compounds see Frey et al. 2000.

5.10. So what's it all about?

We intimated in Ch.4 that mineral-N fertilizer is not, in spite of common opinion, the solution to sustainable agriculture and food provision, but in its dominant position part of the problem of our post-war 'industrial' agriculture. In the present chapter we saw that indeed the central role accorded by post-war policy to this fertilizer had disruption of organisms, crops and soils as a result. Yet, within the mineral-fertilizer paradigm, as institutionalized by the government in full accord with most experts, there was no possibility to discern the disruptions. One could not even imagine they were there…
The result was a great delay in the reception of the many signals. In fact, those signals were received by 'outsiders', researchers from other disciplines not dominated by the fertilizer

paradigm (e.g. medical researchers and ecologically conscious researchers like Barry Commoner), and people conversant with traditional farming systems (e.g. agroforestry systems) who saw things deteriorate. But as to the sciences, these proved human indeed, and it was a painful process to become conscious of the fact that as to e.g. BNF assays we had been focusing on artifacts.

Recently we indeed saw a re-discovery of BNF – but note that in their agricultural and research policies governments still are stuck within the post-war paradigm. If the TNCs that at present dominate breeding and seed trade are similarly stuck I don't know. But with the value of their products being dependent on this post-war paradigm that allowed them to grow to their present position, they evidently are not in a hurry to introduce a change toward an agriculture that once again centers on farmer and local ecology. Still the notion that their products (their few HYVs plus all the package that is 'needed' with them) indeed are inferior in essential aspects is quite enough to displace them from the central place and reinstate farmer and local ecology in their stead. In fact a select company of public breeders in Europe is presently taking the lead in this reinstatement (Newton et al. 2009).

Still, the present situation is bad enough. Half a century of 'industrial agriculture' policies now made food thrice too expensive for the poor (that is the great majority) in Asia and Africa (Doll et al. 2008). Where they formerly spent already ¾ of their income they now cannot even procure half of the food they need. And as a result of their governments' 'modernization policies' plus the forced changeover to export crops the formerly dominant subsistence production of their agricultures has largely disappeared. The low food prices that, thanks to subsidies etc, made the accelerating urbanization a 'possibility' are no more. We apparently experience the end of the post-war deruralization-urbanization epoch as it depended on the ongoing drive for an 'industrialization' of agriculture that was implemented with massive government subsidies.

If this all is not bad enough, note that other dangers deriving from this 'industrialization' drive hardly got mentioned up till here. A major one is the endangerment of our surface waters – including our drinking water reservoirs – from cyanobacterial toxins (hepato- and neuro-toxins, suspected carcinogens also). They are present in roughly half of the now common 'algal blooms' that dominate our surface waters chiefly from agricultural nutrient leaching. Once leaked from the microorganisms and free in the water these toxins can hardly be separated from it. So this is a true 'ecological time bomb' needing fast policy changes – but as yet none are visible here.

I haste to stress that there is the other side of the present subject: the near-complete disuse of natural resources like BNF and mycorrhizae in our 'industrial' agiculture. We have good reasons to speak of 'the end of industrial agriculture', but that could very well mean 'the reinstatement of farmer-and-ecology centered agriculture'. We are not just 'stuck': we are stuck with our industrial agriculture, but outside of it there are wide perspectives. Research into BNF and other symbioses & associations can give its modest assistance to the changeover to sustainable modes of farming in which the careful use of local resources is at the center. With that change also a wide array of problems stemming from our 'industrial agriculture' policies will disappear. But I propose to postpone any effort to draw some final conclusions and first take a closer look at our recent history itself.

As indicated before High Modernity was central to that history. Now note that its post-war, all-out, government directed modernization/centralization effort hardly has parallels in history, not the least because the technical means were not there. If only we are able to step out of the modernization paradigm, the close analysis of this post-war ‘project’ is comparatively easy, just because of its historically unique character. I will take the effort in the following chapters - but of necessity in a piecemeal way (as our efforts at historical description ever are). Still I hope that the bits of history that will result will enlighten our effort at analysis and evaluation.

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6.

Feeding the world – from the laboratory?

6.1. Impairing plant health

Powdery mildew is one of the crop diseases where a close connection between nitrogen fertilizer application to the crop and severity of the disease is apparent. Griffiths' summary of Bainbridge's (1974) painstaking research on powdery mildew of barley is especially illuminating here:

> *"In a detailed study of powdery mildew infection on plants receiving different amounts of nitrogen he found that spore germination and appressorial formation were unaffected but that, with increase in nitrogen, the number of infections (as percentage of appressoria) was much greater and further, that colony size and spore production per colony were also much increased. The epidemiological consequences of this change in host response are clear: they are equivalent to changing from a highly resistant to a highly susceptible variety."* (Griffiths 1978 p.7/8)

Researchers like Bainbridge did perform their researches because the disease-proneness of crops that received high nitrogen fertilizer gifts was widely known. As Goodman et al. wrote (1967 p.203):

> "*In agricultural practice it is a very well known fact that the application of nitrogenous fertilizers in large amounts tends to increase susceptibility of plants to many infectious diseases.*"

E.g. specifically as to wilts Sadasivan (1961 p.454) wrote:

"The effect of nitrogen in promoting vascular wilt diseases seems to be almost general".

A decade earlier still George McNew – who exemplifies the close relations of post-war agricultural advice and big industry – in his contribution *'The effects of soil fertility'* to the 1953 USDA Yearbook of Agriculture *'Plant diseases'* wrote (p.100, 102):

> "*Some diseases are severe on weakened, undernourished plants. Many others are most destructive when plants are growing vigorously. .... Used to excess, [industrial nitrogen fertilizer] encourages rank, vegetative growth, delays maturity, and tends to cause thin cell walls. Fungi may penetrate the thin walls more readily than normal ones. Infected plants collaps more easily. Cereal plants lodge. Lesions on leaves elongate rapidly. ... The roots, water-conducting tissues, leaves, and fruits of plants that are supplied with nitrogen are more nutritious to most pathogens, which grow better in them than in nitrogen-deficient plants*".

One would think that after such a litany McNew would come with some strong research program, to analyse these shattering consequences of the high industrial fertilizer gifts, and search for alternatives. But instead we read (p.101):

> *"There is no reason for starving the plant into an unproductive state in order to escape disease. If the plant is properly nourished and capable of full development, the disease control measures, such as spraying or soil disinfestation, are fully justified. They are a form of crop insurance".*

If one considers that a big part of pre-war agronomic research, in the US and elsewhere, had focused on rotation with legumes etc. and had found them able to deliver both high yields and sturdy plants, one is wondering why McNew is pushing the farmer on the high-fertilizer island and then burning the bridges behind him.

But as indicated some, at least, independent research continued, with increased understanding of the problem as a result. Bainbridge's research especially offered a clear scientific expression: **a plant variety changes from a highly mildew-resistant one to a highly susceptible one (at the population level) as a result of the nitrogen fertilizer gift.**

This change in susceptibility was accompanied by a change in metabolism, something that had been indicated repeatedly by then. It even shines through quite clearly in the McNew quotation. In fact, the importance of *polyphenolics metabolism* had been apparent a long time, e.g. from the researches of Dufrenoy in the 20s and the 30s (cp. Harborne 1964 p.525 f.). Its relation with fertilizer gifts got always more clearly expressed, e.g. in Kiraly's 1964 publication *'Effect of nitrogen fertilization on phenol metabolism and stem rust susceptibility of wheat'*. A recent summary of the polyphenolics-related healthy-plants characteristics at low nitrogen supply is given by Sander & Heitefuss (1998 p.496):

> "*structural preformed barriers may prevent pathogen ingress into the host; preformed inhibitory compounds may restrict development [of disease organisms]; pathogen-induced changes may contribute to a more efficient active resistance response*".

As to the induced secondary metabolites Harborne (1998 p.362) summarizes:

> '*A bewildering array of different secondary metabolites are produced by plants following fungal or other elicitation as barriers to microbial infection. … In general, it appears as if the type of phytoalexin produced is likely to be related to existing preinfectional agents present in a given family. … In terms of antimicrobial activity, there does not appear to be any obvious structure activity relationship. An extremely wide range of phenolics, terpenoids and nitrogen-based molecules are capable of providing fungitoxicity or inhibition of fungal growth*'.

It is evident that the plant's concept of 'pesticides' is diversity-based, both structurally and biochemically. Evidently, industrial pesticides make a poor show in comparison with the plant's 'pesticides' - not unlike the poor show that industrial formula makes in comparison with mothers' milk. The subject is complex indeed – as plants always are – but not less important because of that. To summarize: (poly)phenolics are connected (a) with structures (tissue/morphology), (b) with chemical resistance to pests & plagues (in production & storage), and (c) with signalling & response (both of internal and of external character).

> We cannot here enter further into this subject of polyphenolics and rust/mildew resistance, but notice that in the 1990s Carver et al. did much follow-up research into the subject (refs. with Rojas-Molina et al. 2007). Note that the problems are general, not only with cereals, but also with e.g. legumes like faba beans (l.c.). It is perfectly possible to change to *'low nitrogen supply'* to prevent disease pressure, because this in effect means *'supply at low concentrations'*. A continuous supply at low concentration is all that the plant needs!

Bainbridge c.s. were well aware that

- both the positive correlation between fertilizer gift and e.g. mildew incidence
- and the comparative insignificance of mildew on crops grown in soils to which stable manures had been frequently applied, had been clearly indicated in the literature for more than half a century (Jenkyn & Bainbridge 1978 mention e.g. Spinks 1913). Jenkyn &

> Bainbridge (l.c.) surmise that the slow, but continuous, release of N-compounds from dung to plants may be a factor in the retardation of mildew development. As to the positive effects of different organic amendments and green manures, in the control of (a.o.) wilts and mildews, this was common knowledge in pre- and post-war decades (Selcuk & Grossmann 1967, refs.).

Some examples can help to realize the truly broad generality of the correlation between increasing industrial (N-)fertilizer use and disease- & pest-pressure:

1.

Almost casually McMurtrey 1953, in his contribution to the 1953 Yearbook of Agriculture (p.97), mentions that *'Fruit trees on nitrogen-deficient soils may produce highly coloured fruits that store well'*. He thus touches lightly on the problems of disease resistance (incl. such at storage) and N-fertilizer use that in pre-war decades had received much attention. As to the biochemical basis for resistance in stored fruits Harborne 1964 (p.521f.) lists public-ations from the late 1910's to the 1930's in his overview of pre-war literature! In parallel, there had grown a far greater understanding of 'nitrogen deficiency' than McMurtrey's remark implies. In post war decades some independent research continued also in this field, e.g. on scab resistance (Mayr et al. 1997 and refs.), as well as on the broader importance of rich poly-phenolics in apples (Treuter 2001 and refs.). Yet, up till now this research and its results did not get re-integrated into main-line agricultural research and extension.

2.

At the start of the post-war surge of N-fertilizer gifts, Bawden & Kassanis (1950) stressed that, as to virus diseases, the 'quick growth' induced by high gifts coincided with maximum virus concentration and maximum virus activity. Quite in contrast, with plants grown at low N concentrations – e.g. in the wild – virusses will be present and will multiply to a certain extent, yet most plants are symptomless (e.g. Navas et al. 1998).

In the 90s the understanding of the active role of plants in regulating virus infections – under healthy metabolic circumstances – received a new impulse thanks to researches into 'gene silencing' in plants. Yet, the results of this research into the relationships between virus and fertilizer have not been re-integrated into main-line agricultural research & extension.

3.

Insect vector problems in plant pathology increase greatly with (industrial) fertilization level. E.g., whitefly problems in the greenhouse are widely known. This problem is more complicated still than the ones mentioned thus far, yet it again is closely connected with changes in the polyphenolics metabolism. That metabolism, in turn, is connected with leaf toughness, with storage of phenolics, and with leaf nutrient quality. We will take a closer look at this problem shortly; for now it is important to stress that an increase of insect vector problems with industrial fertilizer gifts was indicated early on already.

The conclusion is rather straightforward: disease- and pest-problems connected with high industrial fertilizer gifts are formidable indeed. So why did research not focus on ways to reach a sufficient N-nutrition of plants other than those extremely high gifts of industrial N-fertilizer that soon came to dominate post-war agriculture in the western world?

A predominant reason is the conviction that it is only the select number of crop varieties that respond to such gifts with high yields that hold the future. E.g. Lemmens et al. (2004) in their research of Fusarium head blight of wheat demonstrate a strong increase of infection with an increase of the N-fertilizer gift from 0 to 80 kg/ha. Then, without further probing into the matter, from the fact that the dominant HYVs require far higher gifts still, they conclude that "*adaptation of N-fertilization represents no relevant tool in managing Fusarium head blight in practical wheat cultivation*". This is a startling conclusion, if we consider that

(1) in nature wealthy plant growth is occurring without high soil mineral N concentrations

(2) good yields were obtained in divergent farming systems with varieties adapted more to organic than to mineral N-nutrition (that now experience a come-back, R&J.Nellithanam & Samiti 1998).

Lemmens c.s. exemplify a peculiar choice not to look outside the very limited realm (of mineral N-nutrition and breeding) to which main-line agricultural research in the rich world has contracted.

But then, isn't this simply a technological fix? And if so, what is reasonable about prolonging it in this really primary activity, the growth of foods & feeds?
Why accept also an array of potentially devastating problems – e.g. eutrophication and its relation with toxic cyanobacteria – from N-fertilizer gifts that cannot fail to lead to extensive pollution, when clean agricultural production systems, without those huge gifts, evidently are known?
Why try in vain to pursue a high-fertilizer 'precision agriculture' when the inverse relation between the gift and the plant's uptake efficiency was widely known before the war already, and repeatedly demonstrated thereafter (e.g. Black, Nelson & Pritchett 1946)?

Questions like these will be scrutinized later. Yet so much is clear already at this point that there is no scientific logic in our high-fertilizer agriculture and agricultural research. Yet, it did become dominant, and we better turn to history to understand how this occurred.

6.2. Cooperative & organic hybrids

Hybrid corn growth is generally depicted as the leader in the widely heralded take-off in agricultural productivity. Important aspects of its introduction and growth have been covered already, e.g. by Kloppenburg in his thorough *'First the Seed'* (Kloppenburg 1988). As to the choice for hybrid versus open-pollinated corn, his sketch of its *political* character, as distinct from a presumed 'scientific' one, is definitive (p.103 f.). Others have stressed, as he did, the lost opportunities in breeding due to this singular focus on hybrids. And there are other, important, questions.

It is quite significant that nearly a century of research into heterosis – the question why the cross of two pure lines that of themselves produce poor yields, suddenly gives a very good yield in a few cases – has not furnished much of an answer. We are left with the fact that the majority of those crosses do not give such a yield.
In fact, corn genetics is an extremely rich endeavour, both empirically and theoretically. This fact was proved convincingly by Barabara McClintock's research (that decades later would earn her the Nobel price; for refs. see Keller 1993, Nash 1999). A truly research-based strategy would have drawn attention to the exploration of all these riches. By thus widening their practical and theoretical horizons, researchers would maybe have been able to link up with the experience of farmers, who had concentrated themselves for centuries on the breeding of landraces.
Yet, in post war decades researchers seem to develop a kind of "tunnel vision", which causes main-line research to become subservient to the industrial package (industrial fertilizer, irrigation, pesticides and industrial hybrid varieties).

Already before the war most research focussed on the continuation of hybrid breeding. Still, the extensive search for the right combinations (of pure lines), that is part and parcel of hybrid breeding, can easily have a public character. And, as a matter of fact, it had this character, with the regional farmer meeting the researcher within the framework of *cooperative breeding* that was an essential part of pre-war breeding in the US Agricultural Experiment Stations. Because Kloppenburg a.o. paid only scant attention to it, it is advisable to take a fresh look at its history.
It is significant that the report on Corn in the 1921 Yearbook of Agriculture (Leighty et al. 1921), and that in the 1936 Yearbook (Jenkins 1936), both indicate an 80% on-farm use of corn as a feed (l.c. p.165, p.457). In most of the Interbellum, US mixed farming was still the rule, on the numerous corn-growing farms. As such it was both an ecological and an economic necessity. As to the latter, this was due to the fact that higher profits could be made in poultry/eggs, beef/milk, and pork, as compared to grains.
With family farming dominating, the farmers concerned used over 50% of their land for small grains and (leguminous) hays, crops that kept them employed when they were not involved in cultivating the corn crop. The small grains and hays were important also as feed & food – many, if not most, family farms were still self-sufficient - and in maintaining the soil fertility (legumes). One should keep in mind that not even the pre-war, partial introduction of tractors had changed this picture.

A 1946 report mentions some results of the regional, agricultural development between 1934 and 1946. It is a report about model farms, focussing on the well-known New Deal project of the Tennessee Valley Authority (Staff ARD/TVA):

> *'According to a survey made in North Carolina, the average test-demonstrator reporting dairy cattle increased the size of his herd from four in the beginning to approximately six in 1945; and the average number of cows milked increased from three in the beginning to four in 1945...'*

That establishes that the *small* farm under the New Deal is still the *norm*. The report gives also a review of the efforts that have gone into: home improvement, legumes acreage increase, etc. As the *'resources which a farmer has to work with'*, it gives a list of ***Natural resources, Capital resources, Family resources, Community resources*** in which the human, the social and the ecological capital of the farmer predominate. The report uses concepts that soon after the war would be lost - to resurface recently - and it regards these as essential to sustainable agriculture.

So it stands to reason that, by the mid-thirties, many, if not most, Agricultural Experiment Stations still attuned their breeding programs to the needs of this family farmer, with his mixed farm on which he maintained soil fertility primarily by rotation with legumes. This was true for the corn breeding programs, no less than for breeding efforts with other crops (or cattle). As a rule those programs were of a decidedly public character (Kloppenburg 1988, 80f.). A quote (Tiles & Oberdiek 1995 p.164):

> '*Thus during the 1920s the farmers themselves were the linchpin of efforts to improve corn yields. Farmers practising these more systematic methods of selection, especially with an eye to disease resistance, saw their yields improve 5-10 bushels per acre above those of farmers who did not. Their knowledge of the corn plant and its range of variations, and of their own fields and ecological peculiarities, were powerful tools in shaping their own economic stability. … All shared a form of knowledge and a sense of perspective that many breeders/experts, especially a generation later, did not possess*'.

Though hybrid corn breeding next became the trajectory along which big breeders managed to cut off the farmer from breeding, in the 1930s the close relation between farmers and Agricultural Experiment Stations first was maintained also in co-operative hybrid corn breeding.
Admittedly, a steady supply of ever new varieties was needed, because the predominant method of resistance breeding brought only short-term resistance against diseases, that in due time was breaking down because of the arrival of a new and virulent race of the disease organism. As to this need for a continuous renewal of varieties the system was not sustain-able, but up to the war the distance of many Agricultural Experiment Stations to the family mfarmer was quite minimal.

Changes in hybrid corn breeding: The main difference between breeding hybrid corn as compared to open-pollination breeding was the ease with which breeders could limit certain selection procedures to Station-only activities. But note that as long as 'their' farmers used chiefly organic fertilizers, breeding of necessity required attention to G x E interactions, also because success depended (unwittingly) on e.g. rhizobial and mycorrhizal symbioses. Only when industrial fertilizer was promoted, to the exclusion even of organic fertilizers, and heavy use of pesticides was introduced, breeding of hybrid corn was uncoupled from the local soil and ecology. These changes accelerated greatly as a consequence of the war, and then thwarted 'organic breeding' (e.g. fertilizers disrupting the symbioses on which the organic varieties were thriving). Still, it was only when big seedsmen managed to cut the relations of public breeders and farmers, that co-operative breeding of varieties for organics-based agriculture was stopped.
But note that much of the historiography of hybrid breeding is still dominated by back-projection – as if high-yielding hybrid corn ever was hybrid corn responding to high gifts of mineral fertilizer. Fact is that high-yielding hybrids were bred for organic fertilizer too, with only the war and post-war years bringing the change to mineral fertilizer-dominated agri-culture, and public breeding increasingly forced to serve private breeders instead of farmers..
But it is likely that a decrease in farmers' expertise in breeding was apparent earlier. Especially the big farmers had got accustomed already before the war to a steady supply of new crop varieties from the Stations, especially in connection with the (loss of) disease resistance. The problematic sides of this development had been noticed by the well-known geographer Carl Sauer. He had stressed that, because of '*the extinction of species and varietal forms*' that accompanied this contraction of breeding to the Stations, it was part of building '*a house of cards*' (Sauer 1938 p.149f.).

Where close cooperation between Experiment Station and regional farmers was the rule, that is, in most States up till the war, the power of the big seedsmen was still limited. In those years, the fertilizer-responsive corn hybrids of those seedsmen anyway did not deliver the promised further growth in yields, because they relied too much on a narrow range of inbreds (Kloppenburg 1988 p.118).

And so we still see the 1938 Yearbook depicting a hybrid corn variety that evidently responded very well to a previous leguminous cover crop (Fig.3 p.437). It is even somewhat sturdier than the fertilizer-responsive hybrid shown later (Fig.3 p.494).

The USDA had by then been busy with fertilizer materials for a long time, but esp. after the Dust Bowl the great importance of the maintenance of soil structure & fertility using legumes, green manures, and farmyard manures, was emphasized. Experts in the USDA under Wallace even admired the age-old practices of European farmers in the production and use of such manures (Albrecht 1938).

So there was not some ‘steady evolution’ towards big farms, where crop varieties were used that had been bred to respond to large gifts of fertilizer. And yet, after the war there is this sudden introduction of these ‘highly responsive’ varieties, and soon that accelerating growth of farm size as well.

A note on agro-economic trends: From 1939/40 wholesale prices for farm products rose fast, with consumer prices rising far more gradually (Vatter 1985 p.91). For a short time agri-business took huge gains. Then, during the 1943-1945 years of strict price controls, wholesale prices were near to consumer prices, agribusiness was obliged to content itself with modest gains, and farmers’ retail was effective. As a result, and for the first time in decades, the farmer received the larger share of the consumer price.
But as soon as price controls ended, agri-business stepped in. Wholesale and consumer prices sky-rocketed, but not because of high prices to the small farmer. Then ‘industrial’ agriculture, cutting down on soil husbandry, and using cheap fertilizer instead of stable manures and rotations with legumes, outdid the small farmer. It outdid him *by externalizing the huge costs, costs to ecology and rural community first of all*.
The combined shift – small to large, organic to mineral - was announced as resulting from the application of ‘Science’ in farming. Then authors like Schultz and Mosher, from a fervent faith in this ‘Science’, spent their years in having it implemented everywhere. Yet, this massive endeavour was *built from ‘functional rationality’* (*sensu* Mannheim), and its ‘Science’ lacked substance…

As a matter of fact, already before the war many big landowners discovered that it was profitable to cultivate vast tracts of land using machines and low-wage, temporary workers. The banks especially had acquired a lot of land, due to the fact that they had foreclosed on a lot of mortgages of the small farmers. The State, of course, was stuck with the responsibility to pay for the social and ecological costs. The Dust Bowl proved that those costs were enormous.
Then, in the 1930s, Roosevelt and his Secretary of Agriculture, Henry Wallace, decided to take those big landowners to task. Unfortunately, their efforts have largely been forgotten, so it is time to refresh our memories.

6.3. Agricultural economic democracy

When Europe after WW II followed American ‘industrial’ agriculture as an example, it did so without evaluation of its inherent, social and ecological, destructiveness. Because many European countries after the war introduced the welfare state, the social destruction of rural regions was alleviated somewhat by e.g. housing policies for the urbanizing rural poor. Yet, the lack of evaluation shows up in the Common Agricultural Policy, that favours the big farmer and (especially) agribusiness, and leaves the small farmer with diminished resources. The CAP copied the input-output, factory model that is at the core of post-war Modern-ization, and stuck to it even when devising Environmental Regulations (Hogg 2002, esp. p.231 f.). As a result, farmers became hard-pressed between oligopolistic suppliers and purchasers, and most of them lost control over methods of farming and retailing (l.c. p.233).

As to the US, it was the Depression that saw, especially in the South, an accelerated growth of big-farm monocultures at the expense of the sharecroppers and small tenants. Cotton monoculture was widely known in the South, offering bare subsistence to the poor whites and blacks. Its roots were not only in the pre- Civil War plantation monocultures, but more still in the break-up of mixed, rotation-based, farming late in the 19th century. It was a break-up induced by ‘scientific advise’, as promoted by government, and it soon saw the gains of the first few years turn into permanent losses.

When during the first New Deal subsidies were made available to leave land fallow (to lower surplus production), many big landowners simply displaced most of their sharecroppers and tenants, intensified crop growing on the bigger part of their land, and collected the subsidies for the smaller part left fallow. The resulting misery for tenant and sharecropper was immense – witness Dorothea Lange’s photographs (e.g. Gordon 2006). All-in, the shift to big-farm monoculture was a consequence of (a) the appropriation of government subsidies by the wrong economic powers (b) the externalization of huge social costs. Both points were known in Europe, and yet, the CAP was constructed along the same lines.

But then, the CAP reflects the American example also in being unaware of the essential character of ruralities for agriculture: remember that the USA, as a ‘frontier society’, lacks the socially and ecologically integrated village that is central to sustainable, traditional agriculture in Europe. With the CAP tying payments to production, not to labours of care, the rich receive the most, and the ‘care-takers’ (e.g. small farmers) the least (cp. Scott 2002).
And so, a decade ago, the payments under the CAP for set-aside (fallow), were cashed in by e.g. big landowners in Portugal, who ploughed vast stretches of land for the first time, and then received payment for leaving it at that. The direct result was destruction of famous orchid grounds.

It is a historical fact that things could have turned out (much) better. For in 1933 the new Secretary of Agriculture Henry Wallace was immediately confronted with the displacement problem, when the political left (in the person of Norman Thomas) castigated him for allowing the sharecroppers’ displacement (Schapsmeier & Schapsmeier 1967 p.133). But the experienced Wallace evidently thought that only indirect action, the ‘building of muscle’ needed to take the big landowners to task, would have a chance of success. Wallace empowered the Soil (Conservation) Service, shaped the Bureau of Agricultural Economics (which he knew very well), and formed the Farm Security Administration, to look after the interests of agricultural labourer, sharecropper and small tenant (see e.g. Maris 1940).

In fact the FSA was a successor to the Resettlement Administration, established by Executive Order of Roosevelt himself (Tugwell 1959). It had been a truly remarkable agency, but Congress had declined recognition, so that the RA had to rely on funds from the President for emergency Depression Relief. Next its successor, the FSA, wás recognized by Congress, and certain of its functions authorized. Yet it had its wings clipped by the introduction of modifications and restrictions, year after year, when it asked for funds (Kirkendall 1984 p.76f.). This happened more and more frequently from about ’42 on (Markowitz 1973 p.26).
The FSA was attacked vigorously from the start. Just one example: Missouri governor Lloyd Stark wrote to Wallace in January 1939, that on investigation a (peaceful) roadside demonstration of displaced black sharecroppers showed ‘un-American and communist practices’, which could be traced directly to the FSA (Cantor 1968). Wallace was not slow to react and asked the FBI to investigate the case. When the FBI released its findings in March ’39, Stark and the big planters were ruled to be absolutely wrong. So by then the outcome of the struggle had not been decided, but the big landowners had not gained a winning position either.

The shrewd Wallace had been building a less vulnerable structure in the meantime, in a niche of the USDA he thought safe. In 1938, Wallace had re-organized the Bureau of Agricultural

Economics, after he had reached an agreement with the Land-Grant Colleges about the planning of Farm Programs, and had made Howard Tolley the BAE director. Wallace knew that Tolley was studying farmers' practices. For instance, a map displaying farming types had been prepared already under his direction. And, Tolley refused to push large-scale agriculture. Tolley and his BAE officials insisted that both experts and farmers should participate significantly in the planning process. For them county planning was a means of integrating the ideas of laymen with those of Federal and State technicians and administrators, the fusion of practical experience and 'expert knowledge'. As Tolley's friend Wilson defined it, the goal of the planning project was

> *"economic democracy in action ... farmers, experts and administrators cooperating in the different phases of policy formation"*.

Henry Wallace's choice was thoroughly studied. The BAE had been his father's creation in '22 (when he was Secretary of Agriculture) and as such for Wallace a 'safe' agency he could rely upon (Winters 1967). And: since the Bankhead-Jones act of '35, Wallace had a growing lump-sum at his disposal for wide-ranging, fundamental agricultural research (Pursell 1968). It was the long-time Congress-man Marvin Jones who had written Section 32 of the Act: the first guaranteed annual appropriation for agriculture in US history. An automatic appropriation renewable without Congressional consent. Wallace was quick to understand the possibilities it afforded him.

> **Martin Jones** was Chairman of the House Agriculture Committee from '32 till '40 (see May 1977). He came from a subsistence-type farm and adhered to the American ideology of the self-made man, yet without the social-darwinistic overtones voiced by most of its adherents. This distrust of social darwinism was one of the things he shared with full-blooded New Dealers like Tolley and Rugwell. More clearly still than they, he perceived the tendency of urbanites to misjudge the fundamental importance of soil & farming. The fact that he understood the importance of the land, made him a fervent supporter of soil conservation, a subject in which he was at one with important members of the New Deal USDA.

Taking no risk, Wallace in '39 secured approval from both the President and the Congress to give the BAE more of a say in terms of planning future developments. Now the road was free for a truly remarkable program. In Gilbert's words (2001 p.235) this program was about:

> "*a formal cooperative partnership between representatives of an entire sector (agriculture), government administrators, and applied scientists, seeking to shape and reform public policy. By 1941 it was underway in nearly twenty-two hundred counties, over two-thirds of all those in the United States. More than two hundred thousand farm men and women were involved in this network of planning committees, extending from neighbourhoods, communities, and counties to states and Washington, D.C.*".

The committees became increasingly citizen-oriented, in spite of the fact that the wealth farmers were over-represented and that the expert members dominated the discussions. (It was difficult to find small farmers who could take enough time off to serve on these committees). Things seemed to go better and better (Kirkendall 1984 p.77f.) – until the committees were abolished in '42. This was done under the pretence that the urgent and precarious war situation required it - but in plain fact by groups of people that regarded the committees and their work as an organizational and ideological threat.

But till then a group of social scientists in the BAE's Division of Farm Population and Rural Welfare under Charles Taylor had, from '40 on, done truly remarkable research in six, geographically diverse, rural communities (Jellison 2001). The starting point had been the recognition of the fact that the 'traditional' family farm displayed possibilities that a highly mechanized, capital-intensive agriculture lacked. (Capital-intensive agriculture had attracted most attention before, but then had shown its weaknesses during the Depression). Taylor c.s. wanted to know the impact of New Deal programs on these communities, and how they had weathered the Depression. Close attention was paid to the role of the women, in the course of the research project.

The results left no doubt: **'traditional' family farming, within a stable community that accorded an important role to women, was far more effective than 'modern', single cash-crop farming that left little space to on-farm initiatives of women**.
A whole lot of modernizing assumptions, that for decades had reigned supreme, was invalidated. More important, far wider horizons would open – if only people were willing to leave their strongholds...

> That it was exactly the Amish, and especially their women, that had been most creative and had shown most initiative, was unacceptable to the 'modern mind', yet Taylor's researchers were both able enough to discern the practices, and so independent that they did indeed gave a full account. Stinner et al. 1989 refer to this research project, and give more information also on the sustainable character of Amish agriculture. See Cosgel 1993 for some socio-economic aspects of their practices.
> It was exactly the account of the Amish practices that became rather well known (Kolmorgen 1942). So there evidently is wilful neglect, in the post-war choice to 'industrialize' agriculture. Yet, Europe followed suit, in the wilful neglect of its own peasants/small farmers.
> Note that also the ecological destruction in the US had been well covered in widely circulated publications. Yet, Europe followed its example, and so the CAP (cp. Convery et al. 2002) '*has exacerbated the environmental problems by:*
> *• encouraging greater inputs and specialisation on farms • providing grants for modernisation and amalgamation*
> *• providing support to raise stocking densities in marginal areas; and • providing support for one form of land use, which has increased the subsidies required to encourage other uses, for example conservation, afforestation*'.
> The CAP policies because of their historical roots square with the, more recent, EU Agri-Environmental Regulation (cp. its Art.2, l.c. p.258). Only a complete revision of the CAP, its suppositions and approaches, and especially its concept of 'industrialization', can bring rapproachement.

6.4. Opening horizons - and the role of the war in closing them again

Henry Wallace served his last term as Secretary of Agriculture in 1940. In the Foreword of the Yearbook of that year, "*Farmers in a changing world*", he wrote about the policies being implemented by then (p.vi):

"*It is a sincere effort to contribute to economic democracy in these United States*".

These were no mere words. During the Roosevelt Administration, far greater attention had been paid to the grass roots than had been common before, and that in many and disparate fields (think of Loomax' famous folk song & music research projects for the Library of Congress). The USDA Bureau of Agricultural Economics, that had been organized in 1922 because of the post World War I agricultural depression, now had a Division of Farm Population and Rural Life. This Division had started doing the thorough, integral research of different 'farming styles', with a number of distinct regional groups of farmers, that was mentioned in the previous section. These researches were announced in rather general terms by the Division's director Charles Taylor in his article *'Contribution of sociology to agriculture'* in the 1940 Yearbook. Evidently, leading figures in Wallace's BAE were by then committed to a fully empirical study of rural America. That means, they had started to refrain from pushing their modernization theses, and were intent to learn from the common farmers themselves instead.

This open-minded approach was evident also in other contributions to the 1940 Yearbook. *'Cooperative marketing'* (Stokdyk 1940) and *'Farm-city cooperative associations'* (Gubin 1940) received due attention. Notice that Gubin's subject was a precursors of the *'Community supported agriculture'* of our days. In addition, several authors pointed to an upgraded form of *subsistence agriculture* as benefiting a very large part of the rural populace (Kifer et al. 1940, Baker & Taeuber 1940, Tolley 1940, Wilson 1940). It is pertinent at this point to quote Wilson (p.933), because his *"Beyond economics"* shows that at this time things were considered feasible that, after the war, were completely neglected (to the detriment of both rural America and of peasants everywhere):

> "*For vast numbers of farm people that no other practical plan takes into consideration, small proprietorship with self-sufficient practices could produce a much higher standard of living than is now their lot. A change to self-sufficient agriculture would in these cases constitute material progress rather than retrogression. There is nothing medieval or retrogressive about a family supplying its own food from its own acres by means of progeny-tested hens, blooded sires, hybrid corn, pressure cookers, glass jars, electric refrigeration, and quick freezing. Yet it is precisely by such application of modern technology that subsistence practices can be most effective. There are hundreds of thousands of farm families who produce practically nothing but a single crop of which there is such a market surplus that the price is too low to provide them with the cash to buy the things they need. Yet they remain dependent upon the precarious and insufficient cash income from their one market crop to supply many things they could produce themselves with little or no out-of-pocket costs. Diversity of production to include a supply of their own consuming needs would in the first place reduce the need for cash outlay and in the second place tend to decrease the surplus which stands in the way of a good price*".

In fact, the rural research projects (under Charles Taylor) after 1940 continued for two more years, with highly informative results (see Gilbert 2008 for an account emphasizing the efforts spent in reaching democracy at the 'shop floor'). Yet, by then the war had caused the temporary suspension of democratic procedures everywhere in economic life and introduced far-reaching competencies for war production committees in their stead. Then some people in those committees, in combination with conservatives in Congress, used their power to withhold most of the means – printing facilities, finances – for the publication of the research results. Of the individual researches, only a limited edition was allowed, without the volumes of photographs, and the work on a final evaluative study was cancelled (Jellison 2001). In the War Administration increasingly controlled by conservatives from Congress, the BAE found itself more and more constricted. In '46 a disappointed Tolley resigned and moved to the newly formed UN Food and Agriculture Organisation (then still in its idealistic phase).

A similar fate befell most of the research projects of the Division of Climatic and Physiographic Research, headed by the geographer C.W.Thornthwaite. This Division was part of the Soil Conservation Service that, as of 1935, was part of Wallace's USDA. Employing an array of resources, it gathered information on erosion from the greatly different agricultural regions in the US, from a historical and geographic point of view (Trimble 1985). This research started making the agricultural landscape of the US visible, including the qualities (or the lack of them) of the farming methods of its sociologically differentiated groups of farmers. It was discontinued in 1942 (as were the sociological projects, see above). By then, none of the results of the wider research and only part of the *Physical Land Surveys* – that had been started earlier already and that covered small areas only – had been published. These studies all contain valuable agricultural, historical and also sociological information about the respective areas. Yet, most of the information gathered was shelved (in the National Archives in Washington), till this day.

Of the fate of many, if not most, photographs of the famous Dorothea Lange we know, that all this 'sensitive information' was transferred to the Office of War Information in 1942, and then never left the archives again (Gordon 2006). Also Dorothea Lange's photographs showing the fate that befell Japanese Americans (internment camps with loss of possessions) were censored.
Similarly, all this 'sensitive information' on agriculture was evidently locked away deliberately, only to be discovered again when archives were opened to a new generation of researchers. Clearly some people in strategic positions in 1942, used war-time restrictions to 'clean up' the agricultural research & policy scene.

Henry Wallace: It is important to remember that Wallace himself, though Vice President under Roosevelt in '40 (Markowitz 1973), hardly had an opportunity to stop this dismantling of what was in effect his life's work. For one, his busy schedule obliged him to focus on e.g. relations with Latin American countries. The ideals (of a 'peoples' century' following the war) that he defended, made him win sympathy with many people there, but not with those who were involved with big enterprise in the US. And then, after being appointed head of the Bureau of Economic Warfare by Roosevelt, Wallace in that position resisted the big banker who served as the head of another war-time Bureau, time and again. But this banker made an unreserved choice for capitalism, and by resisting him, Wallace soon met growing opposition from conservatives and big industrialists in the US.
And so, after the disastrous '42 elections, Roosevelt tried to buy the peace with Congress by ending the BEW (and Wallace's position in it) (Markowitz 1973, Stone 1988). Still, as we learn e.g. from his diary (see Blum (ed) 1973), Wallace stayed loyal to Roosevelt. His fulfilment of his duties was exemplary – but left him little time for politics. In the end, in '44 with the presidential elections approaching, Roosevelt chose for the 'low-risk' Truman (Hamby 1973) instead of Wallace. Something that also Eleanor Roosevelt, though increasingly impressed with Wallace, could not prevent (Kirkendall 1984).
So in spite of the perspectives in 1940, Wallace's appointment as Vice President soon caused the decline of his influence. After the '42 elections 'his' USDA could get dismantled, in essential respects, with Wallace neither having time nor being in a position to stop it.

As to the Farm Security Administration, in spite of its enemies among big farmers and in Congress, it got a central place at the behest of the Mexican government in the Mexican Farm Labour Agreement of 1942. The Mexicans had had very bad experiences with American treatment of Mexican agricultural labourers that had been requested to come to the USA during World War I. So when asked agin to send Mexican labourers, they insisted upon the formulation of some clear rules, and upon a central role of the FSA in the execution of the Agreement (Scruggs 1960). The result was an agreement that *recognized rights for the Mexican labourer that the big US farmers always had denied to their farm hands.*
The background to this agreement was rather straightforward: first of all the big Californian farmers saw the dustbowl migration dry up in mid war, because workers went to the new war plants at the Pacific Coast instead. (Read John Steinbeck's *'The Grapes of Wrath'* for a moving account of the fate of Dust Bowl migrants up till then). Furthermore, because they wanted cheap

seasonal labour only, their workers could easily be conscripted. (The full-time farming people were exempt, Blum 1964). This was one more reason for these Dust Bowl migrants to move to the new war industries (that would give them exemption).
In spite of the awful treatment of the Mexican labourers in the past, representatives of the big farmers yet again asked the government to recruit Mexicans while suspending nearly all regulations of the immigration law. But federal government representatives soon discovered that the Mexican government would not comply. On the contrary, it wanted an agreement with specified rights and requested the FSA involvement. One of the reasons of this request was that the FSA's labour camp & health facilities were models of their kind, and the Mexican government wanted thís standard for its labourers.

Then in '43 a new wave of agricultural immigrant labourers arrived, from Jamaica (Hahamovitch 2001). Oliver Stanley, the Secretary of State for the British West Indies appointed in 1942, and a reformer, because of the extremely difficult war labour situation in Jamaica, shelved his worries about the few guarantees of workers' rights in the contract offered by the US State Department. A role for the FSA in the guidance of the workers was important to his decision – as was his rejection of the assignment of farm workers to farms in the South (for fear that they would be abused). But pretty soon the FSA proved to have very limited means only, even though it wanted to protect the workers. Worse, when the FSA refuted the claims of big Florida growers about a pending scarcity of agricultural labour, the War Food Administration overruled and chose to heed the big Florida growers. The Jamaican labour situation being bad enough still, Stanley reluctantly gave in: Jamaican and War Food Administration officials drafted a new agreement allowing Jamaican workers to labour also in the South. Exit FSA.
In what is one of the badly researched episodes in American history – not even the 'why' of the low turnout at the elections has been the subject of research - elections in later '42 changed the face of both the Senate and the House (MacGregor Burns 1989 p.191). It was not long before a kind of final attack on the USDA (as Wallace had built it) was launched. When that occurred the labour situation deteriorated quickly, both for the Jamaicans and for the Mexicans. In Congress, the representatives who no longer had to face a tough Jones, arranged their infamous *"Hearings on the Farm Security Administration"* (78th Congress, 1st Sess., 1943-1944, Pt.3) and succeeded in having the FSA bridled. With that the US became the only example, in the western world, of a nation with a population of agricultural labourers without any rights at all. Its widely heralded large-scale, 'industrial' agriculture was, to put it bluntly, dependent on lawlessness.

> Fortunately much of the ensuing history has become more widely known due to the efforts of the Mexican American **Ernesto Galarza,** who toiled in the California fields as a youth and yet distinguished himself in academic pursuits, completing his Ph.D. at Columbia University in the 1930s (Pitti 2001).

6.5. In the name of Science

After the infamous 'Hearings' things went very fast indeed: the war ended, Roosevelt fell ill and died, and Truman, who was hardly acquainted with many important tasks, took over (Hamby 1973). Remember that Truman, though referring to himself as a farmer, was far more of a speculator, like his father, and a businessman (Kirkendal 1974). Mississippi congress-man and cotton planter Jamie Whitten was by then one of the leaders in the 1946 attacks in Congress on the BAE – attacks that succeeded in having its power curtailed (Summers 2001).

Howard Tolley, who was 57 by then and after years of hard work felt his energies draining away, because of the political repression, resigned as head of the BAE in 1946. He changed over to FAO and took some of his associates with him. A fascinating issue is their relation to Latin American rural social scientists who, especially from the 1960s on, were critical of the dominant development ideology. Yet, within FAO, mainline US politics soon destroyed many ideals of 'economic democracy' and family farming. Its voting power in the many Technical Programs, for which finances became available under Truman's Point Four program, gave the US its financial muscles.
Dundon 2003 p.430 informs us about **Jamie Whitten**: '*Whitten was an extremely powerfull Congressman who entered the House in 1941. His poverty-stricken district was largely agricultural and typified the peonage system of the South. He soon took the chair of the Agricultural Appropriations Committee and held it for decades. He could decide the fate of almost any federal agricultural policy, and, in rage at the exposure of AAA abuses [also in his district; the Agricultural Adjustment Act funds going to large farms, to landlords instead of tenants or sharecroppers] he simply abolished the BAE*'.

The (one) Yearbook of Agriculture for 1943-1947 epitomizes in its title, '*Science in Farming*', the radical changes wrought in the USDA and in agricultural policy. Except for a bit of Home Economics there is no reference to social aspects in the Yearbook, and history has disappeared behind the consistent use of the word 'progress'. In other words, the so-called "natural scientific" approach presented in this Yearbook is a non-historical and a-social one. There is no trace of the integrated-science approach that was so important in the 1940 Yearbook.
Clinton Anderson, the new Secretary of Agriculture, opens his foreword with "*On my farm in New Mexico and on farms the country over I have watched, marvelling, the onward surge of science in farming*". He is silent about the fact that in his case, and in that of most of his fellow big farmers, an essential corollary to this 'science' is the cheap labour of a host of agricultural workers who had essentially no rights (in his case mostly Mexican, cp. Pitti 2001). Yet by then Anderson, Whitten and other stake-holders in big agriculture are convinced that they have higher aims – a conviction that hinders them in becoming fully conscious of the distortions inherent in their presentations. It is worthwhile looking at these two men as examples of a general trend.

Anderson chose *'Life more abundant'* as the title of his Foreword. It is a fragment of a pronouncement made by Christ in the Gospels and used by Anderson because of its religious connotations. Yet by lifting it out of its context, Anderson gives it a meaning opposed to the one in the Gospels, but in accordance with the 'American religion' in which the US is the ideal country, the one and only example to be emulated everywhere. (For a closely reasoned treatment see Marty 1958). This American religion will soon meet its negation in energy- and environment-problems, Vietnam, etc.
Whitten in 1966 publishes his *'That we may live'* as a passionate defence of DDT, against Rachel Carson's *'Silent spring'*. No doubt conceived in close association with the pesticide industry, the book nevertheless is a curious mix of politics & conviction. By 1966 Whitten is the long-term chairman of the House Agricultural Appropriations Sub-Committee, in which position he has (exclusively) financed the further 'industrialization' and scale-enlargement of agriculture for years. His 1949-1994 chairmanship of the Sub-Committee earns him the title of "a kind of permanent Secretary of Agriculture" (Summers 2001 p.320).
With the power of the purse at their disposal, Anderson, Whitten, c.s. took care of the further institutionalization of their policies. But of course, these policies had their material sides, so what about hybrid corn and fertilizer in this connection?

Much of big farm monoculture in the US had poor credentials – as had gradually become clear to the USDA experts in the 1930s. Yet, it was precisely the big farm monoculture, with its low-wage seasonal labour, that lent itself to increasing mechanization and fertilizer use. For mixed farming, that up to the war had been dominating corn growing in the US, such wage labour or increase in fertilizer use was simply not to any advantage.

> Undeniably, also on quite big farms *tractor use* was hardly more effective than *horse-power* (Ankli 1980). The family farmer who wanted to be as self-reliant as possible quite often chose to hold on to animal traction; many others used both horses and tractors.

In the mid-thirties, the mixed farming system, with its rotations in which legumes figured prominently, was still dominant in American agriculture. The increase in the number of tractors caused a decrease in animals held for traction (Kifer et al. 1940 p.512) and consequently also a decrease in the acreage required for feed for those animals. Yet, most of the feed was for poultry, cattle and hogs on the farm itself and so even if he changed over to a tractor, this only led the farmer to a modest increase of his flock.
At this point, this mode of production was not only still very dominant, but there were even increasing doubts about options that formerly had been considered 'more progressive'. In short: a system that largely depended on organic N *also for corn hybrids bred in the public sphere* was up to the war still of over-riding importance. And it is again only when we look more closely at the war years that we find a definitive change.

In the first post-war Yearbook Merle Jenkins gives us information on hybrid corn breeding, just before and during the war, in the South especially. He informs us that progress had consisted especially in the application of 'uniform tests of hybrids' and remarks (p.390)

> *"The uniform tests bring to light and permit the selection of parent lines whose hybrids perform satisfactorily over a wide range of growing conditions. ... [Such hybrids] are not likely to perform badly under some peculiar or unusual set of conditions. The identification of these widely adapted parent lines permits simplification of the hybrid corn program as a few widely adapted hybrids can be made to serve a whole region".*

In a nutshell Jenkins offers us here a pseudo-technical description of choices made with far-reaching consequences. To be able to grasp this, we will take a somewhat closer look at these choices.

In the 30s corn breeding at the Agricultural Experiment Stations in fact saw two main lines, the 'industrial' one and the cooperative one. The first aimed at mechanized corn growing (it had been initiated by father Wallace in the 20s). The second, as we have seen before, aimed at breeding, in cooperation with the regional farmer, hybrids adapted to the regional mixed farm and its resources. During the 30s the latter breeding option was simply part of cooperative breeding – breeding in close cooperation with farmers - at the Agricultural Experiment Stations that had been the standard for decades already. And under the New Deal, cooperative breeding emphatically did not change. As this type of breeding focussed on rotations and the use of legumes, fertilizer use was a side-track. In contrast to this, the mechanized mono-crop option did lend itself to exclusive use of industrial fertilizer. When this led to e.g. lodging of hybrids, this did not lead to doubts, but only to extra efforts in selecting fertilizer-resistant hybrids that would allow mechanical harvesting.

With the effort at ‘uniform standards’ – to which Jenkins refers - it soon became apparent that ‘cooperative’ hybrids, with their adaptation to regional soils and to specific organic practices, were fine-tuned to the regional farmer’s resources, while the ‘industrial’ hybrids had been selected for their performance under high-fertilizer, mechanized agriculture. Public breeders faced ecological complexity, and so were serious about issues of sustainability. Private breeders, to the contrary, by skipping the problems, in fact economized by externalizing the costs of their enterprise. While public breeders did their utmost in e.g. exchanges, they were faced with the increasing secrecy of private breeders, plus their outright theft of public hybrids. The legal instruments were in place for the government to halt this cynical disruption of the public system – but it did not use them.

Yet public breeders were hampered by *a lack of a clear theory on crop N-nutrition in organic agriculture* (if we may thus designate the mixed farming that was still dominant then on most farms even in the US). We will take a short look at this subject, and then, in a later section, study it more closely.

6.6. Transplanting laboratory reality

Many of the efforts at estimating the needs of/potential for crop N-nutrition had, for lack of better ideas, ended up with some assessment of ‘available N’ in terms of ammonium and nitrate. As we shall see, quite a few researchers knew very well that things were more complex than that. Yet, given the problems in exploring organic N in soils with the analytical methods available in pre-war decades, most of them hoped for better times in regard to tests of ‘N-nutrition potential’.
Still there were other researchers who díd change over to tests that at least measured something, even if the results were uncertain at best (conceptually even). They started measuring e.g. ammonium and/or nitrate even though

(a) the great soil (micro)variability as to N-contents and related matters was well-known already in the 1920s (e.g. Post 1924)
(b) the lack of correlation between the ammonium & nitrate measured, and the ‘fertility’ looked for, was common knowledge.

Their ‘measurement of a virtual reality’ has continued right up to the present, as we shall see. Yet, in pre-war decades it was easier to make such mistakes than it is now. I mention two research methods that were in the ascent and that, for lack of evaluation, could easily lead the practical researcher astray.

First, Hoagland was propagating his ‘soil-less cultural methods’, that soon would become very fashionable. Yet from the start he just posited that plant nutrition was a matter of ions-only; as to N-nutrition a matter of ammonium and nitrate only (e.g. Hoagland 1925). But as to N-nutrition, before the war the occurrence (also) of organic-N nutrition of plants was hardly in doubt, and N in soil organic matter a focus of research (Austin 1918). The scientific literature of those years indicates that already then Hoagland c.s. were treading on quicksand.

> The first extensive research into the subject, in the last decade of the 19the century, had been performed at the highest level and since then organic compound (incl. organic-N) nutrition had repeatedly been indicated (Jost 1907; Abderhalden 1914 S.415 refers to work of members of the Academies in England, France, and Germany). An example: E.Hamilton Acton 1889, ‘*The assimilation of carbon by green plants from certain organic compounds*’, Proc.R.Soc.Lond.47(1889)150-175.

Some other publications: Th.Bokorny 1897, '*Über die organischen Ernährung grüner Pflanzen und ihre Bedeutung in der Natur*', Biol.CentralBlatt XVII, Nr.1, S.1-20; and R.O.Brigham 1917, '*Assimilation of organic nitrogen by Zea mays and the influence of Bac.subtilis on such assimilation*', SoilSci.3(1917)155f.
Rather widely known became the exquisite researches of the French plant physiologist Marin Moulliard, whose experiments were reported in his '*Nutrition de la plante*', Tome 2 (1921) '*Formation des substances ternaires*', the chapter '*Nutrition carbonée organique des plantes vertes*', and Tome 4 (1925) '*Nutrition azotée des végétaux supérieurs*'.

But of course, somebody had to disclose that kind of literature to practical agricultural researchers. Moulliard's publications could have served the purpose, but apparently were neglected by fertilizer industry-related research (that because of finances was quantitatively the largest). Moulliard is not so much as mentioned by the '*Literatursammlung aus dem Gesamtgebiet der Agrikulturchemie*' (1931-1939) that after some years came to dominate the scene. As a consequence, a distinct field of research withered during the 1920s and 1930s.

Second, for certain types of plant physiological research the use of soilless culture with clearly defined nutrient solutions seemed to offer new possibilities. Yet, after the dust had settled, it soon became apparent that most plants by far would not survive a pure solution culture. Still, this 'controlled experimentation' greatly appealed to the imagination of many researchers and, eager for a shortcut, they were prepared to change over to the nearest equivalents (gravel or sand drenched in inorganic salts solution).
Note that research following Moulliard c.s. required greater experimental abilities, especially in the field of organic chemsitry, than most researchers could furnish. These were the years in which researchers still could not easily obtain organic compounds or chemical instruments from suppliers. So in the context of its time a temporary focus on inorganic nutrient solutions is at least understandable. But that is definitely not the case with the lack of evaluation of major theories (like those of Hoagland) within agricultural research, especially in Germany that in those decades was dominating the scene.

It will be clear by now why researchers connected with organic-N farming – as in cooperative breeding in the Stations - were in constant danger of copying concepts & methods that in fact hardly pertained to their field of research. In addition to this there was the great diversity of on-farm nutrient situations in family farming. Only an intensification of farmer-researcher cooperation could have offered a way out. But in the US from about 1942 on such cooperation was increasingly put under pressure: 'public breeding' was re-defined in accordance with the seed-companies' wishes and co-operation with the farmers diminished (see Kloppenburg 1988). The 'organic' family farm lost its research support.

Quite in contrast to all this, industrial hybrid breeding was single minded: it went for the cheapest fertilizer available on the market. Because of transport costs, it paid close attention to the presumed concentration of nutrients in the fertilizer materials at hand. Therefore it took a shortcut and started using the easily determinable ammonium & nitrate.
Starting as it did from 'industrial' assumptions, it was not hampered by any ecological-physiological considerations as to crop nutrition. Industrial hybrid breeding had far less qualms about limiting its theory to just the 'industrial' nutrients than 'organic breeding'. The organic breeders were wrestling with life 'out there', and especially with the intricacies of crop nutrition in organic farming (that was the dominant mode of farming, before the war). With the lowering of production costs, mineral fertilizers won the day in 'industrial' breeding, and the big seedsmen focussed exclusively on their use, to the exclusion of organic crop feeding. They devised **laboratory crop nutrition experiments that** limited plant nutrition

exclusively to industrial fertilizers, and **excluded any interaction with the soil and soil constituents** (as we shall see shortly). Soon their high-fertilizer hybrids proved to hold their own in wide regions, provided that the agro-meteorology was not too different and that ample fertilizer gifts were used. Unfortunately it did not come out that their selection procedures had implied the exchange of normal agro-ecologies for highly uniform 'laboratory'-ecologies.

Next the war, with its war productivity committees' emphasis on quick-and-easy results, intervened to make this shift to a 'laboratory ecology' rather definitive. Two points remain to be considered:

First, remember that the corn grown on the mixed farm was largely for on-farm feeding purposes. Only the end product was marketed: beef and/or milk, poultry and/or eggs, or pork/bacon. Yet the war shifted the attention from these 'luxury' products to large-scale production of cereals etc. Large-scale use of the 'industrial' varieties promised higher yields with less manpower (the item that was scarce because of the war). And so we see e.g. a fourfold increase of hybrid corn acreage in the South, between '42 and '46.
Second, already in the beginning of the war it was widely realized that the huge nitrogen fixation industries (Bosch-Haber process), which had been constructed for explosives production in a very short time-span, would produce low-cost fertilizer after the war (as construction costs etc. had been covered from public finances). In the words of the American Society of Agronomy's president Bradfield in '42: *"after the war there will be available for use as fertilizer at least twice as much nitrogen as we have ever used at a price much less than we have ever paid"* (quote: Kloppenburg 1988 p.118).
With attention diverted from the family farm to large-scale agriculture, and industrial fertilizer promising to be very cheap, the organic way, rotations became less and less popular, and more and more farmers started focussing at crop growing with high fertilizer gifts.

Soon the narrow kind of 'science' envisaged by Anderson, Whitten, c.s. would guarantee that the war-like 'productivity', although an 'industrial' productivity concept indifferent to human and ecological consequences, got accepted as the chief standard of selection. The position of public breeding, that because of the emphasis on the war had already come under pressure as to its cooperative breeding work, was weakened more and more after the war. For example, because the Federal Seed Act was not applied properly, cooperative breeding did not receive any protection. With the complex story of this struggle eminently told already by Kloppenburg, I can refer to his book (Kloppenburg 1988 p.105 f.).

Within a few years after the war, the emphasis in corn breeding – and its R & D – was completely diverted away from cooperative/organic to industrial breeding. In the following section we will look at the result, guided by a leading textbook of its time.

6.7. Shrinking agriculture by breeding

Reading the American Society of Agronomy's 1955 *"Corn and corn improvement"*, one is struck by the way the book's wider contributions have been shaped by the book's pragmatic backbone (Ch.V till XIV on 'industrial' corn culture, breeding and processing).
First:
there are no contributions on (a) soil microbiology (b) aspects of corn ecology, and (c) hardly any on roots. These are highly significant omissions, as they express a singular **abstraction from (interactions with) the environment.**

Second:
neither are there any contributions on (a) the ethno-botany of corn (b) the great number of farmers' varieties then still extant (c) the latter's different regional uses and their roles in local dietary patterns (in e.g. Mexico). In the same vein, there are (d) no contributions on the history of cooperative breeding for hybrids in rotation/legume based, organic farming, even though that had been a primary focus in the pre-war Mexico and the US (and still had its defendants in several of the Agricultural Experiment Stations). Together (a) – (d) exemplify a dogged **determination to abstract from the farmer and the local community, and from all farming systems except the 'industrial' one.**
Third:
there is some mention of the role of corn in widening the scope and theory of genetics, e.g. a reference to two of Barbara McClintock's researches (on transposons, see for McClintock: Keller 1993 and Nash 1999). Yet, this remains sterile, for next genetics is shrunken to hybrid breeding. This brings us, ultimately, into the present focus on hybrids also for other crops, with its dangerous reduction of mitochondrial performance and malformation of productive tissues because of the cytoplasmatic male sterility that is commonly used (see § 10.13).

Hybrid breeding is the method of choice not because of e.g. its sustainability and sturdiness, but because it is singularly effective in withdrawing breeding from farmer and local community. Hybrid breeding is for obtaining 'seed & food power', not for exploring the plethora of breeding approaches that are at the service of man and society.
Approaches exploring open pollination (within the local ecology) and plant-microbe interactions (in the local soil) are discarded. What is left is *product research*: the researcher developing hybrids, not because of their inherent qualities, but because government or industry wants him to. When breeding education at large starts focussing at this product research – and it did – the next breeders generation will hardly be prepared to explore the existing possibilities that are inherent to other approaches.

But in the immediate post-war years other approaches than hybrid breeding were still current and an interest in the breeding methods of the Mexican farmer would have been quite normal. What is more, as we know from Carl Sauer, Rockefeller Foundation research in corn and wheat in Mexico could very well have started with an investigation of (a) the ethno-botany of farmers' varieties (b) of their (soil) ecology and (c) of their use in diets (especially their mixes with beans - a glaring omission in e.g. Colwell 1946).
But as it was, Rockefeller research transplanted the recent 'industrial' type of research from the US into Mexico. For example, Miller et al. 1948 added industrial fertilizer gifts as conceived in the US to these Mexican farmers' fields, without attention to the farmers' organic ways of fertilizing, their differential treatment of varieties, etc.
Even when legumes had already been inter-planted with the corn, Miller c.s. did not deviate from their preconceived ways – and with their industrial fertilizer gifts disrupted both the legume's nitrogen fixation and (as a result) the corn's organic nitrogen nutrition.
Before long the Mexican farmers' varieties were collected for use in corn breeding, but that not on their own merits (as to e.g. organic agriculture and use in diets or feeds), but again exclusively on their 'industrial' breeding value.

It will be clear by now that the pragmatics in the ASA's textbook approach was of a narrow kind indeed. Its editor George Sprague's decision to leave out so much that evidently would have been of value in widening the breeders' and other researchers' scope, nevertheless was in accord with the opinion of the US research establishment of the moment. Sprague embodied the political character that corn breeding acquired in those years: he was both of the

USDA's Agricultural Research Service and of Iowa State College. Sprague was in no doubts about the USDA's research objectives:

> *"the objective in plant breeding is to develop, identify, and propagate new genotypes which will produce economic yield increases under some specific management systems"* (Sprague 1971 p.96 as cited in Kloppenburg 1988 p.117).

Breeding was for mechanized, high fertilizer corn growing only. The political choices made from the end of the war on had purged American corn culture, as officially endorsed, from all options but this one. Research was expected to extend the industrial agriculture version, in that way making it **a 'carbon copy' of dominant American industrial research**.

An exception in *"Corn and corn breeding"* is Schneider's *"The nutritive value of corn"*, open as this author is about the many ways in which (the industrial) corn is nutritionally deficient. Yet even Schneider proves not to be conversant with foreign scientific literature, or with the soil scientific literature in English (e.g. the soil microbiological literature). He misses out on e.g. Kohnke & Vestal's 1948 publication on the feeding value of corn - in which they prove the inferior feeding value of high fertilizer corn - as well as on Colwell's 1946 indication of high nitrate contents of high fertilizer corn.
With the other authors contributing to the book – that is authors chosen from the group of foremost American researchers – the lack of acquaintance with prior research from the European continent, and very often also with the wider scientific literature in English, is more glaring still.

In regard to the socio-economic and cultural aspects of the version of agriculture these researchers were willing to promote, the following quotation from one of them will suffice (Stringfield 1955, Introduction):

> *"Since about 1940 corn culture in the US has been purged of much excess weight by the absence of excess farm labour. It has prospered by the great expansion of adapted corn hybrids, by large supplies of low-cost nitrogen, by the extended use of limestone, by better practices in soil conservation, by improved and expanded mechanization, all of these and other benefits largely growing out of institutional and industrial research (Fig.1)."*

Here the (American) industrial model is promoted as the only 'real' one, and farm labor is depicted as only an additional factor of production, with its role prescribed by this 'institutional and industrial research'. As Stringfield continues:

> "*Agricultural education by schools and colleges, extension services, farm papers, et cetera, has been a necessary link in the process of revising corn culture*".

It is puzzling to see all of those experts and educator working in concert to make sure that the next generation will carry blinkers…
As to the Fig.1 that Stringfield mentions, it is a photograph of gravel boxes - with the gravel drenched in industrial nutrient solution – in which corn is being grown. The subscript informs us that

> "*Modern corn culture is based in part on research in plant nutrition. Good corn plants are being grown in quartz gravel. Nutrients can be added or removed at will.*"

Soil is here a mechanical base for the plant, plus an industrial (!) nutrient solution. From the soil scientific literature of the age, corn researchers could have known that this their 'soil' was a complete artifact, missing as it did soil life (!), soil organics, soil minerals and soil (micro) aggregates. The 'soil' Stringfield c.s. are boasting of has scarcely any relation to a healthy agricultural soil. In the parlance of those years, it has no 'crumb structure'. Nor are there any earthworms in it...

The industrial re-definition of ‘soil’ embodied in this ‘advanced’ method of research in plant nutrition cuts the bonds with real-life ecology as well as with the array of proven farmers’ practices. It places all the tools in the hands of the ‘industrial’ researcher, yet it is artifactual to the core. The consequences of this artifactuality were far stretching (as we shall see shortly). It is not to much to conclude that the kind of corn breeding and culture that is propagated in the 1955 ASA volume is of a very specific and local kind indeed: it is determined to impose a highly artifactual ‘laboratory ecology’ on all of corn breeding & agriculture. Soon its aggressive propagation also outside the US will amount to the imposition everywhere of its neglect of e.g. soil & soil life and of the plant’s roles in nutrition other than as the recipient of the industrial nutrients offered to it. Corn growing in that way indeed became a laboratory subject for experts – yet it missed out on most of real-life corn growing by farmers through the ages.

6.8. Nature and society re-entering the laboratory

> *‘Theory is always poorer than experiment, and experiment richer than theory’* - Herman van Riessen

The industrial type of breeding had its field experiments as well, and these gave researchers, who were sensitive to it, clues to ‘another world out there’. Stringfield who was quoted before was one of them. By the 60s he had become quite a bit more reflective than he had been a decade before. Here are some of his pronouncements in his authoritative 1964 review in the Advances of Agronomy series (quotations from pp.113-116):

> *“The best of current homozygous inbred parents are so restricted genetically....that their practical values must always be accepted as tentative, and their use as a calculated risk”*
>
> *“What breeder has not seen hybrids of low defence value, in heavy trouble in farmer’s fields?”*

Stringfield is quite open about the doubtful “benefit” of high fertilizer ratios:

> *“At present, farmers can afford to apply mineral nutrients so lavishly that refined efficiency in fertilizer ratios could not rate as a major breeding objective”*
>
> *“Increased stands were required better to exploit higher yield potentials. Then fertilizers were increased to support the higher stand, and so the spiral has been going”*
>
> *“Farmers continue to thicken their stands hoping to gain more and more return from more and more applied nutrients. Troubles have arisen from excesses in this leap-frog game, and more will follow”*
>
> *“Early stalk disintegration with its accompanying ills and barren or near-barren plants are the most commonly observed troubles when crowding pressure is beyond the tolerance of the hybrid”* .

As is clear also from other sources, the high yielding varieties were **selected** by using a comparatively huge excess of fertilizer. The polluting effects were clear from the start, as were the lowered defences and the physiological & morphological defects of the plants. Yet curiously this did not lead to a probing of the fertilizer concept itself. On the contrary: in a few decades ‘industrial fertilizer’ had been accepted as truly fundamental to an agriculture able ‘to feed the world’. We will shortly look at the arguments – or the lack of them.

Colleagues of Springfield also warned for the extreme genetic impoverishment that had become the ‘standard’ in industrial breeding. For example, Moll & Stuber, in their widely read 1974 publication, warn of the great danger of *'severely limited germplasm'* combined with *'monocultures over large areas'* (p.306). But then, of course, the wide destruction of the US corn crop, that some experts had warned about for a decade at least, had occurred already in the previous year. The 70s would see some important attention to the subject, e.g. the New York Academy of Sciences' 1976 symposium on the matter. Yet, when privatisation won the day and the concentrations in the breeding sector accelerated in the 70s and 80s, silence set in regarding these acute dangers of industrial breeding.

Still these agro-concern concentrations, with their globalisation of the dangers immanent to the industrial HYVs, were themselves riding on the wave that made *industrial breeding part of politics* – hybrid corn breeding first of all. These are the words of R.A.Iones of USDA's Foreign Agricultural Service spoken in '61 (as quoted by Springfield 1964, Introduction):

> *"I think that when the final history of the agriculture of this period is written, they will refer to the chapter of hybrid seed corn as the golden kernel, and they will print the chapter in letters of gold".*

Here industrial hybrid corn, with its high fertilizer requirement etc, is a cornerstone of the Development Gospel. As such it had been eagerly adopted already by the USSR, especially after Chrushchev had visited the Iowa cornfields in 1958 (Hosking 1985 p.358 f.). So with the two super powers accepting the same approach, who could defer?
As it was,
with nature re-entering the laboratory through the back door by way of field experiments, politics entered through the front door and required close adherence to its promising 'development' goals in exchange for liberal finances.

Significantly, breeders, fallible human beings like the rest of us, were only too sensitive to the fame that integration of their labours into the Development Gospel would bring them. And then, FAO itself was actively spreading this gospel (Sen 1960, Wells 1965). So a researcher would not gain popularity by deviating from this 'standard'. Leading researchers of the age, like Norman Borlaug, who received the Nobel price for his breeding wheat HYVs, did in fact conform. We had better realize the human complexity of the situation. These were the hey-days of the 'American religion' before its downfall (see Nashell 2000) and even a strong character like Reinhold Niebuhr got entangled in it (Fox 1990).

In regard to agriculture, it was amazing that in the course of these decades 'industrial' agriculture was regarded as a definitive development. Decades before Fukuyama's *'The end of history'*, the great majority of western policy makers were convinced that we had found the definitive answer to 'feeding the world' with our 'industrial' agriculture. By the 1950s any discussion or evaluation was deemed superfluous - we will see some startling examples later – and 'industrial' agriculture and its course were deemed self-evident.
Again there is this wider 'faith-context'. In the early 1950s people had found a way out of their war-related anxieties by committing themselves to economic 'progress' in Europe as well. But then, of course, a proof of a country's 'take-off into economic growth' was its commitment to 'industrial' agriculture. It was definitely all a matter of circular reasoning. But notice that the 'existential investment' turned doubts into blasphemies...

We already saw that a tenacious belief in centrally conceived S & T was a central dogma of this faith in progress (and as such integral to the 'American religion'). In the words of Rockefeller Foundation President Harrar and his Director for Agricultural Sciences Wortman (Harrar & Wortman 1969 p.90&107):

> *"A vast reservoir of scientific agricultural information and capability is available which can quickly be brought in action.."*
> *"..the fact remains that agricultural change is not contingent on changes in the existent pattern of land distribution. Rather, it depends on well-conceived and well-executed action programs based on the essential components of research, education, and extension"*.

It is safe to say that at this point many of the experts, who were personally acquainted with agricultural conditions in the Third World, started questioning the wisdom of this centralized approach. After all, tenancy problems had been a focus of research for quite some time.

In 1950 the UN General Assembly had given FAO the task to investigate tenure conditions in the world (resolution 401,V). The 1951 *International Conference on Land Tenure and Related Problems in World Agriculture* was part of its implementation of the task (UN-DEA 1951; Jacobi 1953; UN-DEA 1954/1956, world overview & recommendations). So the need for 'changes in the existent pattern of land distribution' was only too clear, and Harrar & Wortman's 'fact' ('change not contingent on changes in land distribution') flies in the face of a reality that was not only widely experienced, but that had been reported extensively.

Yet one thing, especially, obstructed the development of alternative approaches for quite some time: the unconditional faith in fertilizers. In retrospect, an important reason for the growth of this faith was exactly the *absence of evaluation and discussion of fertilizer use*, in post war decades. We will take a short look at the situation in the States.

In 1947 the prominent agricultural scientist Kellogg, in his book '*The soils that support us*', still sees links with traditional farming when explaining (p.232):

> *'The two most important sources of [nitrogen] are barnyard manure and that fixed by bacteria'*.

Yet, a bit later in the same book, he writes that this is of course not enough for *'the large amounts of nitrogen carried'* away with *'our truck crops'*. Kellogg here expresses a matter-of-fact approach, where intensive probing of a difficult question would have been the only fitting approach. The *pragmatism* of researchers like Kellogg made them skip evaluation as soon as a development seemed successful.

And indeed, we meet Kellogg again in the 1957 Yearbook of Agriculture *'Farmer's World'*, where he stresses exclusively the importance of industrial fertilizers. The same can be said of his colleagues Newman and Hill, for whom biological nitrogen fixation and even soil N delivery apparently have disappeared from sight (Kellogg 1957, Newman & Hill 1957).
In 1960 Kellogg is already so used to this dominance of industrial N-fertilizers, that he has no doubts left (neither has he in Kellogg 1964). Indeed, in his development from 1947 till the 60s there is not a trace of thorough evaluation. For Kellogg and his colleagues, the fact that N-fertilizer use in the US had more than doubled from 1950 till 1954 was proof enough. They were pragmatists...

It is safe to say that main line research and policy simply believed that plant N-nutrition had been solved thanks to the Bosch-Haber process. No effort was made to prove anything. They did not even compare the N-nutrition of industrial varieties with that of land-races and wild varieties. In their eyes these industrial developments constituted progress, period.

6.9. Nature's voice? But which language?

> *We cut nature up, organize it into concepts, and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way – an agreement that holds throughout our speech community and is codified in the patterns of our language. The agreement is, of course, an implicit and unstated one,* ***but its terms are absolutely obligatory****: we cannot talk at all except by subscribing to the organization and classification of data which the agreement decreed*'. W.L.Whorf 1941 (cp. Rollins 1972).

During its hey-day, the centralized S & T approach, as embodied in the Rockefeller Foundation's approach, led to absurd pronouncements (about the 'unimportance' of land distribution) that opened the eyes of many experts. The 70s and 80s next showed prolonged efforts to bring about a fundamental change, with experts from Latin America leading the way. Yet, the lure of Harrar and Wortman's concept of a 'vast reservoir of scientific agricultural information and capability' that is centrally available remained strong, also for those critical experts. After all, it was at the heart of the re-assuring Gospel of Modernity, with its promise of 'progress' everywhere thanks to this 'vast reservoir…'.

In the meantime, nature kept looming in the background. After pre-selection of hybrids in nutrient solution-plus-indifferent substrate (quartz gravel/sand), field try-outs could be planned - so Stringfield (1964) warned his readers - as *'carefully controlled tests of genotypes'* in such a way that they lacked *'involvement with critical ecological variations'*. The result was that the *'potential of explosiveness'* of the *'genotype x environment interaction'* would only show up afterwards - and unexpectedly. The full quotation:

> "*The genotype x environment interaction .. has a potential of explosiveness – a potential that may not appear in a short-term study. The most carefully controlled field tests of genotypes often deceive us because they lack involvement with critical ecological variations which have their innings in later seasons*" (Stringfield 1964 p.113)

The apparent simplification that had made the 'laboratory approach' so attractive was just that: apparent only. For with real plants in the real world, manifold interactions with soil and environment dó occur, provided that the breeder does not hinder them to do so. Irrigation, the use of a lot of fertilizer, and pesticide use, constituted a manifest hindrance, as it was an effort to replace local soil ecology with an industrial laboratory artifact. Yet the local soils & ecologies time and again interfered with this projected laboratory ecology, and G x E interactions became manifest. That the main-line breeding research – like Borlaug's earning him the Nobel prize – artificially tried to minimize genome x environment interactions evoked a clear warning from several researchers (e.g. Moll & Stuber 1974 p.295).

Still those researchers had to face the fact that industrial breeding was thriving due to concepts and methods that could be 'brought to the laboratory'. For the laboratory 'translation' (John Law) of agricultural breeding, to be institutionally and economically successful, required its metamorphosis fróm a cooperative ínto an 'expert' system.

The 'experts' of the new institutes could only then be in the centre, if the following elements were removed from there: (1) soil as a non-reducible biotic & a-biotic system (2) symbioses of plants with microorganisms, and (3) farmers with their local practices and ecologies.
Only an industrial breeding system pretending that its R & D was an essentially centralized laboratory endeavour, could be 'owned' by central institutions & industry.

But then, as the experts were convinced that the answers to all the problems could be found in the laboratory, a strict research-and-extension hierarchy had to be devised and maintained, that as such kept farm & farmer subject to central orders. Harrar and Wortman had emphasized the importance of this, and post-war policies exemplify it (e.g. Penders (ed) 1955). As it was, this specific institutionalization (centralization) of breeding was on a par with denying essential roles to the local soil, symbioses, and practices. This included the denial of the, often, superior adaptation of farmers' varieties to their local natural & cultural environment. The researcher who emphasized that these 'outside' entities (outside as to the institute) were essential for breeding defected from the institutes' self-definition.

Surely, in the context of true 'public service' such defection would be possible. But in the post-war US this 'public service' soon got narrowed down to 'providing the seeds industry with the material they want': 'public' was equated with 'good for big business' (Kloppenburg 1988 gives a good account). More recently the global wave of concentrations and privatizations in the seed sector signalled the end of most public breeding...

But before this wave, the complex environment manifested itself in many ways, and the alert researcher receiving those signals also wrote about them (see examples given). Yet, in this description and interpretation he was hampered by his training, for that had acquainted him with 'industrial breeding', but hardly with the soils, symbioses, local agro-ecologies, and local practices without which real plants do not exist. **He needed to be familiar with the 'local plant'**, to be able to express his impressions, **yet he had only the language of 'laboratory plants' at his disposal**.

The directives of the industrial agricultural science approach, as expressed by Harrar and Wortman, had been followed only too well. Because education and extension had obeyed those directives, in all officially sanctioned schools and institutions, the acquaintance with the many aspects of the 'local plant' was largely deficient. Examples:

(a) the knowledge of mycorrhizae, and of soil biology at large, was not to be found within this main-line agricultural knowledge circuit
(b) neither did the training of breeders (or geneticists) familiarize them with farmers' experiences and approaches through the ages
(c) even at the biochemical level the training of main-line researchers and extension personnel did not acquaint them with e.g. root exudates, polyphenolics metabolism, etc.

Note that e.g. research into e.g. mycorrhizae requires a long and intensive hands-on training (in addition to a thorough theoretical one), but the necessary 'tacit knowledge' was nowhere to be found in the main-line research & education circuit. Time and again, we find researchers hinting at phenomena that do not fit into main-line research, yet their own training makes it difficult for them to cross the border into the unknown. And, the more main-line agricultural research & extension got institutionalized, the less 'crossing borders' into regions that did not fit into the industrial paradigm was appreciated... All in all, even if they were aware of unexpected (local) possibilities, or of grave dangers, professional breeders more often than not were hampered in expressing them. It is easy to imagine that those breeders had mixed feelings about deviating from the prescribed paths, especially when they realized that this could lead to financial ruin. A very human 'solution' for them was to give incidental signals only and then quickly to return to everyday tasks.

It will be clear by now that to conclude to the sufficiency of the industrial breeding approach from its impressive post-war institutionalization amounts to circular reasoning.

Besides the *genome x environment interactions*, the importance of *genetic diversity* in breeding was realized, by some breeders. The subject had been grossly neglected in breeding in the US, and the reliance on just a few R-genes pro crop is readily spotted in the USDA Yearbooks of Agriculture. This is curious, because before the war in Europe breeders were definitely aware of its dangers. But now the signals were received, and many varieties (landraces, wild varieties) were collected. Yet, the importance of local interactions *that require in-situ maintenance* of these varieties was not acknowledged. But surely, denying these interactions is of no avail: the maintenance of genetic diversity without the active cooperation of local farmers is hardly an option. This is an important reason to return to forms of *cooperative breeding*, some of which used to be common in pre-war USA anyway.

We come to some provisional conclusions.

1.

In spite of the fact that 'industrial breeding/agriculture' was getting information 'from nature' (as communicated by alert farmers and researchers), at its core there was & is its creed that farmers, soils, crops and ecologies are essentially subject to industrial concepts. This is an assumption that is startling indeed and we will have to take a closer look still at some of its decisive components.

2.

The information coming in 'from society' has been of a mixed character. Recently a closer study of the actual practices of ('non-industrialized') farmers in breeding has brought increasing understanding of the way they operate and of the limited scope of 'industrial breeding'. Yet, in the meantime an accelerated privatization of breeding, by a few global agro-concerns, occurred. It amounts to a concentration of strong, economic forces that want everybody to buy their (few and extremely limited) varieties. And what is decisive: they engage the government to punish those who don't. Here the tension between nature & farmers and those economic forces has reached a breaking-point.

We looked at the 'role model' of industrial agriculture, the US, but it is of course pertinent to look at Europe also, and to include the long-term effects of the war on the post-war agricultural policies & developments. In regard to the ongoing post-war 'industrialization' of agriculture, the role of the Common Agricultural Policy of the EU will also require some close scrutiny.

Yet, before we do that, it is pertinent to take a closer look at the presumed industrialization of agriculture. The post-war creation of a great number of research institutes focussing at 'industrial' agriculture is evident. But just as evident is that they all worked with the same assumptions, e.g.:

(a) soil is 'translated' into an inert substrate + industrial nutrient solution
(b) plants are obediently receiving huge fertilizer quantities and, yet, not changing their metabolism on the way
(c) the environment is made uniform, so that it fits our large-scale high-intensity operations
(d) in spite of farming systems being open systems, we need not bother about e.g. leaks to the environment
(e) the agricultural experts are nominated by institutes & industry
(f) farmers are operating inputs, machines & protocols externally devised in central institutes

(g) highly centralized breeding, delivering the few ‘prime’ varieties, is the way forward
(h) therefore the global breeders & agro-industries are at the centre of it all.

These are startling assumptions indeed, and we do well to continue our examination of this historically unique phenomenon of ‘industrial agriculture’.

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7.

The high-fertilizer construct

as historical phenomenon

‘Industrial’ agriculture is emphatically different from ‘organic’ agriculture. This difference is generally explained as stemming from unparalleled progress, in the change-over from ‘traditional’ to ‘modern’ approaches in farming. Yet, one wonders about the connection between the two. There is no doubt that agriculture was essentially ‘organic’, up till very recently. So did ‘industrial’ agriculture indeed develop out of ‘organic’ agriculture, as the word ‘progress’ suggests? Or did it fail in this respect, because it strived for revolution rather than progress?

It is the widely hailed ‘Green Revolution’ that indeed makes us suspect that much. Still, this desire to be revolutionary, to ‘shake the foundations’ of agriculture, also needs its rational explication. After all, food provision is fundamental to human and social existence, and so we have no choice but to be ‘down to earth’. But then, was the choice to develop high-fertilizer agriculture actually ‘down to earth’?

The first half of the 20th century encompassed the ‘Second Thirty Years War’, which was a total war, and global rather than European in character. Indeed there was no dearth of abrupt changes, not only in the realm of politics, but also in the social, the cultural and the ecological realms. It seems hardly rational just to consider all of those changes as ‘progress’. But then, agriculture was part of those big changes, and we have no choice but to take a close look at its historical post-war ‘revolution’, even in the case that it is akin (for us) to some kind of pseudo-religious conversion. That is not just an abstract possibility: notice that pronouncements like ‘thanks to industrial fertilizer we can feed the world’ have a pseudo-religious ring to them.

Be that as it may, in this chapter we will try to open the Black Box of this intriguing change-over from ‘organic’ to ‘industrial’ agriculture.

7.1. Agricultural modernization as rupture

At the start of post-war’s fast institutional growth of agricultural research it was known that

(a) mineral nutrient concentrations in most soils are low also when biomass production is high

(b) the organic matter quality is a prime determinant of the fertility of agricultural soils.

Yet the new institutional research did not start from this established knowledge, but focused at an ongoing increase of fertilizer gifts instead, to the exclusion of any other option.
But then, this research was part of the government policy everywhere to re-build agriculture the industrial way. Its purpose was to build a high-flow agriculture after the example of big industry - where high inputs were leading to high outputs. The new agricultural research and extension network was purposeful in its attempts to design ‘agricultural factories’ for ever-bigger inputs & outputs. In Cummings’ words (1971 p.80):

> '*there must be: 1. The availability of technical information which will provide the basis for a change in agricultural practices resulting in substantial increases in production. … Sustained progress requires the development of a viable institutional structure which continues to turn out new and more effective innovations which can contribute to increased productivity…*'.

The new institutional network never intended to base itself on former farming systems or on biomass production in ecosystems. It was purpose-built for the design of an 'industrial' agriculture, at the beginning of post-war growth, with government and agro-concern jealously watching over the adherence to this grand purpose. When problems showed up it was decided that only bigger & better-coordinated efforts would do the job. Efforts that needed the active support and direction of the government:

> '*One of the most profound recent discoveries about the process of agricultural modernization is that the laissez-faire approach will not do the job, that leaving agriculturists to their own initiative and resources is not enough, and, conversely, that external environmental intervention is necessary through the use of modern technical, physical, economic, social, educational, and political inputs. … physical, political, biological, economic, social, technological, and educational variables must be manipulated so as to set a stage conducive to achieving the human behavioural pattern required*' (Leagans 1971, p.x).

Post-war agricultural research was part and parcel of the Modernization Gospel of those decades. It was fed by ideologically motivated functional rationality, with its research focus legalized and rigidified (*sensu* Mannheim) by the government.

There was broad recognition that the designs implied a break with former farming systems and plant growth in ecosystems, but this was considered a condition for progress, not a liability. As Schultz expressed it:

> '*The man who farms as his forefathers did cannot produce much food no matter how rich the land or how hard he works. The farmer who has access to and knows how to use what science knows about soils, plants, animals and machines can produce an abundance of food though the land be poor*' (Schultz 1964 p.3).

This was neither (agricultural) history nor science, yet it was convincing to Schultz's public (as to himself). Except for those who knew the differences between industry and a great many other human activities, most people not only nodded, but gave their full support, many a (big) breeder and farmer among them. A quote from a prominent breeder (Bell 1968 p.46):

> '*Amelioration of the adverse effects of environment has been one of the outstanding achievements of agriculture and the acquiring of some degree of independence of the environment by mechanization, fertilizers, protective chemicals and buildings has been a feature of agricultural progress*'.

Yet there were others who expressed their doubts about the new, main-line institutional research. This is proved by the responses which Smilde, of the Dutch 'Institute for Soil Fertility' (which at that time had a government-directed and exclusive focus on mineral nutrition) received at his 1972 lecture in London (cp. Smilde 1972 p.39f.). Devine, for example, stressed that

> (**a**) investment in (modernizing) agriculture '*is colossal, and in some countries such as Holland there is over-investment. The farmer is investing more than he can pay, in fact, and the machines are tending to drive the farmer from their farms because they cannot afford to spend more on investment*'. Likewise he stressed (**b**) that, different from proposals for exclusive indoor feeding ('zero grazing'), '*with proper management grazing can be very efficient, and it does avoid heavy capital expenditure and*

> *also a slurry disposal problem. … There is very little doubt that one of the major problems of the intensive stock farmer today, and probably increasing in the future, is disposal of animal excreta*'.

And Low put up strong resistance to Smilde in matters of rotations:

> '*you write "The need to rotate crops is now being disputed as control of weeds and soil-borne diseases and maintenance of soil fertility and soil structure can be achieved by other means". I am wondering what other means you have in mind for maintenance of soil structure*'. Likewise: '*you say "narrow rotations or monocultures of cereals are being practiced already"* [in the UK, yet in fact] '*there may be the odd field where they can keep up monoculture, perhaps for decades, but the system seems to break down on any scale*'.

Note that those objections point to 'traditional' farming practices being evidently superior – economically and agro-ecologically - to the 'modern' practices advocated by Smilde c.s. of the newly institutionalized research network.

In the same vein 'modern' approaches were questioned, time and again, by those experts who were really acquainted with biological nitrogen fixation (BNF). Jensen (1950), for example, stressed the deteriorating influence of mineral nitrogen fertilizer on BNF and ended his lecture with the words

> '*While extended cultivation of seed legumes … may play important parts both as food and industrial crops in the more immediate future, the really big revenue could perhaps in the long run come from the present vast area of natural pastures*' where even a very modest increase in BNF '*would suffice for a very significant additional production of animal proteins – meat, milk, and wool – and the nitrogen not converted into protein would not be all lost to the soil. … it is perhaps not least along these lines that we may hope to achieve one of the essential freedoms from the Atlantic Charter: the Freedom from Want*'.

Nearly three decades later his colleague Bond was still of the same opinion (Bond 1977):

> '*We also appreciate that biological fixation is a relatively slow process, so that the 250 kg of nitrogen per hectare which can be fixed by a clover stand in one year is probably only a small part of one day's production by a nitrogen factory. However in the long term it will be more sensible to make maximum use of the biological process in order to avoid the depletion of fossil fuels and the damage to the environment that may result from the liberal application of fertilizer nitrogen*'.

Evidently, besides the newly institutionalized agricultural research network with a discourse dominated by functional rationality, there were all the time also 'traditional' researchers for whom substantial rationality loomed large, and evidently the two were quite often in discord. So the newly institutionalized research network was 'superior', not in its science, but in its powerful legal and economic framework. It was a construct of government High Modernity and embodied a break with agricultural as well as scientific tradition.

7.2. Powerful?

> '*Inorganic nitrogenous fertilizer became available at cheap prices following World War II, when the industries that had been producing explosives found themselves without a market. Timing was ripe, as soil erosion had been taking its toll. … nitrogenous fertilizers disguise soil erosion at the same time that they exacerbate it. Adding nitrogenous fertilizer reduces the organic matter in soil by stimulating*

decomposition, and this in turn changes the physical properties of the soil that influence moisture-holding capacity, compaction, and vulnerability to erosion. Ironically, the inorganic fertilizer added to replace lost soil fertility ends up promoting processes that further reduce the soil's fertility and demand further fertilizer additions' (Soul & Piper 1992 p.55/56).

We saw already that there is no exaggeration in statements like Soul & Piper's. In countries like the Netherlands fertilizer application was enhanced in an exorbitant way, also to soils that had been considered fertile before. And indeed, for years already in the Netherlands anecdotal evidence has indicated fertilizer-induced loss of soil fertility, even in the new Zuiderzee polders. In countries like the Netherlands **there is no systematic research into this problem of fertilizer-induced loss of soil fertility because it is unthinkable within the official paradigm**. It is not just that this is the paradigm sanctioned by the Departments of Agriculture everywhere, but it is also reductionist to the core. It comes to a dead end when soil fertility cannot be restored by just adding some more industrial ingredient.

Of course it was not just a matter of pushing the use of fertilizer/industrial explosives, even though this product's links with the centres of power are clear enough. Barkin & Levin's short sketch points to some important aspects of the broader historical context (1998 p.70):

> '*Displacing agronomists who had worked in the peasant tradition, technical staffs* [mostly recruited by governments, the US government first of all,] *applied agro-chemicals, machinery, and non-renewable energy resources to increase productivity for a limited set of crops.* …[FAO, the IMF and the World Bank] *responded by giving their full support for the rising strata of technically "progressive" and commercially aggressive rural entrepreneurs. … the transformation sometimes represented a conscious and deliberate onslaught against a sophisticated local technology. In 1930s Mexico, for example, the Cardenas government strengthened a local extension and research system; the system had been attempting to help peasants who received new lands under the agrarian reform improve their ability to raise crops within traditional diversified milpa productive systems. After determining the political implications of such autochthonous agriculture, Norman Borlaug and a group from the Rockefeller Foundation worked to dismantle the system. … The developmentalist idea that only one pathway of modernization exists had devastating impacts on rural economies and ecologies, closing off serious discussion of alternatives and accepting the harmful impacts of this pattern of development as the inevitable cost of progress'.*

As we shall see in Ch.9, the indictment of the Rockefeller Foundation research is justified. The example of Yugoslavia and Russia made us aware already that politically motivated dismantling of viable autochthonous agricultural systems was likely a general characteristic of 20^{th} century 'progress'. As to the displacement of former agronomists 'who had worked in the peasant tradition', Cline (1971 p.97) gives us some interesting information on British agriculturists in Africa whose

> '*experimental data are sound; the British scientists were thorough, competent workers. They proved conclusively that the use of fertilizer, by itself, was not a "firm technological base" in that environment.* [And he adds] *The British experience in Africa is not unlike that of earlier days in the United States'.* (For the inadequacy of industrial fertilizer in the US see e.g. Harper 1945 p.19f.).

Note that the displacement of agronomists who had worked in the peasant tradition is decisive when evaluating all those 'Green Revolution' promises to fight hunger in Africa and the world at large. Ill-founded, yet great-sounding, promises could only be made after the experts who really were in touch with local soils and agriculture had been side-lined. One wonders if the famines that next developed in many countries in Africa are not only a consequence of political instability, linked with gross economical injustice, but also of reckless agricultural advice from the West. We have ample reason to take a close look at the curious links between ‘science’ and ‘history’ underlying this advice, that indeed was adamant in ‘closing off serious discussion of alternatives’.

In the first post-war years there was exclusive propaganda for hybrid corn responsive to high fertilizer application. It started in the US and was widely adopted, first by FAO, then also by the communist world. In many countries it led to the prohibition of farmers’ varieties. Note that, with this exclusive emphasis, it was low-quality (as to nutritive qualities) varieties of the low-protein crop corn that were aggressively promoted. Autochthonous farming systems rarely produced the inferior nutritive qualities of this high-fertilizer maize, not only because mixed cropping with e.g. beans provided high-quality protein to the diet, but also because their ‘organic’ corn varieties were of higher nutritive value than high-fertilizer varieties. The exclusive promotion of mono-cropping of high-fertilizer varieties (a) denied and disabled mixed cropping of corn and beans and (b) marginalized or prohibited (farmer-centred) use and development of the array of crops and crop varieties known to autochthonous agricultural systems.

Shewry reminded us (2003 p.765): ‘*A wide range of minor crops are cultivated for their tubers, particularly in the tropics. Very few of these have been studied in detail*’. Likewise, it has been observed for years that a wide range of high-quality protein leafy vegetables were current in autochthonous agricultural systems, and that these also received scarcely any attention in our centralized agricultural research network. And as to grains the examples are not less provoking: (a) the war-related prohibition of spelt culture (with its quality protein) in Germany and the Netherlands and the subsequent destruction of the spelt landraces (b) the rejection of buckwheat with its unsurpassed protein content & quality (Ikeda 2002) simply because it proved not amenable to high-fertilizer breeding (c) the forced transition to centrally bred wheat from ecology-adapted barley landraces in China-occupied Tibet that subsequently resulted in a horrible famine. Add to all this that yield statistics, even in those few cases that allow an evaluation of sources, hardly ever show the losses (of other crops) that are part and parcel of the introduction of the HYVs. Altogether the enthusiasm displayed in promoting ‘Green Revolution’ varieties is clear enough, but so is their forceful introduction everywhere thanks to governments changing laws & rules, financing the ‘industrial’ package, and actively disabling peasant agricultural systems that had been viable for centuries.

The corn program was followed on its heels by the Rockefeller program for fertilizer-responsive wheat, a program bypassing the Mexican peasant and focussing on newly irrigated areas in Sonora. Pest problems showed up quite early, as did other ecological and soil problems, and yet ‘engineering’ large irrigated areas for high yields (of this one crop) proved irresistible for many governments. The political character of it all was only too clear. This was the case when the Johnson administration insisted on India’s all-out acceptance of the new varieties as a condition for food aid, when famine threatened the country in the mid-60s. Note that this included the acceptance of dominant US-participation in Indian fertilizer industries (that had to be extended), as well as acceptance by the Indian government of central aspects of American breeders’ rights, and the prohibition of farmers’ seed propagation. Up to this day the introduction of those ‘High Yielding Varieties’ has been depicted as a de-politicized episode of true humanistic assistance, not as the aggressive economic and political manoeuvre it factually was. As to India, it soon became apparent that yield increases (in grain, not in total biomass) were largely limited to

> '*the winter-crop wheat which depends mainly on irrigation, and far less improvement in summer yields. The 1974 projection for the wheat harvest in India was for 30 million tonnes, but only 22 million tonnes were harvested and the decline has continued over succeeding years. … Fertilizer use increased nine-fold in India between 1960 and 1974, and in the Punjab the use of tube-wells went up by a factor of five and pump-sets by threefold. Yet both these measures, apparently so optimistic for the Green Revolution, did not prevent the flattening off in food production. Worse, while there is no worthwhile evidence that ordinary people in India as a whole ate better even during the successful years, there is a good deal to suggest that dependence of the country on outside food resources was growing all the time*' (Dumont & Cohen 1980 p.168f.).

Myrdal in his widely known, three-volume study of socio-economic problems in Asian countries, '*Asian Drama*', in the 60s was adamant already that low grain yields in countries like India were not the result of some imagined 'ceiling' in yields of their own varieties, but the result of the adverse socio-economic (and cultural) circumstances there. If those countries would have dealt with those problems by giving their peasants space to breathe, their own 'non-improved' varieties would have been good for far higher yields then had been obtained in previous years. In later years it was Vananda Shiva who not only pointed out that higher yields with those farmer varieties were possible, but also proved it.
The governments simply ignored Myrdal's documentation and Shiva's proofs and kept promoting the HYVs as miracle seeds. Obviously they unconditionally relied on the unrealistic promises of the Green Revolution and actually believed that it offered a short-cut to the solution of the food problems. Without any doubt political considerations played an important part in this process: the promises of increased power from the centre proved at least as irresistible as the yield increases that were projected….

Neither the wheat nor the corn programs had even been subjected to a mid-term evaluation before they got implemented so comprehensively. And this in spite of the fact that early-on already it was perfectly clear that most new, centrally bred varieties had only a short 'life', due to the fast ecological-evolutionary rise of pest and disease problems (Johnson 1961, Prell & Day 2001 §9.1.5). Pesticides soon turned out to be a part of the same fiasco:

> '*New chemical pesticides were developed after World War II as an offshoot of chemical warfare. … the chemical war on pests has in many ways proven a miserable failure, creating worse pest problems than existed before the introduction of chemical pesticides. This quick technological fix has proven unequal to the myriad dynamics of nature*' (Soule & Piper 1992 p.56).

There were ample reasons to do serious research in the sustainability of the paths chosen. Yet for the time being such research was not even considered. And so an accelerated introduction was also chosen for the high-fertilizer rice varieties that came in focus very soon after corn and wheat. Cummings in his 1971 account is also brimful of optimism about the accelerated multiplication of the International Rice Research Institute variety IR-8, apparently unconscious of the risks. Within a few years IR-8 supplanted rice landraces on vast stretches of land in e.g. Indonesia and the Philippines, the governments heavily subsidizing fertilizer and pesticides. But it was a queer package deal (Cummings 1971 p.78):

‘*The cultural practices necessary for maximum returns include good seedbed preparation, timely transplanting, heavy fertilization, full weed control, and control of stem borers and leaf hoppers with insecticides*’.

There were so many prerequisites that even in the 60s it should have been perfectly clear that the new system was fragile indeed, both socio-economically and ecologically. Note that as to the ecological vulnerability in those years we find e.g. some corn breeding experts expressing clear awareness of impending problems. But evidently most experts were ‘pragmatists’ like Cummings and displayed an impatience, grown considerably in wartime, to promote anything that ‘worked’ (cp. Cummings 1945).

The spokesmen voicing main-line opinion had been ‘sure’ for a long time already that any crop nutrient problem was to be solved with the provision of ample industrial fertilizer. Their tunnel vision led to recklessness: the narrow genetic background of the new varieties, and the grave problems that had already beset the dwarf varieties in which they had their origin, were no secret (Cummings 1971 p.86). And indeed, it lasted only a few years before devastating pests like the brown plant hopper showed up in e.g. Indonesia, pests of which it was perfectly clear that they originated in the extremely narrow focus of the HYV package.

Before long also yields in the IRRI fields, where ample labour and means were available to reach yield ceilings, started plummeting (soon 30-40%). After great research effort at finding the causes for this drop there remained one chief culprit: soil fertility had suffered from the intensive approach and could not be restored with any of the measures the HYV package could offer. Ultimately the excessive use of industrial fertilizer, both in crop cultivation and in crop variety breeding, was at the roots of the fiasco. So we better take a close historical look at the origins of that excessive use.

7.3. ‘Organic’ vs. ‘mineral’ in plant nutrition

With the impressive growth of classical organic chemistry in the second half of the 19th century, chemically distinct research into organic-N nutrition of plants was soon started by some of the renowned researchers of the age. As McKee (1962 p.127) reminds us, Ville 1862/63 had already found methylamine and ethylamine roughly equivalent to ammonia in buckwheat N-nutrition. Then Baeßler 1887 found maize using asparagine as a nutrient, and Lutz 1889, extending the Ville researches, proved uptake of the methyl- till pentyl-amines by higher plants (Blanck 1927 S.174/5). By the end of the 19th century, members of a.o. the English, French and Prussian Academies were engaged in this kind of research (Abderhalden 1914 S.415). Although industrial research was still largely in its infancy, older strands of scientific research were certainly not, and the chemically distinct organic-N plant nutrition research originated there (refer to p.). Then before World War I that research had its follow-up in the US too (e.g. Schreiner’s 1910 and 1912 publications, cp. Schreiner & Brown 1938).

> Related research into N-nutrition of fungi took a close look at organic-N nutrition, both simple and complex, e.g. Bunschoten 1933 reviewed literature from the final decades of the 19th century on.

These were decades in which researchers from a broad academic background displayed great interest in mycorrhizae and biological nitrogen fixation. From Frank’s 1894 fundamental publication to the end of the 1930s, when attention shifted to phosphate uptake studies, both because they were easier and because ion uptake studies were more fashionable among plant physiologists, researchers considered **organic-N absorption** for the plant a primary role of mycorrhizae (Alexander 1983 p.73). Melin proved in 1953 with the new methods then available this organic-N uptake for ecto-mycorrhizae (Melin & Nilsson 1953, cp.Melin 1958 S.286), and in later decades Read stressed the importance of mycorrhizae for organic-N nutrition of plants. In spite of that mycorrhizae-mediated uptake was consistently neglected.

> As a result e.g. Hodge et al.'s (2001) proof of mycorrhizal direct access to organic-N in soil organic material, and Cappellazzo et al.'s (2008) characterization of an amino acid permease of (endo-mycorrhiza) *Glomus Mossae*, came to many as a surprise.

As to the primary role of **biological nitrogen fixation**, both symbiotic and non-symbiotic, in nature and agriculture, some extensive research had been done already at the close of the 19th century. Note that the famous French academician Berthelot was one of those researchers. Although a critical evaluation of his many original pieces of research is quite in order, his proficiency in methods of N-analysis was incomparably higher than that of his critics, so that as a rule there is no scientific reason to doubt the increases in bound nitrogen that he measured.

Versatility in organic chemistry as well as in chemical analysis, as exemplified by Berthelot, was rare then as it is now. So it is hardly surprising that in those very same decades plant physiologists chose to make their methods chemically transparent by using nutrients that allowed easy and distinct determination. Now organic-chemical research in soil processes and plant nutrition was cumbersome, with its requisite 'tacit knowledge' confined to a small group of researchers, well versed in preparative and analytical organic chemistry. There were distinct contributions to the field, e.g. those of Schreiner et al. 1910 (as mentioned in Popp 1931 S.245 and in Schreiner & Brown 1938), but these were contributions of a 'select few'. (Note that suppliers were few and that researchers had to produce their own scientific equipment and do their own compound synthesis).

> **Research received its first impetus to become a mega-enterprise** during World War I, then especially during WWII. Until then it had been mostly 'little science' and suppliers of scientific equipment and of chemicals were few: for 'samples' one was dependent on colleagues elsewhere. Personal contacts and correspondence were important indeed, as in former centuries (Hartog 1914 is an example). More often than not researchers together with their laboratory technicians (if they had any) had to do most work themselves (cp. Michael Polanyi's account of his work of the 20s in Berlin). As a result PhD theses of those pre-war years often give far more practical information than nowadays, and on the whole give the reader a good view of the research practice (e.g. Röhner 1929).
> Theses not rarely explored a new field extensively, as in de Jongh's 1938 high-level work on nitrogen-fixing leaf symbionts. That work was after the war extended by the similarly high-level researches of Johanna Ruinen, but were complete neglected by the new agricultural research establishment (a neglect that testifies to its tunnel vision). Yet, many of the 'classical' aspects of research were still evident in Europe in the first post-war years (e.g. Gerritsen's 1951 ATP synthesis). But once research became institutionalized and 'big style', researchers proved no more able to observe things outside their specialist institutions, where research was dominated by functional rationality, with government and/or industry watching over the adherence to the prescribed goals.

In a way plant physiologists had their own good reasons to focus on mineral-ion uptake studies **but these reasons did not derive from plant characteristics as to nitrogen uptake & metabolism**! In practice plant physiologists before WW II acknowledged the reality of plant organic-N nutrition (e.g. Jost 1907 p.143), yet refrained from researching it themselves. From e.g. doctoral theses from the Interbellum we learn that most of them did not subscribe to the statement of Hoagland c.s. that only inorganic nutrients were of any real importance in plant nutrition. Still, most physiologists explored organic plant nutrition and exudation only incidentally in the course of some other research project. But note that their research clearly allowed for organic plant nutrition (e.g. Robbins 1937).

What is more, during the 1930s Virtanen's research into the subject became widely known and for some years drew attention to organic-N nutrition in the broader research community as well. Note that in 1937 Virtanen was invited to give a series of lectures in biochemistry related to food & agriculture in the UK (universities of London, Reading, Cambridge; cp. Virtanen 1938 Preface). It stands to reason that the invitation stemmed from a real interest in 'organic' agricultural subjects. For authoritative publications, like Hall's in 1919, caused Interbellum research in the UK and elsewhere to stay alert to the 'organic' side of plant growth and agriculture. During those same decades sound agricultural advice in the US took established practices of European farmers as exemplary for the own farmers (e.g. Albrecht 1938). By the same token soil fertility was still approached as an integral concept in the USA, taking the 'established practices' as the norm, e.g. Schreiner & Brown (1938 p.374/5):

> '*In order to understand soil fertility as influenced by organic manures, green manures, and good farming methods study should be made not so much of the nitrogen content … as of the organic chemical changes themselves. In this field of research much remains to be done, but a study of soil nitrogen compounds has been in progress for some years. …. That nucleic acid and others of the protein-degradation products can serve directly as plant food in building up plant tissue has been shown by Schreiner and Skinner (1912)*'.

It is only during World War II and post-war years, with their directive agricultural policies, that the subject gets marginalized everywhere. This is closely related to the marginalization of the small farmer and his intensive use of local resources during that period.
As to broader agricultural research up to World War II, it is essential to realize that nothing like an 'overthrow' of the organic view of plant nutrition had occurred. Both adherents to Thaer's '*Grundsätze der rationellen Landwirtschaft*' (1809) and adherents to Liebig's '*Die organische Chemie in ihrer Anwendung auf Agrikultur und Physiologie*' (1840) had their blind spots and these became apparent in the course of the century. It was commonplace to admit that the **possibility** (as demonstrated in water culture) of mineral plant nutrition did not prove the plant's **limitation** to such nutrition in field soils. The big gap between water culture and soil culture was clear enough, especially due to the severe problems of the water culture itself. And so, even a cursory examination of this 19th century clash of opinions teaches us that, by the end of the century, the organic approach in crop nutrition had not been overthrown, but had matured, with first-rate researchers among its adherents.

By then legume culture, for example, had also been introduced on large estates, with very convincing results (Schultz-Lipitz' work in the 1870s, as finally published in 1881). The microbial legume symbionts had been discovered by Voronin (1866) and their essential role in the N-nutrition of plants defended by researchers like Hugo de Vries (1877). But it was Hellriegel (1886) who established conclusive proof of the fixation of nitrogen from the air by the symbionts. His work also convinced Gilbert and Lawes of Rothamsted, who realized that they had been sterilizing-away the necessary organisms in their own painstaking experiments. Their extensive review of the professional literature (>100 pages in the 1889 vol. of the *Philosophical Transactions of the Royal Society*) marked the end of a century of lively debate.

For some decades, Rothamsted researchers were open to wider biological perspectives on agriculture. Quite decisive was here that Rothamsted director Hall in his 1919 '*The book of Rothamsted experiments*' stressed the importance of the knowledge of (soil) micro-organisms in agriculture, and deplored the common lack of such knowledge.
In those same years Winogradsky a.o. introduced the free-living N-fixers to the scientific and agricultural communities. Winogradsky's research projects were executed at the Institut Pasteur for decades and for a time had their follow-up there after WWII (Waksman 1946; Winogradsky's 1949 Œuvres Complètes; Martre-Coppier 1954). Yet, they were forgotten later on.

All in, all we can safely say is, that before WW I the 'organic ways' were being explored in agricultural and related sciences, with quite a general consensus about (a) the prominence of soil micro-biological factors in nature and agriculture (with BNF an important part of these) and (b) the direct use by plant also of organic-N compounds in soils. Note especially that there was not yet this peculiar post-WW II gap between the 'traditional' approaches of the farmer and the 'scientific' approaches of agricultural R & D. Legume-based rotations as well as farmyard manure preparation were common subjects of attention for farmer and researcher. Yet, already after WW I, the relationship between farmer and researcher would become more and more strained, especially in Germany. In spite of that, the 'organic ways' in farming would stay quite common up till WW II, and in many countries cooperation between farmer and researcher continued accordingly. Then with WW II an abrupt break occurred in this field, which lasted until very recently - when especially agricultural research concentrating on the Third World brought the subject into focus again. An example follows.

Yanni et al. 2001, in an extensive paper based on their address at the *8th International Symposium on Nitrogen Fixation* (Sydney 2000), when writing about the strains of the root-nodules forming rhizobacteria (N_2-fixers), are convinced that (Yanni et al. 2001, p.866/7)

> '*it is significant that [Rhizobial] strain E11 can intimately colonize roots of not only the 'superman' rice varieties that have undergone significant breeding development and require large N inputs to reach their yield potential, but also roots of other rice varieties that perform acceptably on marginally fertile soil without major N-fertilizer inputs. The latter is typical of real-world cropping systems that we hope would likely benefit from bio-fertilizer inoculants that this project will ultimately develop*'.

His designation of Green Revolution varieties as 'superman' is hardly a slip-of-the-pen. Yanni c.s. have ample reason to be cynical, because the Green Revolution breeders had missed the obvious. In fact some of these breeders realized themselves that this was a forgotten chapter in their breeding practices – witness Norman Borlaug's reference in his Nobel prize-lecture to the future importance of BNF. Green Revolution-breeders were, of course, people of their age. Conforming to the age's extreme conceptual and methodical reductionism – a characteristic of US-inspired post-war research in general – their breeding approach missed out on e.g. most symbioses. There is no doubt that they received signals of a field reality that did not fit into their reductionist research paradigm. But in those cases the accelerated centralization of research into sub-disciplinary institutes guaranteed that following up such signals would become well nigh impossible for decades.

So more than a century after Schultz-Lipitz' extensive publication Yanni c.s. re-discovered the high-level agriculture possible with grains-legumes rotations. Like others, but now with more prolific methods, they found intimate *Rhizobium*-cereals associations, and concluded:

> '*there is no longer any scientific basis on which to doubt the existence and potential benefit of the recently described Rhizobium-cereal associations. The 'textbook' version of the Rhizobium life style typically includes two ecological niches: a*

nitrogen-fixing root-nodule endo-symbiont of nodules and a heterotroph that persists sapro-phytically in soil when its specific legume host is absent. This and the other recent studies cited above provide compelling evidence to justify a revision in the rhizobial life cycle that would include a Plant Growth Promoting cereal endo-colonist as a third component when the legume is rotated successfully with a cereal crop … We predict that [e.g.] the rice-legume cropping system has the potential to derive even greater benefit from real-world applications of the knowledge gained through this multinational research project to optimize Rhizobium-rice associations' (l.c.)

And indeed, their research group, comprising researchers from six countries, all conversant with the practices and needs of real-world farmers, may duly be called 'multinational', as distinct from transnational seed firms pushing centrally devised high-fertilizer varieties at the expense of all ecological and social interactions. But then - what made practically the whole western world forget about agricultural research that had not only been exemplary in character, but that had also been widely recognized as such? And why exactly did Germany take the lead in this 'forgetfulness'? Before focussing on that specific question we will once more take a look at post-war's 'acceleration' of research.

We saw already that introduction of Green Revolution varieties was precipitous. But that haste, of course, was hardly unique to breeding. Research guided by functional rationality, and empowered by government and industry to reach the pre-set goal, was continuously in a hurry to introduce more 'efficient' designs. In-depth evaluation, comparing 'old' with 'new', and including social and ecological effects, was hardly deemed necessary, given the general opinion that the new was certainly better than the old. Introducing it now would unquestionably mean a 'big leap forward'! Here it is pertinent to note that Communist China's Chairman Mao's mind-set was to be found everywhere, in post-war decades. In most places, however, there were still some legal rights left protecting citizen and community, and these were effective at least part of the time.

Still the influence of hurried R & D was only too evident in post-war decades, from the construction of big dams to the 'renewal' of city neighbourhoods with high-rise flats. Chargaff, as usually, makes some poignant remarks:

'*Es ist sogar denkbar, daß bei einer dem menschlichen Denken und Fühlen besser angepaßten, weniger hektischen Geschwindigkeit die Forschung auf manchen Gebieten andere, bessere Wege eingeschlagen hätte. Wenn zwischen der Veröffentlichung von Hahn und Strassmann und der ersten Anwendung der Kernenergie nicht acht, sondern achtzig Jahre vergangen ware, wer kann sagen, ob es dann auch zu Hiroshima und Nagasaki gekommen wäre?*' (Chargaff 1988 S.128).

The acceleration of research during WW II caused an impatience in research disciplines - selected by government – which dispelled the long-term perspective from their research. Only of late the guiding importance of 'sustainability' for research was re-discovered (but many parties only paid lip-service).

As to agriculture, we saw already that some researchers, whose experiences predated the war, still used the long-term perspective in evaluating e.g. the exclusive emphasis on mineral fertilizer. Others were more hesitant, acknowledging the need for organic soil husbandry to prevent soil deterioration. They also acknowledged that organic crop nutrition was preferable for farmers in many countries. Yet, at the same time they were greatly impressed by the new all-out introduction of mineral fertilizer-based crop growing (e.g. Åslander 1958).

As it was, the war-related centralization of food policy and agricultural administration, as coupled with the evident power increase of the explosives/fertilizer industries due to the war, induced a sudden change-over to hurried research with mineral fertilizer within the newly institutionalized research. It stands to reason that some aspects of this change had been prepared in pre-war years already.

7.4. Experts adapting to war-related shifts in power

At the end of the 19th and during the first decades of the 20th century Germany had a leading role in many scientific disciplines. So much is evident from e.g. a considerable number of multivolume handbooks in German, which were published up to WW II and after. Conversely, big and sudden changes in Germany quite likely had their corollaries in these disciplines. Indeed, World War I brought cataclysmic changes, strongly influencing a.o. German agricultural chemistry and related disciplines, and the results were subsequently transferred to other countries.

When we read Mitscherlich's (university of Königsberg) '*Review of the German literature on plant nutrition and soil science, for 1924*' in the leading journal Soil Science (Mitscherlich 1925), it is striking to see that he left out … nearly all of the exciting soil science of those years! There is not a hint of soil (micro)biology or of soil morphology. (That may be a reason why later on we find no more reviews by him in Soil Science). This is curious indeed, considered from a scientific point of view, for soil microbiology was in full swing by then, and had already covered quite extensively both mycorrhizae and nitrogen fixation in soils.

Winogradsky reminded his readers (in the 1926 issue of the Ann.Inst.Pasteur, cp. Winogradsky 1949) that Oméliansky's 1923 *'La fixation de l'azote atmosphérique par les microbes du sol'* (Petrograd 1923) had already given a critical discussion of the subject as it had appeared in some 430 scientific publications, by then. Mitscherlich cannot but have known about such first rate, soil-scientific research, because it reappeared in contributions to the 1927 International Congress of Soil Science. There C.Stapp, of the Biologische Reichsanstalt für Land- und Forstwirtschaft in Berlin, gave an extensive review of post-1900 research in BNF (Stapp 1927, cp. Brenner 1927). Note that his colleague from France, J.Magrou of the Institut Pasteur in Paris, offered a comparable survey of mycorrhizae research (Magrou 1927, see also Rayner 1927). By then the relevance of pedology, including soil morphology, had already become abundantly clear due to the work of Russian scientists at the end of the 19th century and up till World War I (Joffe 1929).

Quite revealing is also Mitscherlich's vocabulary of agricultural chemistry. For he makes no effort at all to arrive at biological or chemical speciation, but uses broad 'generalizations' instead. Yet, chemists were speaking about 'N' or 'P' only in the context of total-N or total-P analyses. When treating 'the chemistry of N (or P)' they were always speaking about specific compounds and compound classes. Likewise, biologist started to focuss on specific compart-ments in cells and tissues, with the location of chemical compounds deciding about their effects on the organism. Note also that organic-chemical soil research had been pursued for some decades (e.g. Schreiner's 1910 and 1912 publications), so Mitscherlich ought to have taken notice of it. Instead, the industrial mineral nutrient is always his 'baseline', reminiscent of the approach used by the Liebig school in de mid 19th century. But that approach had already been superseded in the 1860s, e.g. by Fr.Alb.Fallou in his 1862 '*Pedologie oder allgemeine und besondere Bodenkunde*' (cp. Joffe 1929). Furthermore, for decades already speciation had become **obligatory** in both organic and inorganic chemistry.

It is puzzling enough that Mitscherlich is completely silent about the first-rate research of compatriots like Schultz-Lipitz, Frank and Hellriegel, and about the results of internationally renown researchers like Oméliansky and Winogradsky. It is stranger still to see so many others follow his example. The agricultural chemist E.Blanck, for example, does not totally deny organic-N plant nutrition, but he does trivialize it without doing any pertinent research himself (Blanck 1927 S.175f.), the all-too-human attitude of someone not well-versed in the preparative and analytical methods of the organic and inorganic chemistry of those years. Blanck could have been on the alert: his colleague Haselhoff proved, on the basis of experiences during the war, the importance of organic fertilization for sustainable agriculture and the insufficiency of mineral fertilizer in maintaining soil fertility (Haselhoff 1928 S.105 f.).

Yet, Blanck stuck to his emphasis on mineral fertilizer, and that was soon to have far reaching consequences. For he became editor of the multi-volume *Handbuch der Bodenlehre*, that henceforth did no longer pay attention to organic plant nutrition. Popp, for example, stated, in his 1931 contribution to the handbook (Popp 1931 S.245), that '*Ammoniak und Salpetersäure … allein für die Ernährung der Pflanzen in Frage kommen*'. Such a statement presupposes extensive research, preferably personal research, into plant organic nutrition, but in the whole handbook there is no reference to it. Evidently prior events had clouded the perception of most of the German experts involved with the handbook – but the reasons for it are not to be found in the development of academic research.

It was not a 'clash of opinions' that resulted in the disregard of organic plant nutrition by many experts in Germany, but the extensive institutionalization of the agricultural economy and research brought about by the First World War. When Walter Rathenau became director of the 'Kriegs-Rohstoff-Abteilung' in August 1914, it soon dawned on him that, in case of a prolonged and intensive war, Germany would soon be without resources even for direct warfare. He then obtained permission to organize the building of an extensive network of nitrogen fixation factories – as many as the chemical industry could manage (Waeser 1922 S.14f). Due to enormous investments, as a result of which **the state got interlocked with huge German industries** (IGFarben!), Germany became self-sufficient in nitric acid for explosives within record time (cp. also Borkin 1979).

Because there is some confusion about historical details, there follows a short overview of the industrial growth of the Bosch-Haber process up to and during World War I, as explained in Waeser (1^{st} ed) 1922 and Waeser (2^{nd} ed) 1932:
- first patent application Haber process 1908 - small scale trial Bosch-Haber process in Ludwigshafen 25 kg NH_3/day in 1911 - scale-up Ludwigshafen to 1000 kg NH_3/day in 1912 - start NH_3 factory Oppau end of 1913, production soon up to 25 ton NH_3/day as ammonium-sulphate (in effect about 7500 ton NH_3/yr) - after the start of the war change-over and first enlargement of Oppau production to feeds for the explosives industry , to the eq. of about 100 ton NH_3/day at the end of 1915 - further extension to capacity of (the eq. of) some 900 ton NH_3/day in 1917/18.

As indicated, this 'big leap forward' concerned production for warfare. It was achieved by focussing on this large-scale task to the exclusion of any other. Then after the war the state and big industry stressed the 'need' to regain their enormous investments, and we arrived at self-congratulatory statements like Waeser's (1922 S.14): '*Die damals unter den lastenden Zwang des Krieges ins Leben gerufene Industrie erweist sich heute als segen-spendender Faktor für die deutsche Landwirtschaft*'.

The psychological element in such a statement is strong indeed: it could not possibly be true that this work of so many dedicated, top-level experts and industrialists was for destruction only, could it? After World War II similar probing questions were asked by scientists and industrialists in the US, with similar results in terms of the ongoing disregard of any options but the centralized, big industry ones. Here the importance of the psychological factor manif-ested itself more clearly due to the aftermath of the Manhattan Project. It also shows us how a 'peaceful use' of scientific discoveries – this time of nuclear energy – can be institutionalized, without sufficient research, once the state and its bureaucracy put their weight behind it.

Schreiner et al. (1938 p.488/9), in terms of time further removed from WW I, gave a somewhat more neutral account, which points to these same war-roots of industrial fertilizer, however:

> '*The influence of the World War on the production of new fertilizer materials was very marked. The insistent demand for propellant powders, high explosives, and other death-dealing materials, chemical in nature, was so great that huge chemical plants were erected to meet the requirements. The chief concern of all belligerents, insofar as explosives were concerned, was to have a plentiful supply of nitrogen.* … [The German] *utilization of atmospheric nitrogen as a raw material so impressed certain of the allies that steps were taken to install* [similar plants]. … *When the World War terminated, the huge chemical plants, geared to capacity prod-uction of wartime necessities, faced a difficult situation. In order to avoid ruin, these plants turned to the manufacture of nitrogen and other compounds for fertilizer use'*.

The N-fixation industry and industrial fertilizer delivery to agriculture became thoroughly cartelized/monopolized in Germany, during and after World War I, next also in other countries (e.g. Waeser 1922 S.79, and extensively in Waeser 1932). This led to a system in which the state and big industry were, for all practical purposes, hardly distinguishable. From then on the fertilizers from the N-fixation industry were rather aggressively promoted. In Waeser's words (l.c. S.43): '*Um den Widerstand der Landwirte genenüber den neuen Stickstoffdünge-mittel zu beseitigen, errichttete die BASF 1920 mehrere landwirtschaftliche Beratungsstelle*'. The research efforts of the past decades were conveniently forgotten. Turning a blind eye to recent discoveries, industry spokesmen reverted to Liebig's approach of the 1840s. (And even that only ecclectically, for Liebig was convinced, as others would be after him, that plants in general obtained their mineral nutrients from the soil **solids**).

Other countries soon copied the German example. Waeser provides valuable information about the US, especially in his '*Die Entwicklung der Stickstoffindustrie in den Vereinigten Staaten von Amerika*' (l.c. S.196-235). There he relates how in the US the catalytic conversion of ammonia to nitric acid had been introduced shortly before the US declared war on Germany, in 1917. This was one of the results of the European study tour of Charles Parsons, then Chief Chemist of the US Bureau of Mines and subsequently Secretary of the American Chemical Society from 1920 on. The war-induced economic changes in the US were huge indeed: of the 400 million investments (1914-1918) in the chemical industries some 300 million came from the government, and of these, 116 million dollar were for nitrogen fixation industries only. While the US in 1914 exported explosives for some 6 million dollars, in 1917 this amount had increased to some 800 million dollars. The war brought huge financial gains to the explosives factory Du Pont (*Du Pont de Nemours Powder Co.*) which reportedly had the, then hardly conceivable, amount of 1 billion dollar as its own financial capital in 1920. When in 1919 the government began to sell nitrate stocks to the agricultural sector at a comparatively low price, a situation akin to that in Germany developed, and 'agricultural chemistry' became the show-piece of big industry.

We will take France as our last example. That country received the free use of the German patents in exchange for leaving the huge German factories intact (Borkin 1979). The French refrained from demolishing the German nitrogen fixation industry because Parsons a.o. had warned that the technological knowledge of the leading BASF scientists and technologists was not yet available anywhere else. And so we see France making its own head-start into industrial nitrogen fixation, again because of its military importance, promoting agricultural use of industrial fertilizer at the same time. It was as ready as Germany to ignore pertinent research of some of its greatest scientists.

> **The Netherlands**, that stayed neutral during WW I, started with the Bosch-Haber process only after the belligerents indicated. It was the government enterprise 'Staatsmijnen' that took the initiative. It soon followed the example of BASF in pushing industrial fertilizer. With research in the use of organic fertilizer hardly financed, things got completely out of balance - simply from the financial capital amassed in the 'Staatsmijnen'. There was no careful comparison of 'organic' and 'mineral' practices, but from the start only this industry-stimulated research dominated by functional rationality.

In a distinctly war-related way the state and big industry got intertwined everywhere, with a tenacious form of cartelization as a result. The founding of the '*Convention Internationale de l'Azote*' in 1929 made international cartelization an accomplished fact (Groosman & Vingerhoets 1976 p.121f.). So considering the power relations of those decades, it is at least politically understandable that one looks in vain for connections to proven agricultural practices (like rotations with legumes or manure preparations), or to research on organic versus mineral plant nutrition. The one and only perspective is that of industrial product development, and a role for plant and soil of their own is nowhere to be found, let alone a crucial role for the farmer. But then, why did Mitscherlich, Blanck, c.s. as agricultural experts line up so easily behind the industrial party, at the same time neglecting the work of e.g. Schultz-Lipitz, who had demonstrated the need for and possibilities of e.g. legume-grain rotations on large estates?

Mitscherlich focussed on agriculture on the large Prussian estates. There during the 19th century the 'farmers liberation' had taken a different course: permanent tenancy and farm labour were replaced by large-scale agriculture plus seasonal labour by Polish migrants. The Prussian Junker society had its roots in the all-out militarism of Frederic the Great and acquired its definite shape from a scale-up in farming that was accompanied by extensive rural proletarization. Research à la Schultz-Lipitz explored the possibilities of an agriculture, also on large estates, that put the professional skills of tenants and labourers at the centre. But most Junkers denied that they needed the expertise and care of the local farmer, and many experts took this 'fact of life' as his starting point. It simply was not politically correct to focus on possibilities that are opening up when local expertise is granted a central place. The parallel with the rejection of artisanal options in industrial development is manifest.

The idea of creating a type of agriculture that was regulated by a bureaucratic centre, which claimed to be able to provide the means to improve the fertility of the land, appeared to be too tempting for many policy-makers. They despised the idea of being dependent on the expertise and personal care of the local farmer. It was politically expedient to argue that farming could be done the bureaucratic way, and this approach received a lot of legal and financial support. Mitscherlich and the Prussian Junkers were on the same wave-length in this respect and soon their 'large-scale agricultural gospel' spread to other parts of the world. Communist Russia embraced it wholeheartedly, and capitalist countries like England and the USA followed suit (Prianishnikov & Domontovitch 1926, Volk & Tidmore 1945). It all boiled down to the same thing: a complete neglect of sound ideas pertaining to soil fertility and a total rejection of the traditional organic farming practices.

In the Interbellum we see the 'mineral' and the 'organic' approach side by side, in a curious way. There is some solid attention to soil organic matter, to rotations and green-manuring, and to compost and manure preparation (mainline research and extension after the war is completely oblivious to the results of this research). At the same time others can afford to not even mention this 'organic expertise' and just focus at industrial fertilizer or, more insiduously, to refer to industrial fertilizer as the 'standard'.
It is especially the easy equation of plant N-nutrition with mineral-N provision that makes experts turn from those complex organics towards the 'clean' industrial minerals. Within this industrial frame of mind there is a strong inclination to look for laboratory chemical methods to determine 'fertilizer needs', yet at the same time most experts also acknowledge that those methods are 'not yet' satisfactory. Mitscherlich himself displays both attitudes (Mitscherlich 1936).

> Illustrative as to the curious **Interbellum** situation is the late New Deal '*Soils & Men*', the 1938 US Yearbook of Agriculture. The 'organic' side is well represented: Albrecht 1938, Feuster 1938, Nikiforoff 1938, Pieters & McKee 1938, Salter & Schollenberger 1938. But Ross & Mehring 1938 are completely on the 'mineral' side, and one senses that they and others are greatly impressed with this huge fertilizer industry. Schreiner et al. 1938 and Schreiner & Anderson 1938 exemplify something of a mix, yet at the same time they are conscious of the fact that '*after many years of intensive research along this line [of chemical determination of mineral components] … the objectives have been only partially accomplished* .. And they stress: *'if an agriculturist were dependent upon chemical data or any kind of laboratory data alone the results would be very disappointing*' (Schreiner & Anderson 1938 p.470).

Still many experts (and farmers) are enthralled by the huge explosives/fertilizer industry and its easy supply of 'fertilizer'. Even experts who acknowledge a central place for organic matter in soil fertility are tempted to focus at the easy aspects, the supply and use of the industrial 'fertilizers', because they are 'produced to specification' and can be profitably transported. That is the reason that even experts who work for the Tennessee Valley Authority divert farmers away from 'organic' husbandry, with its local resources and net-works, towards an agro industry-dominated 'industrial' agriculture (cp. Staff TVA 1945).

It is a precarious balance in the Interbellum, easily disturbed by major upheavals like World War II. Already in the Interbellum many an author could afford to ignore the 'organic' practices that are at the core of time-honored agricultural systems, thus suggesting that agriculture is merely an extension of the fertilizer industry. Next came wider standardization: note that standardization as an institutional process was anyway dominated by big industry and government. Indeed it did not take long for them to put the world upside down and start 'standardizing' organic fertilizers with Na-nitrate (Popp 1931 S.259). Then, in its turn, such 'unsachlich' (*sensu* Hengstenberg) standardization greatly hindered the acceptance of organic soil upgrading and organic plant nutrition as research subjects in their own right.

> In the 1950s research in organic soil upgrading and organic plant nutrition are finally swept away by the new wave of government-dominated research and extension. It is no exaggeration to refer to this process as the **abolition of the organic core of agriculture**.

The government bureaucracy has been very active in that process of abolition, with its enlargement & empowerment in Depression, war and post-war years a precondition. But it was only with the wartime centralization, which greatly increased the Administration's distance from farmer and farming, that the 'tunnel vision' could prevail within the bureaucracy.

7.5. High-quality research: Winogradsky, Virtanen, Waksman

When we turn from industry-style 'product research', with its functional rationality and tunnel vision, to the small-scale, but often high-quality, independent research in the Inter-bellum years, we soon perceive that not only the government, but industry as well received the timely warnings that there was something amiss with the 'product approach'.

For example, the foremost soil microbiologist of the age, Sergei Winogradsky (Paris), at the end of the 20s warned chemists of the nitrogen fixation industries at least twice about the strong inhibitory effect of e.g. nitrates on growth of and nitrogen fixation by Azotobacter in soils. One of these warnings was given at the *Congrès de l'Azote synthétique*, in Montpellier in May 1927 (Winogradsky 1927a; see Winogradsky 1949 p.647f.). Another one was given during the *VIIe Congrès de Chemie Industrièlle* in October of the same year (Winogradsky 1927b; see Winogradsky 1949 p.660f.). As to its agricultural consequences, Winogradsky had by then cooperated with Lagattu (Montpellier) in field experiments in vineyard parcels for six years. They had pointed out that Azotobacter growth & N-fixation was very clear in parcels that had not received N-fertilizer for six years, and strongly depressed or completely absent in soils that had received such fertilizer in any of those years. (Note that this was shortly before the general introduction of soluble phosphate fertilizer, as distinct from rock phosphate, was to disturb agricultural soil microbiology, with its repression of effective plant-mycorrhizae symbioses).

Winogradsky was the leading researcher on Azotobacters and other soil microbes for decades. This position he had earned especially after he had devised his 'direct method' of soil microbiology. He first presented it in his **opening lecture** to the *1st Int.Congress of Soil Science* and after that then refined and extended its application (Winogradsky 1935). Its chief characteristic was the choice of a soil-like solid state for selective growth of the micro-organism sought for, instead of the solution culture method that was common in industrial microbiology. With this method Winogradsky had discovered the immense differences between the solution-cultured Azotobacters and the soil Azotobacters. One of these differences had to do with their reaction to nitrogen fertilizers: while Azotobacters grown on soil-like plates were being inhibited and suppressed by fertilizer, the solution-cultured species reacted to e.g. nitrate with abundant growth. As others had discovered too (Lipman & Teakle 1925), it was only under soil-like circumstances, while withholding nitrogen fertilizer, that Azotobacter nitrogen fixation was important.
After other researchers had presented similar soil microbiological method developments before or at the 1927 Soil Science Congres (e.g. Lipman & Teakle 1925, Rossi & Ricardo 1927), nobody could be in doubt about the typical nature of soil microbiology and its need for soil-adapted methods. Indeed, we see a modest but steady flow of research in such methods, up to the first post-war years (Rossi et al. 1936, Starkey 1938, Eisenstark & McMahon 1949). And it was the prominent American researcher Waksman himself who in his 1946 biographical assay on Winogradsky once more pointed to Winogradsky's proof of the repression of Azotobacter's N-fixation with the addition of nitrogen fertilizer (Waksman 1946 p.217; cp. also Aso et al.

1939). So there is no excuse for post-war agricultural research and extension to ignore those results.

Before the war probably Waksman was probably the foremost expert on soil organics and humics. He demonstrated the necessity to use lignins to prevent nitrogen losses from the soil, and the lignin-decomposing fungi's preference for organic-N (refer to Waksman & Iyer 1933; Waksman & Hutchings 1935, 1936). Note that by then also 'soluble' soil organic-N was in focus (Sadasivan & Sreenivasan 1939 proved the importance of the movement of organic-N in soil, cp. also Bhuiyan 1949). These, of course, were facts that were essential to the guidance and evaluation of post-war agriculture, and yet they were completely neglected by post-war agricultural research.
Note that outside that circuit investigations like Waksman's had a modest follow-up after the war (e.g. Bendig 1951, Bremner 1951 & 1954). From e.g. contributions to the 5th Int.Congress of Soil Science it must have been obvious to everyone that there was a sequel to Waksman's lignin studies in the independent research circuit, a sequel that, like its predecessor, had a clear agricultural appeal (Prévot et al. 1954, Okuda & Hori 1954, Tinsley & Zin 1954). With Prévot c.s. working, like Winogradsky before, at the famous Institut Pasteur, the quality of their research projects was not in doubt. Evidently, Waksman's war-induced switch to antibiotics research did not cause the split between agricultural R & D and the soil scientific research that was following Winogradsky's and Waksman's lead (his research into antibiotics was a sequel to previous soil microbiological research - Waksman 1937, Waksman & Foster 1937, Waksman & Hutchings 1937; also Martin & Waksman 1940).

We saw already that the high-quality, agriculture- and BNF-related, researches of Virtanen from Finland was soon confronted with the same utter neglect after the war, once more in spite of the excellent follow-up research. Here the newly institutionalized agricultural research circuit took no notice either, but opted instead for a near-complete break with the agricultural and scientific past. Soil microbiology was not to partake in the wartime and post-war accelerated growth of research (noticed in Norman 1946). A new agricultural reality was to be constructed, this time 'industry-like', with high industrial inputs supposedly leading to high outputs.

One of the ominous results was the incapacitation of mycorrhizal symbioses (cp. Jansa et al. 2006). Note that mycorrhizae allow a fine-tuning to the soil, by e.g. adapting their hyphal morphology to the pore sizes that are available, and by helping the plant adapt to drought. The change-over, end of the 1920s, from rock phosphates to soluble phosphates as fertilizer was a major factor in disturbing mycorrhizal symbioses. It could take decades for soils to recover from it (Jansa et al. 2006 p.103, refs.). Impeded uptake of spore elements is also one of the results of industrial-N fertilization. In the words of Buscott et al. (2000, p.606):

> '*We thus conclude that a surplus of nitrogen in the soil will have a negative impact on the plant on the long run; roots with a greatly reduced degree of mycorrhization can suffer from nutrient imbalances as the acquisition of other nutrients by fungal hyphae can become limiting. In addition, plants will probably be much more sensitive to drought stress and to attacks by root pathogens*' .

Mycorrhizae allow a fine-tuning to variable nutrient concentrations in soil, as well as direct access to mineral and organic patches. These qualities allow for constant nutrient supply without e.g. high solution concentrations of minerals, but they are lost in 'industrial' agriculture (e.g. Aiko & Ruotsalainen 2002).
Post-war's 'industrial approach' proved wasteful indeed in agriculture, a wastefulness as to (a) natural resources, (b) the humans needed for their careful management, and (c) disruption

of ecologies due to the loss of mineral nutrients to the environment. The short-term policies of the wartime economy were not alert to such wastefulness, for sure, but how could they give birth to a post-war society in which this wasteful course was considered progress?

It goes without saying that we need the broader historical framework to get a feel for the post-war 'possibility of wastefulness' in the agricultural and food economy. Yet, there is also a decisive war-related cause, and that is the enormous investments made by government in the explosives industries. In the post-war years, in which short-termism was still rampant, those investments translated in cheap industrial fertilizer being pushed as a 'yield booster' without consideration of contexts. The story is told in e.g. the elucidation on the Web of the '*Kenneth A.Spencer Award [2008] of the American Chemical Society for outstanding achievement in agricultural and food chemistry*'. There we read:

> '*In 1941, [Kenneth Spencer] organised and became president of the Military Chemical Works Inc., a wholly-owned subsidiary of the Pittsburgh & Midway Coal Mining Company [his father's and his]. In 1940, Spencer submitted a proposal to the US War Department calling for the development in the Kansas-Oklahoma-Missouri area of a chain of war plants to produce explosives. As a result, he was requested to take on the responsibility of constructing and operating the Jayhawk Ordinance Works near Pittsburgh, Kan., to supply wartime demands for anhydrous ammonia and ammonium nitrate. … At the end of the war, The Military Chemical Works Inc. leased the Jayhawk Works for peacetime operation. The name of the corporation was changed to the Spencer Chemical Company and it ceased to be a subsidiary of the coal company.*
> *The Jayhawk Works was purchased outright from the government in 1948. Additional plants were purchased or built in Calumet City, Ill., Henderson, Ky., Vickburg, Miss., Fort Worth, Texas, and Orange, Texas. … Three years after Specer's death [in 1962], the stockholders … voted to accept a favorable offer from Gulf Oil Company to purchase the company and in October 1963 the sale was consummated*'.

In a pregnant way, the US agricultural economy became an extension of the war economy. It was not just the rise of a 'Military Industrial Complex' (Eisenhower's warning) that endangered the life of the nation, but the all-out transformation of the socio-economy to one dominated by big industry and government in their 'revolving door' unity (cp. also Ch.9).

> It was the third time in recent American history that the economy saw the rise of big economic powers due to huge war profits (direct and indirect). The Civil War initiated the era of the Robber Barons, World War I with its enormous profits for e.g. Ford and Dupont initiated the 'boost' that ended with the Crash, and now WW II brought an unprecedented shift to an economy rules by big enterprise and cheap oil.

With a similar unity soon dominating the socio-economic scene in the communist countries, it became 'unthinkable' that it was not for real. Yet, its roots in war are unmistakable. More specifically in regard to agriculture, we look in vain for any considerations of sustainability. The intergenerational perspective, though normal for most farmers through the ages, lost out completely to considerations of corporate/government power & profit.

7.6. Pragmatics?

It stands to reason that this sudden shift had its preparations in the pre-war agricultural economy. For example, an aspect of the new 'pragmatics' was the sudden growth in the US of large-scale beef, pork, and poultry 'production', with its need for huge feed quantities, that is, a need for feeds coming from extended areas. This growth can help us discern the wider social and economic 'pragmatics' of those years.

In the US up to the 30s **poultry & eggs** constituted a small-scale industry run by farmer's wives (Gisolfi 2006). Especially in the South it was a decisive part of the cash flow of the farm household. Then when feed suppliers contracted farmers for poultry farming, the economy of family farming changed fundamentally: product flow and finances were now organized by the suppliers. Soon the poultry farmers perceived they had lost their independence. Before long their position deteriorated to that of agricultural labourers without any rights at all.

Since the later decades of the 19th century an important part of the **beef** production in the US had been the extensive raising of big herds on vast ranges of land which had been very cheaply procured by wealthy financiers after the Indians had been 'removed'. Soon people holding questionable permits were allowed to use vast stretches of public land for their herds. There was a growing tendency to overstock the ranges, which in the dry 1930s was the primary cause of the Dust Bowl (not just non-irrigated wheat growing). This deterioration of rangelands due to overstocking was hardly a secret in the years around the war (Merrill 1947, McClymonds 1947). When during the 1930s the herds were driven off by the drought and the dust storms, large-scale feeding lots were created which required the 'import' of large quantities of feeds. The impending nutrient overload of the environment, due to the concentration of excreta from the animals, was common knowledge from the start, yet mainline agricultural research kept silent.

Most of the organic materials that formerly had been an integral part of the on-farm nutrient and soil organics cycle were now pursued as feeding materials for the big feedlots. But the small farmers – and even in the US they were the big majority still – were faced with big producers who 'externalized' the social, ecological and health costs of their enterprise. In other words, they were faced with big economic powers that could command big product flows without the burden of keeping social and ecological capital intact. In the atmosphere of especially the war years their short-term 'productivity' was overriding the importance of the long-term needs of e.g. soil conservation, in spite of the fact that such needs had been spelled out rather well by then (cp. Bennett 1940, 1948). As is always the case with wars, the time frame got narrowed to the extreme, and yet it was that frame that henceforth determined rule giving and economic development.

At first the small producer was still sought after for the feeds that the feedlots needed. And for a time it seemed advantageous to him to sell the organic products and buy mineral fertilizer instead (Rubins & Bear 1942). This was especially the case during the first post-war years when mineral fertilizer was cheap (coming from industries that during the war had been financed by government). But with that move the farmer found that his former corn varieties were loosing their yielding potential, adapted as they were to the rotations with legumes that he practiced before. Changing over to corn that had been selected for its response to high fertilizer applications, he was soon to find out that he was no longer served by his former Experiment Station breeders, but was now at the mercy of big private breeders.

By then the big buyers started bypassing the small producer more and more frequently. To collect their huge quantities from those small farms they needed an extensive collecting network, so they preferred buying with big farmers specializing in e.g. the corn crop. The conservation style of farming lost out to big, mono-crop, mechanized farming because it was more ‘profitable’ to assume that mineral fertilizer could do what rotations and organics had done before. In spite of the fact that the importance of conservation farming was generally known, ‘industrial’ agriculture took over and became the focus of the new agricultural research.

Within less than a decade local resource-based small farming was in multiple ways ‘broken up’ by external economic powers. As to e.g. poultry keeping, corn-legume rotation, crop varieties, traction, organic soil fertility maintenance, and small-scale dairying and pig breeding, farmers were dissuaded from using their own local resources and then soon found themselves at the mercy of big economic powers:

> ‘*One chronic source of debt is production loans to pay for purchased inputs: fuel, seed, fertilizer, and pesticides. In recent years, on average, farmers have had to purchase 80 percent of their production items and have produced only 20 percent on their farms. Usually the purchases have required short-term operating loans*’ (Soule & Pieper 1992 p.56).

Important as to the start of this growing debt problem was the shift from the ‘organic’ corn hybrids, coming from the regional Agricultural Experiment Station, to the mineral fertilizer-responsive hybrids from the private breeder.

Some farmers gave up basic self-sufficiency because of the momentary attraction of bigger cash flow. Others because economic powers cut across their local networks of supply and demand. All of them had the access to their local ecological resources disrupted. From ‘farming as a way of life’ that, thanks to its use of local, non-monetary resources, managed to keep a distance from the market, the farmer came at the mercy of an economic system that wanted him to ‘produce for the market’ to the exclusion of all other approaches. Here especially the war economy initiated big changes. Soon enough only the ‘monetary window’ on his agricultural reality was all that was left for the farmer, in spite of the fact that he was perfectly aware that true stewardship for his local resources and environment was thus hardly possible. Before long the present system evolved, as described by Soule & Piper (1992 p.77):

> ‘*This emphasis on short-term economic returns is a major obstacle to the adoption and maintenance of good stewardship measures. It is hard for farmers to justify to their bankers the initial cost of soil conservation methods…because the return, the value of soil saved, is not only difficult to express in dollars and cents, but must be measured over a span of decades. … Many farmers have had little choice because of high debts and marginal incomes. Often bankers have made their decisions for them. Thus the economic values used to judge success in agriculture are often at odds with good stewardship. … The problem is that the short-term time frame generated by the profit motive is incompatible with the longer time scales inherent in natural biological processes. …* [And they conclude on p.121] *A move toward a sustainable agriculture will involve shifting the dominant goals from industrial productivity and efficiency to goals that acknowledge agriculture as a biological and social process*’.

But then, an economy not acknowledging agriculture as a biological and social process is simply out of step with biophysical and human reality. Any viable economic system needs ‘measuring rods’ fit for the field it seeks to explore, and as Odum explains (1994 p.203, 207), it rarely is a monetary one:

‘*calculated… [in a biophysical meaningful] way, the macro-economic value of an environmental resource is usually much higher than its market value. Whereas market values are what is important to small-scale transactions of individuals and businesses, macro-economic values* [esp. such based on solar emergy, cp. Ch.1] *are suggested as the proper evaluation for public welfare and maximizing overall wealth and prosperity. One value should not be substituted for the other’. … ‘Thus, evaluations of natural capital with monetary cost underestimate the contributions to the system’s real wealth. If sustainable natural capital is the objective, underestimating their value will further contribute to their loss. Without feedback reinforcement from the human economy the environmental producers that are necessary for maximum economic production are pulled down by the exponential growth tendencies of the consumers* [cp. wasteful production & lifestyle and/or insatiable human wants], *thus diminishing the wealth on which the buying power of money is based*’.

Indeed, if anywhere, it is in agriculture that we need producers who operate in harmony with the environment and use its resources in true stewardship. Note that the little we know of the environment and its resources in a general way still leaves us at a loss locally. ‘On the spot’ we need local expertise to make sense of it all and to develop careful stewardship. Neglecting those overriding needs, post-war agricultural economics discarded farmer and ecology and shifted to momentary monetary evaluations instead. It introduced **a system that lacks the feedback reinforcement needed for the sustenance of the natural (and human) capital on which sustainable crop growth/food production depends.**

Barkin and Levins (1998 p.71/72) describe succinctly how this system

‘*has transformed and continues to transform the eco-social structure and dynamics of rural society in two major ways. First, the internal redundancy with its buffers against adversity, and the networks of sustaining negative feedbacks, have been undermined. They have been replaced by stronger destabilizing, polarizing, and eroding pathways and positive feedbacks, respectively. Poverty sends young people to work for wages away from the farm. The resulting labour shortage on village lands leads to neglect of public works and the purchase of inputs as herbicides to compensate for unavailable hand labour. The fallow is shortened to use more of the land for production. This strategy increases weed problems and further exacerbates the labour shortage. In addition, the purchases of inputs increase the demand for cash and promote the shifting to crops for market; the monoculture then reduces the productivity of its gardens….Secondly, the entire rural economy has been inserted more completely into the larger systems of the rural-urban dynamic and the global economy. The dominant pathways have become the links to the outside that make the rural communities flow-throughs of the larger economies. The complex criteria that guided the practical decisions of relatively homogeneous communities have been reduced to simpler economic calculations within an increasingly differentiated population or else removed entirely from the control of the local communities. The long-term trend toward trade practices that place peasants at a disadvantage relative to agricultural businesses when prices are determined, and agriculture as a whole at a disadvantage relative to industry and finance, also undermines the effectiveness of the decision-making powers that remain in the village’.*

7.7. Feedback reinforcement – of what?

War frequently led to requisitions in rural regions which put too much strain on the people and the ecology. The feedback reinforcement of natural and human capital that is needed for sustainable agriculture was always outside war's timeframe. Similarly, where political regimes changed over to wholesale exploitation of their population, they also imposed a 'state of war' on their rural regions, again denying the need for feedback reinforcement. The 20th century had two total wars, plus totalitarian regimes over vast areas. So it would have been a miraculous escape indeed if that pattern of 'requisitions thwarting the feedback reinforcement of natural and human capital' would not have had a long-lasting impact on rural society. In plain fact that miracle did not occur.

Instead the blocking of 'feedback reinforcement' is a characteristic of all post-war projects that 'industrialized' our interactions with natural resources. Though it is evident that 'industrialized' fisheries and forestry lack the feedback reinforcement that is essential for their sustainability, post-war R & D focussed on ever-bigger 'harvesting' rates. Oblivious to the caring management of those resources, this R & D with its functional rationality aimed at an always increasing 'production'. Up to this day 'tree harvesting' got always more mechanized, with always larger harvesters causing always greater compaction and erosion, ruining the very capital on which forestry depends. Quite generally, post-war R & D as dominated by functional rationality maximized 'harvesting rates' by neglecting the feedback reinforcement of its resources. The social consequences are no less radical than the ecological consequences. It all is too reminiscent of a 'war regime' to be incidental.
The question arises why the war approach was standardized in peace-time. A central problem is here that substantive research - R & D guided by substantial rationality ('*sachlich*') - after the war was displaced by government-promoted research of the functional type. In the Netherlands this is exemplified by the collusion (about 1950) between dr.Cleveringa from the old agricultural advisory network, with officials and researchers of the newly institutionalized network, on the importance of organics in agriculture. Underlying this collusion was the decision to cut the bonds between e.g. local farmer and researcher, and concentrate R & D in central institutes. Where we first had a network of regionally active, quite autonomous agricultural advisors, it was replaced by a centrally directed, institutional substitute, with an extension network that was typically working top-down. No doubt centralized, war-time regulations have been a prime instigator of this change from farmer-, soil-, and ecology-oriented research, to research abstracting from them.

It is not difficult to find substantive, agriculture-related, research appertaining to the local soil and ecology during the Interbellum. Research into symbioses with mycorrhizae and biological nitrogen fixers figured prominently in it, as did research into non-symbiotic nitrogen fixation and rhizosphere research. At the end of the 30s enough was known of those soil resources to intimate their importance for agriculture. During those same years there was something of a rediscovery of 'traditional' farming practices, in particular because of the destructiveness of much of US agriculture as demonstrated by the 'Dust Bowl'. Moreover, during the 30s many people realized that 'farming as a way of life' was not only perfectly possible, but advisable too, thanks to the plethora of local (non-industrial) resources. It had become clear that the farmer should not get too deeply involved 'the market': he needed a real distance from it if he did not want the continuity of family farming & farm to be jeopardized.

There were, in short, many reasons to take another look at the ways in which the farmer in 'traditional' agriculture was using local ecological and communal resources. And indeed we find in the US e.g. Albrecht 1938 in his '*Loss of soil organic matter and its restoration*' refer to Old World practices (Albrecht 1938 p.356,358). Likewise Pieters & McKee 1938, in their paragraph '*Effect of turning under green manures on yield of subsequent crops*' (p.436 f.), give impressive examples of organically-enhanced soil fertility. The fact that feedback re-inforcement of local resources was a decisive aspect of sustainable agriculture was evidently not in doubt. Yet, main-line agricultural research after the war managed to ignore it all.

At the 1st International Congress of Soil Science in 1927 Magrou had already presented an extensive overview of the importance of **mycorrhizae**. The English top expert on Indian agri-culture, Howard, in his widely read 1940 '*An agricultural testament*', once again stressed the importance of mycorrhizae for crops. And US author Schmidt (N.J.Agric.Exper.Station) in 1947 informed Soil Science readers that especially British researchers had focussed on mycorrhizae research in crop plants in pre-war years. Furthermore before and after the war experts like Melin and Mikola demonstrated the ability of mycorrhizae to use organics – e.g. legumin, purines and nucleic acids – as the N-source for their host plant (Melin 1925, 1959; Mikola 1948). It was known by then that, depending on the relative proportions, mixtures of amino acids often proved better for plant nutrition than ammonium. And as to **biological nitrogen fixation**, Greaves (Utah Agric.Exp.Station) was a leading researcher on non-symbiotic fixation, in semi-arid soils especially (Carter & Greaves 1928, Greaves 1933, Greaves, Jones & Anderson 1940, Jones & Greaves 1943). Following this lead a series of articles on Azotobacter nitrogen fixation in crops was published in the immediate post-war years (Allison 1947, Allison et al. 1947, Clark 1948). And during the 1950s reports remained available, e.g. Wilson's overview '*Asymbiotic nitrogen fixation*' in the '*Handbuch der Pflanzenphysiologie*' (Bd.VIII).

In the 1920s Starkey (like Schmidt from the N.J.Agric.Exper.Station) was already a leading expert in rhizosphere research (Starkey 1929a,b,c; 1931a,b). His 1938 publication on the microscopic examination of the rhizosphere gives information a.o. about *Azotobacters* and *Actinomycetes*. After the war other rhizosphere research was done by e.g. Katznelson (Canada), and soon Rovira and his colleagues from Australia would take the lead. Their research continued for half a century and up to the present. Research into soil nitrogen, as started by Waksman c.s., was continued in post-war years by e.g. Bhuiyan (1949). By then it was known that the major part of soil-N was in the organic matter, becoming available from there first in organic form, so Ghosh & Burris' 1950 publication '*Utilization of nitrogenous compounds by plants*' in fact linked up with an established research field. We saw already that Virtanen from Finland was a leading expert in the field.

Yet, industrial fertilizer research in agriculture already before the war neglects all of this soil research. It does not approach soil fertility as an integral property, with significant contributions from soil (micro) structure and soil (micro)biology, but focusses at mineral nutrients in soil solution (e.g. Skinner et al. 1937). With its complete neglect of chief aspects of the soil, these are 'soil fertility studies' only in name. Bremner expresses his astonishment about it in 1952:

> '*Considering the importance of nitrogen from the standpoint of soil fertility and the fact that about 98 percent of the nitrogen in most soils is organic, the nitrogenous organic complexes of soil have received surprisingly little attention. Very little definite information has been obtained regarding either their nature or their transformations in soil*'.

Bremner's surprise is justified because shortly before the war the importance of the organic side of it all had been stressed, e.g. by Schreiner & Brown (1938 p.374):

> '*In order to understand soil fertility as influenced by organic manures, green manures, and good farming methods, study should be made not so much of the nitrogen content … as of the organic chemical changes themselves. In this field of research much remains to be done, but a study of soil nitrogen compounds has been in progress for some years*'.

Apparently those mainline researchers who limited themselves, effectively, to mineral nutrients in solution, had not even mastered the right 'languages' to 'hear organic-N's voice'. Even biochemical methods proved to be beyond them, also when, in post-war years, these methods were vastly improved with the introduction of chromatography and electrophoresis, methods that made e.g. biochemical speciation possible with rather simple means.

In pre- and post-war years Virtanen was a leading researcher in the fields indicated. In the years before the war his mastery of the, often cumbersome, methods in plant amino acid research was widely known (cp. e.g. Virtanen 1938a). When the development of e.g. adsorption chromatographic methods accelerated from the thirties on (Zechmeister & Cholnoky 1937/1939), first in Europe and then also in the US, it made research far easier. The only reasonable response would have been the general adoption of these techniques by the mainline agricultural research circuit. Indeed careful exploitation of the new methods by the Virtanen school itself soon established broad organic-N nutrition of plants. Miettinen summarized the new research (1959 p.227): '*results clearly show that amino acids are taken up by plant roots without decomposition, and this assimilation may be regarded as a natural phenomenon*'. Virtanen in pre-war years had already demonstrated such up-take of organic-N compounds with legumes (e.g. Virtanen 1938b). Besides Virtanen's school also Boulter et al. (1966) and Jenkinson & Tinsley (1959 and sequels) helped developing methods for the study of organic-N use by plants, while Coulson c.s. developed research methods for the study of polyphenols, including their influences on or within plants (Coulson, Davies & Lewis 1960a, b; Davies, Coulson & Lewis 1964a, b). In spite of it all, the mainline agricultural research circuit persevered in spurning all of this research and its results.

As it was, post-war (mainline) agricultural research missed out on most aspects of the life of soils and plants and focussed chiefly at its own industrial artefacts instead. It was impervious to the signs that indicated the lack of feedback reinforcement of resources which was part of 'industrial' agriculture **because most of the world of soil and plant was outside its frame of reference.** It was not even able to perceive that its 'industrial' approach reached its victories by cutting feedback reinforcement: its exclusive focus at mineral 'nutrients' caused the near-complete neglect of the diverse 'organic' resources needed for the maintenance and enhancement of soil fertility. Its short-term approach stemmed from the war, to be standardized after it due to the introduction of money-only calculations. Therefore it is not surprising that 'industrial' agriculture was conducted 'as if a war was on'.

7.8. Drifting away from substantive research

There is no denying that outside the mainline research circuit there was substantive research into e.g. organic-N uptake and metabolism by organisms after the war. Importantly, it was greatly facilitated by the new chromatographic, electrophoretic and isotope methods (for which see Wieland 1949 etc.). As to plants, this research got extensively reviewed in e.g. the 1950 and 1957 volumes of the *Annual Reviews of Plant Physiology*, so it cannot possibly have escaped the attention. As indicated, researchers like Jenkinson and Coulson made a smart use of those new methods. Yet, it was beyond the scope of mainline agricultural research in those years.

In their recent review '*Plant and mycorrhizal regulation of rhizodeposition*' (2004) Jones, Hodge and Kuzyakov point to the fact that almost all studies have neglected the (plant-directed) influx of C-compounds. Especially where sterile, hydroponic cultures have been used, active uptake will more or less have equalled efflux, with the small net efflux an artefact of the method used. This is one of the reasons why the great importance of soil organic-C compounds for plant growth has been missed by mainline agricultural research. And yet for legumes this importance had already been established by Virtanen (Virtanen 1938b p.46, 47). He ascribed the primary importance of legume-grass associations in meadows and pastures in many parts of the world to organic-N compound excretion by legumes and their active influx in grasses. At that time, only a few other researchers were able to duplicate and extend his results, and the ones who could not were not willing to reconsider their methods. Virtanen's research got discredited, although it was perfectly clear that his mastery of e.g. biochemical methods was superior to that of his opponents (remember he received the Nobel prize for his research in chemistry related to food and agriculture).

After more than half a century it appeared that mainline plant nutrient research took a wrong turn after the war. Yet, all the time there were signals indicating the importance of e.g. organic-N exchanges with the soil (e.g. Rovira 1956, and refs. cited), and the (far) greater exudation in sand culture than in hydroponic culture (Boulter et al. 1966). But, puzzling as it may be, these signals were not picked up by the mainline circuit. Let alone that a coupling was made with the most normal of the soil micro-organisms, the mycorrhizae. Only now, after a detour of more than half a century, the importance of organic-N nutrition and of wider organics exchange (plant-soil) is recognized again. Until recently mainline researchers missed obvious signals from their own field of research due to tunnel vision, and concluded that quantitatively significant organics exchange at the soil-root interface did not occur. The 1957 US Yearbook of Agriculture, for example, shows a picture of research completely dominated by the equation of plant nutrients with mineral (industrial) nutrients. Cp. Dean (1957 p.81):

> '*The nutrient supply originates with the solid phase. The usual path to the plant is from the solid particles to the surrounding liquid and thence to the plant root. The actual transfer involves the movement of ions. .. Chemical and biological processes occurring in the soil solution and at the interfaces with soil particles create the ions necessary for plant nutrition. .. The organic matter of soils is a potential source of nitrogen, phosphorus, and sulfur. .. Biological processes are required to convert these organic sources to an ionic state that is available to plants*'. And as to the nutrient supplying power of soil minerals he adds: '*experiments have been conducted with finely grounded feldspar and apatite, common primary minerals of soils bearing potassium and phosphorus, respectively. When they were applied in quantities comparable with which they usually are found in soils, the rates of supply of the nutrients were insufficient for the good growth of plants*'.

With plenty of 'good plant growth' in diverse ecosystems, there was plenty of good reason to doubt the relevance of the methods chosen for those experiments. Yet everything that could have given a hold – e.g. pondering the function of mycorrhizae – was kept outside the research framework, and instead of 'nature speaking to the researcher', the researcher imposed some industry-like concepts and methods on the reality of soils and plants. His research framework did not deal with soils & plants, but with laboratory glassware containing mineral ingredients in solution. It is hardly surprising that researchers judged 'impossible!'

when people 'from outside' came with 'strange' facts. In their way of thinking industrial fertilizers could do no harm because, after all, everybody knew these were the 'essential nutrients'. Witness Clark in the same Yearbook (1957 p.164):

> '*Do chemical fertilizers, even when they are applied at customary rates, damage the nutritive value of crops? Are they detrimental to the earthworms and to the soil micro-flora? The answer in both cases is no. The nutrients released to plants by decaying organic matter cannot be told from the nutrients applied in fertilizer materials*'.

The last statement needed extensive comparative research to be substantial, yet none was actually done. It simply was a statement moulded by a paradigm in which the reality of soils and plants had been shrunk to a 'nutrient solution' that industry was able to deliver. Indeed Clark in his story about soil life does not even mention mycorrhizae and limits the 'function' of soil micro-organisms to the release of (industrial) mineral nutrients from plant residues and soil organic matter. It is only from within this very narrow paradigm that the extreme impatience can be understood that its researchers displayed when dealing with questions about possible detrimental influences of 'industrial' agriculture in general and of industrial fertilizers especially.

With the end of WW II the faith in the constructability of nature (and society) in the US was stronger than ever. And so we e.g. read with Beeson (1947 p.485): '*There are still vast areas in the world, and in the United States in particular, that will be suitable for many agricult-ural purposes when it is learned how to correct their natural shortcomings*'. Curiously enough authors like Beeson do not even consider the many ways in which 'traditional' agriculture built soil fertility, also on very poor soils. Neither are they critical of the rough dealings of American agriculture with the nation's soils. But then, WW II brought a complete power shift in the US in agriculture, when in mid-war the late New Deal re-discovery of the small farmer and his practices got shelved (cp. Ch.9). The first post-war Yearbook of Agriculture '*Science in farming*' describes a 'science' in the service of the make-over of agriculture after the model of big industry, not a science based on a close and sympathetic consideration of the farming practices of old.
And so Beeson c.s. are convinced that they can solve most nutrient problems with mineral means, e.g. the need for trace elements that by then has become apparent. Note that they did not even consider the possibility that at least part of the trace element problem could be man-made, with chief causes (a) the debilitation of mycorrhizal symbioses by industrial fertilizers and (b) the lack of attention to farm animal ethology. Instead, Beeson c.s. are impatient to remedy the 'natural' (!) shortcomings with their industrial means.

When then groups that want to safeguard the attainments of traditional agriculture stress that things are not that well with industrial fertilizer, and that the nutritional quality of foods and feeds with organic fertilization is different from that with its industrial equivalent, this seems irrational to Beeson c.s. and they react (Brandt & Beeson 1951 p.449):

> '*The organic farming and organic gardening groups have been active in promoting this thesis. Even though biased, they have served to create public interest in the field of nutrition and its relation to health. They have served also as a militant and sometimes maddening conscience and critic for the research worker. A basic divergence exists, however, between the extremist and the research scientist as to methodology and terminology*'.

Note that the 'research scientist' is constructed anew here. He now is sharply to be distinguished from those 'extremists', the 'organic groups', even though former agricultural advisors like King and Howard and former researchers like Hall and Virtanen were closely acquainted with the 'organic practices' and provided decisive reasons to recommend these practices as central to agriculture. Yet, Brandt and Beeson leave us in the dark about the leading role of Virtanen in research into vitamins in feeds and foods (cp. Virtanen 1938c).

> In the years before the war ascorbic acid (**vitamin C**), after a long gestation, had been synthesized and then had been the subject of much research (Niederländer et al. 1938 Ch. B.VII, Karrer 1939 S.751, 752). By then its determination was executed also outside the small circle that had been active in discovery and synthesis (cp. Schoorl 1937). Next the work of Wokes (cp. Wokes 1946, Wokes et al. 1947, and especially Wokes & Nunn 1948 on potatoes) on aspects of vitamin C determination became well known (as that of others, e.g. Hallsworth & Lewis 1944, Kieser & Pollard 1947, and especially Mapson & Partridge 1949). Then György (ed) 1951 offered a welcome overview of relevant studies. For a thorough overview, covering also diverse historical aspects, see Friedrich 1988. For the present (chromatographic) methods of analysis see Levine et al. 1999.

Brandt and Beeson 1951 offers us a rather confused picture: (1) they do not discuss, or even mention, the analytical methods used (2) they mostly compare crops receiving industrial fertilizer-only with crops receiving manure plus fertilizer (not organics-only) (3) when they focus at potatoes, that as to vitamin-C for the population at large are an important crop indeed, they only analyse them 'after 6 months storage at 40 ºC' (mentioned both in the text and in Table 4, so no misprint). All-in, the research approach of this new brand of 'research scientists' is not really encouraging. The more so because by then the extensive discussion of vitamin-C determinations in György (ed, 1951) sets the standard for truly good research. Yet, unaware of the muddled character of their own research, Brandt and Beeson conclude (p.453):

> '*In view of the problems in control found in these rather simple studies, evaluation of the influence of organic fertilization in relation to over-all nutritive quality is obviously extremely difficult. Hence, it seems impractical to devise elaborate, time-consuming, and expensive experiments for a more adequate evaluation of this problem until need for such work is clearly demonstrated*'.

So much about the way in which objections from organic-conscious farmers and researchers have been examined by main-line, post-war agricultural research in the US.

When Schneider (in his 1953 contribution, Ch.IX) gives an overview of work on '*Vitamins in corn*', he does not even mention comparisons of corn grown the organic way with corn grown with mineral fertilizer. Neither does Beeson mention such comparisons when he, in the 1957 Yearbook of Agriculture, discusses '*Soil management and crop quality*'. Instead, after some passing reference to vitamins, he focuses at his 1947 subject, that is, trace elements in soils (though again he is not very specific). He is completely silent about a widely noticed fact: that it is the shift from organic to mineral fertilizer that most often caused the appearance of trace element deficiences (cp. Lehr 1940 p.167 f.). Then when Brunings (1960) edits the volume that gives an extensive overview of mainline agricultural research in post-war decades, there is no mention of organic agriculture, even though the volume is meant to be a guide for agricultural development in the Third World (with its dominance of organic modes of farming).

In the Netherlands Pol's 1960 thesis could have initiated a change: as a general result of his research he found – as others had done before him – that the keeping qualities of potatoes deteriorated with an increase in the application of mineral-N fertilizer. Pol's account of his vitamin determinations is sufficient, as is his account of the starch determinations (where he proves aware of the possibility of big sampling errors, p.54). Yet, he sticks to the use of 'crude protein' as a concept. His results invite a close biochemical analysis, both of his soluble non-protein nitrogen, and of his protein fraction, but none is to be had (neither in Pol's research nor in that of others). Pol had a thorough chemical education: at the Free University, where he had his education, laboratory training was extensive and the famous Coops was one of his teachers (l.c. Woord Vooraf). Yet, his research was financed by a government fund, as was the publication of his thesis, and explorative biochemical analyses were ‘not done’ in the research circuit dominated by the Department of Agriculture. From anecdotal evidence it seems that the researcher often was aware of the problems that were in need of further exploration, but that exploratory research was not facilitated (neither in finances nor in laboratory equipment etc). Pol in the end provided us with a *Summary* that partly mystified the interesting results that he mentioned in his *Conclusion and Discussion*. His research had no follow-up, and main-line agricultural research stuck to its neglect of exploratory biochemical techniques. This consistent neglect indicates that the very thought of comparing organic and ‘industrial’ agriculture was ridiculous, in the tunnel vision of mainliners. Because they were only too sure that traditional agriculture was old-fashioned and antiquated, we now look in vain for any substantial research (by the mainline circuit) that compared organic and ‘industrial’ agriculture in post-war decades. One of the consequences was that mainline researchers were totally unprepared for the serious complications that high applications of fertilizer to grass, for feeds, caused, especially in relation to ruminant fertility and reproduction (as we saw in Ch.5). After some decades of confidently ‘solving’ problems with mineral fertilizers & feed complements, they had neither the concepts nor the methods that could help them to analyse the ruminant problems. When they brought Mg-imbalance forward as the cause of the ruminant problems, instead of venturing into clinical-biochemical research, this was all they had within their paradigm.

7.9. A tragic half-century?

Among the younger generation of agricultural researchers in the Third World there are many who stress the ‘fertilizer-induced unsustainability’ of modern agriculture (e.g. Yadav et al. 2008, Dagar & Singh 2008). That is an important reason why interest in, and building on, ‘traditional’, ‘organic’ farming systems has increased tremendously (Rai et al. 2008, Tripathi 2008). Related to this increase is the keen attention paid to the use of arbuscular mycorrhizal fungi in e.g. pest control (Demir & Akkopru 2007, Sharma et al. 2007).

Yet, the older generation of agricultural researchers in those countries introduced the fertilizer-responsive varieties of the ‘Green Revolution’, based on the conviction that those varieties are superior to the ‘traditional, organic’ ones. They insist that there are some pertinent myths attached to ‘organic agriculture’. P.K.Chonkar is one of them who finds it self-evident that organically grown crops cannot be better than those grown with industrial fertilizer (Chhonkar 2008 p.12, 13):

> ‘*Regardless of whether the nutrients are from organic or inorganic source, plants absorb the same in form of inorganic ions: ammonium, nitrate, phosphate, potassium, etc. Sensors in plants roots, if any, to distinguish between nutrient ions coming from organic or inorganic source have to be still discovered. Once absorbed, the nutrients are re-synthesized into components that determine the quality of produce e.g., flavour, shelf life, etc,., which is the function of the genetic make up of the plants’ variety. Thus any difference in taste of modern high yielding varieties from that of traditional low yielding ones is due to difference in genetic material of these varieties. .. The better taste of the organically grown food is of psychological nature, and could be attributed to ‘Placebo effect’…*’.

Note that within Chonkar’s paradigm he is right. But then, that paradigm, equating plant nutrition with the uptake of industrial nutrients from a solution, was a bizarre one. After the demise of this ‘industrial’ paradigm we realize that for more than half a century researchers like Chonkar have spent their time and energy on a distorted approach of agriculture.

Real agriculture is e.g. about plants roots that are interacting actively with a hierarchic and living soil, with its concomitant soil organic matter (SOM) and soil minerals. Classification of SOM as inert, relative to plant nutrition, hinged upon its designation as a passive source of ionic nutrients. The pre-war research of Waksman c.s. already proved that SOM is not passive, and research from the 1980s on has uncovered enough of its hierarchic and mobile character to intimate that active interaction plant-SOM is the norm. An active interaction in which organic exudates as well as organic compounds that are part of the SOM-hierarchy, play a predominant role.
In fact the conceptual ‘rigidification’ of SOM is one more outcome of this puzzling decade, the 1960s. For in those years Waksman’s approach to SOM was rejected and Flaig’s model embraced (e.g. Flaig1966). That model envisages soil micro-organisms that oxidatively break down plant remains, lignins especially, to phenolics, after which a random polymerization leads to the humics. In this model humics has no longer a hierarchical complexity derived from plant remains, but the far simpler character of an industrial polymer. Hatcher & Spiker stress, in their critical 1988 discussion, that the research of the 60s, including that of Flaig himself, could as well have stayed with the Waksman model, the oxidative modification of lignin. So other things than experimental facts decided about the shift from a hierarchically-complex model of SOM to a rigid-polymer model. Fascination with industrial polymer chemistry – still rather new in the 60s - is one of them. But decisive is that an active, local SOM interacting with active, individual plants and plant roots, did not fit into the picture of a soil inertly waiting for the instructions of institutionalized expertise.

Hatcher & Spiker 1988 is one of the contributions in Frimmel & Christman (eds) 1988. In that same volume, Müller-Wegener re-introduced the concept of direct **interaction between active humics and biota,** as well as interactions of **humics with compounds influencing/ regulating the growth of plants and micro-organism**. As to the hierarchic character of soil aggregates and the relative mobility of SOM in it cp. e.g. Baldock et al. 1990, Puget et al. 1995, Jastrow et al. 1996 and Chotte et al. 1998. Lattao et al. 2008 is a very recent study exploring the character of the hierarchic assemblage that is SOM. Piccolo 2001 explains its super-molecular structure. Nardi et al. 2000 is one of the studies emphasising the active interaction of the plant with the SOM by way of root exudates.

Note that also as to SOM we are back where it all started: at active plant-SOM interactions in which exudates as well as SOM components play distinguished roles. SOM only got ‘frozen’ when industry and its researchers gave it a passive role and neglected to do research in active plant-SOM interactions. The ensuing picture of a petrified soil and SOM implied that plant and farmer had to wait passively for the ‘mineralized’ nutrient, unless the fertilizer industry came to the rescue. But nowadays we realize once more the active character of plant-SOM interactions and we also realize again that the ‘living soil’ offers the farmer great scope for careful interventions. The fertilizer industry, its researchers, and its advisors are not ‘at home’ locally, and so offer no help in those interventions.
A living soil with pro-active plants needs careful interventions by the local farmer who is familiar with the local environment and practices. Research perspectives are now far wider than under the ‘petrifying’ paradigm (note again the re-appearance of organic-N nutrition of plants, as in Jones et al. 2004). Much exciting work is to be done after more than half a century of delay. At any rate, method development has continued, so that the researcher has more ‘instruments’ at his disposal (e.g. Michalski & Shiell 1999, Salaün & Charpentier 2001).
As indicated there is also a tragic side to the wide perspectives that have opened up for farmer

and researcher: ‘industrial agriculture’ as a model project of post-war High Modernity was evidently ‘not of this earth’. After more than half a century of postponement of evaluation, we sense that our accelerated post-war institutionalization of ‘industrial agriculture’ has alienated us from the farmer, the plant, and the soil. That this construct is not sustainable is now widely admitted. But what, in fact, have we accomplished up to now? If the example of Chhonkar is at all representative, there is definitely a big element of tragedy in our post-war efforts.

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8.

Island research

8.1. Why transfer to an island?

In Ch.7 we already looked quite extensively at the frame of mind dominating mainline agricultural research after WW II. The first post-war US Yb. of Agriculture '*Science in farming*' had sanctioned a peculiar kind of 'science' for the benefit of its 'industrial' agriculture. Institutionalized to the exclusion of all other options, its peculiar 'scientific approach' soon came to dominate the minds of the researchers who, from conviction, worked hard to implement its goals. After a decade they no longer remembered any other 'scientific' approach to agriculture than their own. We quote once more (as in §7.8) Clark from the 1957 Yearbook:

> '*Do chemical fertilizers, even when they are applied at customary rates, damage the nutritive value of crops? Are they detrimental to the earthworms and to the soil micro-flora? The answer in both cases is no. The nutrients released to plants by decaying organic matter cannot be told from the nutrients applied in fertilizer materials*'.

We look in vain for any comparative research in support of the last statement, in spite of exciting research exploring the bio-organic chemistry of soils and plants with the chromatographic and electrophoretic methods of analysis that had become available (some positive examples: Mackenzie & Dawson 1961, Muir et al. 1962). It simply expresses the narrow paradigm that had shrunk the reality of soils and plants to a 'nutrient solution' which industry was able to deliver. By the time of Clark's statement mainline agricultural research apparently deemed itself 'of age' and no longer in need of such explorative approaches. It was obviously already a central part of all serious projects aiming at the accelerated development of the Third World, and that was proof enough of its qualities.

Moreover, its idealism was infective, and researchers in Third World countries were eager to follow its injunctions. For half a century mainline agricultural research all over the globe worked within its narrow paradigm, and did so from conviction. The extent to which it ruled the minds of this older, idealistic breed of agicultural researchers in Third World countries as well is apparent in a (2008) quote from its Indian representative Chhonkar (see §7.9):

> '*Regardless of whether the nutrients are from organic or inorganic source, plants absorb the same in form of inorganic ions: ammonium, nitrate, phosphate, potassium, etc. Sensors in plant roots, if any, to distinguish between nutrient ions coming from organic or inorganic source have still to be discovered. Once absorbed, the nutrients are re-synthesized into components that determine the quality of produce, e.g. flavour, shelf life, etc., which is the funcion of the genetic make up of the plants' variety. Thus any difference in taste of modern high yielding varieties from that of traditional low yielding ones is due to difference in genetic material of these varieties. .. The better taste of the organically grown food is of psychological nature, and could be attributed to 'Placebo effect' …*'

It is the reduction of the interactions of plant and soil to just those 'allowed' by the mineral nutrition concept that is striking in Chhonkar's account. It is also informative that in it a genetic-deterministic picture of the plant is in sine with the narrow concept of plant nutrition. Both opt for a 'reality' that conforms to direction from a distance, without 'confounding' interactions with the environment.

Plant geneticisists in the 60s certainly knew about the great extent of genome x environment interactions. But that information was not well received by the majority of agricultural researchers.

> Instead, we see the enthronement of despot DNA by Watson, Crick, and Monod, and their adherents. In spite of high-level endorsement (e.g. by the editor of Nature, cp. Stokes 1982), it lasts only for a time. By 1980, after crystallographer Wang c.s. have made a breach in the DNA fortress, there is a change-over to 'dynamic DNA', and subsequently research into the roles of other macromolecular cell compounds makes a re-start (e.g. the rise of glycobiology). This short sketch suffices to remind us of the fact that the 50s, 60s and 70s constituted a naive age, ready to believe in an unattainable '*claire et distincte*' world. When we considered the social and institutional framework in which mainline researcher Schuffelen had to work (Ch.4), we saw that he would have needed a touch of genius to rise above this peculiar age. During those decades the 'educated man' was convinced that e.g. the Periodic System – and with it much of chemistry – followed from the Rutherford model of the atom (obsolete by then for half a century), and that mathematics became much easier for the young mind when some 'set theory' was at the centre of mathematics education. It is clear that this was not conducive to a re-evaluation of research that recently had experienced an accelerated growth, as was the case in post-war agricultural research.

Chhonkar just echoes the convictions of the post war decades. Allison's '*Soil organic matter and its role in crop production*' (1973) hardly gives any information on e.g. organics exchange at the root-soil interface, or about active humics-plant interactions, and his chapter '*Formation and nature of organic matter*' is largely devoid of chemically specific information (= speciation of compounds and organic groups). Similarly, in his chapter '*The soil and living matter in it*' Allison mentions mycorrhizae, but gives no further information or references. And this in spite of the fact that the subject had been studied by e.g. French and Britisch researchers in the Interbellum (cp. Hartley 1949).

> In fact, when we progress from the 1950s to the 1960s there is apparently a regress in soil chemical research. After the war and till the mid-50s J.M.Bremner, who was trained as a chemist, did explorative research at Rothamsted in which chemically specific information was obtained (e.g. Bremner 1950, 1958). But then, still at Rothamsted, he increasingly changed over to non-speciating techniques, discarding the use of e.g. chromatographic methods (e.g. Bremner & Shaw 1955, Bremner 1957). Next he was appointed professor of soils in Iowa, and limited himself completely to mainline's non-specifying chemical research of soils. A direct consequence is that his 1960 publication on Kjeldahl nitrogen determinations in soils is partly wrong (but note others took it to be definitive!). Likewise in his 1968 and 1969 publications on nitrite interaction in soil, chromatographic methods have been abandone (Bremner & Nelson 1968, Nelson & Bremner 1968, Bremner & Hauck 1969; this same abandonment we meet with others, e.g. Bulla Jr. et al. 1970). An ill-fated development: now we had to wait a long time for the re-introduction of speciating methods, in spite of the fact that e.g. the extent of reactions of nitrous acid with soil organic matter had been recognized from about 1960 on (cp. Allison 1973, p.272/3). And so, for year we would remain unaware of the persistence of fertilizer-induced nitrosamines in soils and plants, etc.

Allison and Bremner were able people, so what had obstructed truly explorative research? We find an indication in Allison's depreciative account of the person and work of Sir Albert Howard, long term agricultural consultant in India and author of the widely known '*An agricultural testament*' that was based especially on Howards decades-long interactions with farmers in India. In many ways Howard was a pupil of F.H.King (on whom see Tanner & Simonson 1993), whose '*Farmers of forty centuries*' (1911 and later) gave a careful account of sustainable agricultural practices in Asia. Howard did only have a thorough knowledge of organics-based agriculture, but just like King he had an an acute sense of research and broad ranging interests. His human-centred management of Indian labour, for example, became widely known (he gave an account in the Int.LabourRev. XXIII No.5, 1931). Furthermore, a biography of Howard had circulated widely after his death in 1947. Allison was by no means a mean person, but nevertheless we read in his book (p.559):

> '*Apparently in India he [Howard] was too isolated from the laboratory and from the tremendous scientific advances being made elsewhere during his lifetime to appreciate that most of his ideas were out of step with reality even before he expressed them*'.

And he quoted Howard's warning '*The restoration and maintenance of soil fertility has become a universal problem*' only to prove how odd Howard's ideas had become…

King and Howard had worked in close interaction with (small) farmers in Asia and elsewhere and knew their practices. This coiuld not be said of Allison and Bremner. What, then, prevented those able men from seeing that they lacked decisive information, both pertaining to the time-honored practices of farmers, and to organic soil-plant interactions? If it was not their intelligence or personal shortcomings, we apparently have to turn to cultural and institutional factors, quite likely subsumed under the term 'technocracy'. They were convinced that the technocratic S & T that had experienced such an accelerated growth was the opportunity for people everywhere to enter the realm of plenty, and especially an era without hunger. A science that worked with the industrial means was definitely the most responsible choice. Any other choice would divert from the path of progress. Technocratic science was for them not an effort to seize power, but a true service to mankind. In their opinion any other approach would be less effective and consequently would leave the population hungry.

Note that what we what we are dealing with here is an ideology. Some of the best informed people of the age had stressed the need to start with the rural people themselves if we want them to 'progress', and not with our presumed advanced agricultural S & T. Succinct statements of that need are Boeke's rectorial address (Un. of Leiden, 1952) '*Dorpsherstel*' as well as Schumacher's '*A humanistic guide to foreign aid*' (1961; also in Novack & Lekachman (eds) 1964). Apparently they were preaching to the deaf.

But technocratic science is reductionist in principle and in practice. It needs to abstract both from an active soil and biota, and from irreplaceable local experience, if it wants to arrive at 'valid' prescriptions. For a science that cannot abstract from soils, plants and farmers, only close cooperation with those 'locals' can do, and that without the certainty of success… Experts and governments in post-war decades were sure that they had something far more powerful to offer. High Modernity was dominating their minds and research. For them as for most people in the West, it was not an ideology, but rationality itself. These were the years in which a volume could appear with the title '*Ratio as the rule of conduct*', and nearly everybody would applaud. It took decades for this 'dream of the age' to loosen its grip on the minds of so many able people.

In what follows I intend first to show that the technocratic view of science was not '*sachlich*' (sensu Hengstenberg), not '*sachgemäß*'. The fields of research of both the earth sciences and the 'sciences of matter' require a mix of a 'lawlike' approach with the proper recognition of the essential contributions of 'contingencies'. Technocratic science is not for real…

Once we know that much, it is also easier to take a fresh look at the results of post-war technocratic S & T. We will see that the idealism of its researchers was no guarantee for lasting results. Quite the contrary, our use of mineral-N fertilizer has led us to grave problems that are insolvable within the fertilizer paradigm. There is indeed a profound element of tragedy in the life of experts like Chhonkar…

But note that the choice for a technocratic approach was just a choice, not some *fatum*. We are free to re-evaluate our technocratic epoch, and to choose research approaches that are true to the life of people and plants. But will we have the courage to closely scrutinize our results – that by now led to all kinds of official standards and regulations, and have established impressive bureacracies and multinational food- and seed-empires?

8.2. Generalisation and contingency I: Earth sciences

> ‘*Explanatory constructs should be formulated not with the notion that a single explanation is likely to be applicable to most soils, but with the idea that multiple causality and polygenesis are likely, and that location-specific characteristics cannot be ignored*’ (J.D.Phillips, ‘*Contingency and generalisation in pedology*’, Geoderma 2001)

> ‘*… one of the principal challenges for physical geography – and indeed all research that attempts to address the earth on its own terms, as opposed to a simplified laboratory or simulation model setting – is to integrate approaches based primarily on generally applicable laws with those based on local, historical and spatial contingent influences*’ (id., ‘*Doing justice to the law*’, AnnalsAmer.Assoc.Geogr. 2004)

Note that Phillips does not just subscribe to ‘Pragmatic Holism’, which according to Edmunds (1996) is the working philosophy of most researchers. Instead, he stresses that ‘laws’ and ‘historical contingency’ are both always present. Sciences studying complex, open systems cannot do without the specifics of local history. In e.g. geomorphology that is easily proved: biological energy is of paramount importance in the transformations of the earth’s surface (Phillips 2009). That is a first reason why local soils always have a ‘pedological memory’ (Phillips & Martin 2004).
There is no contrast here with e.g. physics or chemistry. It is even a fact that it was especially (bio)physicists who paid attention to chaos and complexity in the past decades. What is even more important is that most physicists and chemists are very much aware that they need very specific circumstances if they want their ‘systems’ to work.

> Organic chemistry is greatly dependent on its ‘Beilstein’ and ‘Houben-Weyl’, industrial chemistry on its ‘Ullmann’, inorganic chemistry on its ‘Gmelin’, etc. etc. For an outsider those serials abound with confusing historical detail, yet, for the insider they are the very foundation of his trade.

In the construction of their instruments and in their preparative methods we see ‘*local history condensed*’: early on in their scientific training researchers learn to pay close attention to specific details in the (historical) designs, as they relate to their own specific historical circumstances (to which they will have to adapt them). From preparative and apparatus construction manuals to serial publications in the frontiers of science (Adv. in Chromatography, Methods in Enzymology), the sciences abound with essential details which prove their ‘*foundation in local history*’. Conversely, where attention to historical details is allowed to lapse, sciences are heading towards disintegration, even though they may seem to flourish institutionally.

When scientists and/or their customers (e.g. in government or industry) forget about those *fundamental historical details*, they unjustly claim that their carefully isolated systems can be used straight away as 'models' for reality-out there. Because this clashes with the very found-ations of their own trade, Pantin in 1965 stressed the difference between the 'restricted' and the 'unrestricted' sciences, roughly physics/chemistry vs biology, geology and the rest. By then Polanyi in his '*Personal knowledge*' (1957) had furnished a wealth of examples of the essentially 'traditional' character of the sciences which are rooted in history.

When Phillips showed that we need both 'laws' and 'historical contingency' in scientific explanation, this point itself was hardly new. Kohnstamm as a physicist-philosopher had given a clear exposition, some three quarters of a century earlier (Kohnstamm 1927 Bk.III Ch.II), in his '*The role of causality in the open reality*'. In it he explained also:

> '*But does not the concept of 'open reality', even if it does not abolish the possibility of experience, nevertheless abolish the possibility of science? The answer to that quest-ion depends, for sure, on one's concept of 'science'. If one means with that a perform-ed, cut-and-dried conceptual system to which reality has to conform, than the answer for sure has to be in the affirmative. But to me that then seems anything but the essent-ials of what we are used to understand as 'science'. In any case, real-life science, the science of the big and small researchers of nature, has never put itself on this haughty standpoint. They always intended not to* ***prescribe***, *but to probe and to listen'.*

Here we touch upon a decisive question: when and where did 'science' experience this meta-morphosis in which it changed from 'explorative' to 'prescriptive'? For sure, 'prescription' that surpasses a 'take it or leave it' stance implies some appropriation of power. So when and where did 'science' become thus prescriptive, that is, politicized? The chief moment was, for sure, when it became part of technocracy, with its design and direction from the centres of (governmental/industrial) power. The relations between science and government do have a long history, with both positive and negative aspects. But as to technocracy, as a specific devel-opment of bureaucracy, we are quite sure that World War II initiated something like a water-shed. Since then technocratic 'science' got firmly institutionalized, for sure, but only by allowing government/industry to determine much (if not most) of its goals and methods. **Henceforth policy directions took the place of true tradition with its lively discourse, and in disciplines like (mainline) economics and agronomics the refusal to dialogue with anybody unwilling to accept the conditions of technocracy, became one of the most distinct characteristics of post-war developments.**

For some time now we have been experiencing the end of this epoch of technocracy. Because it swamped our countries with regulations and standardizations and initiated and consolidated true 'empires', there is no easy transition to a non-technocratic epoch where locality and history – 'people and plants' – are once more acknowledged as essential to real life. Yet, within diverse scientific disciplines discourse opened up about real-life phenomena (e.g. rhizo-sphere processes). Conversely, their post-war closure was not just a power issue, but part of conversion to an ideology which the experts shared with major parts of the population. What with the benefit of hindsight is a disturbing determination to forgo discussion with those not adhering to the ideology used to be regarded as a recommendable decision not to lose precious time in discourse with people who were evidently a hindrance to progress.

> Think in this context of the marginalization of: K.Polanyi and Schumacher in economics; Virtanen and Howard in agronomics; Boeke, Timmer, Vink (cp. his thesis 1941) and van der Stok (Vink's promotor, prof. of tropical agriculture in Wageningen) in the combined field.

What remains to be done, now we are in the process of re-establishing the role of history in our diverse disciplines, is of course trying to find scape-goats, but to eliminate the consequences of more than half a century of adverse direction - and bring about improvements.

> The adherence to an ideology is a human phenomenon, and our post-war epoch of High Modernity served itself ill when declaring itself superior to all (other) ideologies. With its rationality essentially of the functional type (*sensu* Mannheim), it was systematically divorcing itself from its proper contexts, and so its faithful researchers were bewildered and without resources when their results proved not viable. Their chief problem derived from that break with contexts - even when those contexts were part-and-parcel of their field of investigation. Conversely, acknowledging such contexts can enable us to trace what went wrong. But note that this implies acknowledging the value of traditional farmers' knowledge and practices. That is, it implies opening up our safe disciplines and careers to a great crowd of local experts that we could formerly conveniently ignore…

8.3. Generalisation and contingency II: Sciences of matter

Physics and chemistry, though completely dependent on the transmission of their 'condensed histories', have been summoned by bureaucrats/technocrats to serve their cause. How, then, did they manage to eliminate the constitutive histories from those sciences? Quite central to their approach was a *presentation* of e.g. chemistry stripped of the 'details' of e.g. chemical synthesis and analysis. The resulting *image* of chemistry is no longer the messy thing that we see in the laboratory and industrial operation. To the contrary, its clarity makes it fit to serve technocracy's purpose.

But there is a problem: chemistry as a discipline is still completely dependent upon the 'judicious use of history', both in the preparatory and in the executive stages of e.g. synthesis and analysis. The chemist who forgets about the matrix dependence of analyses, and the batch dependence of syntheses, does not do justice to his trade. Quality control is an ongoing process in chemistry, and it needs people who are versatile in the 'condensed history' of their trade. So all technocracy could do is to strive to confine those 'messy aspects' to some laboratories under its control and to introduce another, 'clean' type of chemistry, fit for its *claire et distincte* nature and society. For many purposes a 'clean image' sufficed: after all technocracy is first of all an ideology. For the construction and transmission of that image technocracy could depend on several of its institutes.

Still, for technocracy to appear legitimate, also the world out-there had to be persuaded to accept technocracy's version of the sciences. And so scientists laboured, from conviction, to create models that presented just such a 'clean' image, models that promised progress in manipulating the world out there. Only afterwards did those models prove spurious (examples in Ch.1) and the sciences 'made to order' *GIGO*-sciences (id.). To complicate the matter, as a rule the science made to order was composed with experimental physics and chemistry that seemed without reproach. Researchers were convinced to do good science…

An example is the effort to reduce the physico-chemical complexity of soils to a mixture of pure solid phases in equilibrium with the soil solution. It encompassed the preparation and physico-chemical study of systems of pure components - e.g. pure goethite in a model system for soil iron(hydr)oxides. The PhD-students who did the actual laboratory work expressed their amazement that even after its preparation the goethite obtained had do be 'healed' carefully to get rid of ‘imperfections’, and compared this with the far more messy soil situation. They were still encouraged to continue in the same way, because only a physico-chemical ‘clean’ system would allow the application of the methods that would disclose the secrets of the soil system.

Highly qualified and sincere people worked hard, and for a time that was quite enough to convince nearly everybody that some valuable results were imminent. Yet, this idealistic and high-quality effort was, from its inception, fundamentally inconsistent with chemistry as a historical discipline. Note e.g. that the preparation and description of metal-ligand complexes had come of age already around 1900, with further developments in later decades. The potential for a near-infinite number of more hierarchical complexes, with coordination spheres of a higher order, was apparent from that time on. Also the existence of a near-infinite number of ‘imperfect’ specimens of e.g. goethite was apparent a very long time, both from geology and from preparative chemistry. There was no reason to think that a system consisting of such higher-order complexes and ‘imperfect’ minerals would be conducive to some reductive approach that, ultimately, would allow one to subsume ‘details’ under a generalized model and next limit oneself to mineral solution concentration.

> Quite to the contrary, the awareness of the near-infinite number of possibilities led to active research in ‘modular chemistry’, with e.g. transition metal complexes and/or clusters and/or polyoxometallates as building blocks. A short introduction to this ‘supramolecular chemistry’ with its variety of issues is Tsoucaris & Hasek 1999, an encyclopdic collection is Atwood & Steeds (eds) 2004. Some examples from this diverse field: Gallezot 1988, Mingos & Wales 1990, Livage, Sanchez & Toledano 1992, Reynolds (ed) 1993, Balzani et al. 1994, Braga & Grepioni 1994, Schmid (ed) 1994, Coronado & Gómez-García 1995, Eddaoudi et al. 2001. Some recent reviews: Ward 2003, Saalfrank et al 2008, Toma & Araki 2009.

Still, in the past most geochemists focussing at e.g. ancient mineralizing events, that presumably had occurred under high pressure and/or high(er) temperature conditions, had limited themselves to the ‘equilibrium’ approach indicated. The scientists developing techno-cracy’s model systems found not only some of those ready-made models to start with, but geochemists themselves had difficulty in changing over from the historical record and its more extreme conditions to models fit for questions about toxicity, bioavailability and contaminant migration under mild conditions and short timescales (Casey & Swaddle 2003). So for agronomy-related researchers to experiment with equilibrium models was quite normal.

Not normal was that they did not receive deviant signals coming from bordering disciplines. Consider soil iron(hydr)oxides. Schwertmann from his long-standing work wrote in regard to soil goethites (Schwertmann 1990 p.74; cp. also Vempati & Loeppert 1989):

> ‘*A survey of several hundred samples of soil goethites reflects the pedoenvironment. Since goethites in soils form from solution, the activity of Al in solution could be expected to govern the extent of the Al substitution. The activity is determined by parameters such as the Si activity and pH. This explains most of the observations made on Al substituted goethites in soils. Attempts have also been made to apply thermodynamic data for mixed Al-Fe systems in order to explain the extents of substitutions, but kinetic factors would appear to be equally important*’.

Once more we see: **notwithstanding the partial relevance of general ‘laws’, there is no escape from local, historical factors in soils**.

That is the reason that Schwertmann c.s. were careful to make very long-term experiments in soil (clay) chemistry, cp. ‘*The effect of clay minerals on the formation of goethite and* *hematite from ferrihydrite after 16 years' ageing at 25 ^{0}C and pH 4-7*' (Schwertmann et al. 2000). Even at the level of soil iron(hydr)oxides there is no substitute for long-term, historical experiments. There is no exaggeration when Schwertmann (1990 p.81) emphasizes: '*Work with soil iron oxides, rather than with pure synthetic materials should be highly encouraged*'.

> Thanks to his meticulous work and that of others there is ample information on the hierarchical complexity of iron(hydr)oxides and on the decisive importance of the historical circumstances of formation and ageing. E.g. Schwertmann et al. 2000 is no. 19 in a long series of investigations of '*The effect of Al on Fe oxides*' that started in the 1970s and that gave us a.o. beautiful electron microscopic pictures of real-life, complex goethites etc. (see also Rösler, Hofmeister & Held 1992, Janney, Cowly & Buseck 2000).

Still, mainline agronomic (and agronomy-related) research stayed aloof of the work of Schwertmann c.s., as they had done with the labours of Foster c.s. on electron microscopy of soil microbiota. The soil world that they had projected, for decades, from their technocratic standpoint did not conform at all with the real-life soil depicted by Schwertmann and Foster. Yet, it is their technocratic picture of the soil that is at the basis of agricultural policy (and policy-related models) and extension to this very day.

8.4. '*The scientific base … is seriously flawed*'

For technocracy to rule requires not so much the *elimination* of non-technocratic S & T as well as its limitation to prescribed *reservations*. Post-war technocratic S & T depended for its myth of 'scientific progress' on such reservations of 'free enquiry', while its rule depended on keeping them carefully enclosed. In the field of the soil sciences in the Netherlands we have in the decades after the war some distinct examples of 'free enquiry', e.g. Jongerius 1957 and Bal 1971, and more broadly a trickle of academic research in soil biology, micro-morphology and clay mineralogy, but they never succeeded in leaving their reservations. Apparently that is because they were allowed some limited finances for 'fundamental research', but the big finances and specific institutions would come only when they would arrive at 'generalizable' results. With the rule of technocracy in agriculture dependent on a 'modern' picture of soil and agriculture in which the specifics of time and place – history and locality – have yielded to centralized research and industrial inputs, its well-financed institutes were specifically aiming at such a de-localized and de-historized agriculture.

> The technocrats were sure about their cause. Remember Nehru expressing his convictions to Chu en Lai, while visiting a hydropower project: '*These are the temples where I worship*'. There were dissidents, but they could be tolerated, as long as they would not come in the way of technocratic progress. Still, a great crowd of little people experienced that they were considered to be in the way, when they got displaced for the construction of modern towns, big dams or rationalized landscapes. But some of them could be tolerated, either as museum pieces (also in connection with nature reserves) or as academic curiosities.

As a matter of fact, our technocrats were even after W.W.II still faced with a diversity of farmers and farming styles, most of them thoroughly anchored in the local community and agro-ecology. But they managed to 'think them away' when they stressed – without proof - that as to crop yields traditional agricultures had hit the ceiling (Schultz a.o.). Henceforth only 'science-based' agriculture using industrial inputs would be able to break through this ceiling. Note that the 'science' of this 'science-based' agriculture was seriously flawed: the contradictions of industrial fertilizer-based agriculture became widely known after W.W.I already (cp. Uekötter 200.). But to our technocrats the supremacy of industrial fertilizers was perfectly clear. After all, a simple calculation taught them that a bag of industrial 'fertilizer' equalled a wagonload of its organic 'equivalents'. This simple calculation, as presented by the fertilizer industry, was begging the question of true soil fertility. (It was conveniently ignored).

> Note that researchers from the end of the 19th century on had looked into the question of (specific) organics uptake by plants, e.g. E.Hamilton Acton with his '*The assimilation of carbon by green plants from certain organic compounds*' (Proc.R.Soc.London 47(1889)150-175). The French plant physiologist Molliard even became renown for such researches. His four-volume '*Nutrition de la plante*' (1921-1925) was widely distributed in the Interbellum. The volumes 2 and 4 give information on organic nutrition of plants; his 1936 Œuvres Scientifiques gives an overview of publications in the field.
> But when the fertilizer/explosives industry, after its enormous growth as a result of W.W.I, got the chance to dwarf all other types of agricultural research (research not focussing at growing gifts of fertilizer), a very unsatisfactory situation developed. This was especially after the editors of leading German standard works started to align themselves with industry. Industrial subservience became semi-definitive when the four-volume '*Literatursammlung auf dem Gesammtgebiet der Agrikulturchemie*' (1931-1939) was marshalled as a 'definitive' bibliography of the field. Its very unsatisfactory subject division and partial coverage of the relevant scientific literature then helped technocratic research to withdraw to its 'NPK-cage'.

As indicated in Ch.4, very recently history broke into this industrial fertilizer cage, when Khan c.s. starting from their own research in the Morrow Plots, the oldest long-term agricultural project in the USA, summarized the results of all long-term agricultural projects in:

'***The myth of nitrogen fertilization for soil carbon sequestration***' (Kahn et al. 2007) and '***Synthetic nitrogen fertilizers deplete soil nitrogen: A global dilemma for sustainable cereal production***' (Mulvaney, Khan & Ellsworth 2009).

As they wrote in their second publication, '***The scientific base for input-intensive cereal production is seriously flawed***'. Because we lived from wearing out the soil organic matter capital, at the centre of the post-war showpiece (fertilizer-responsive crop varieties) there is no substance, but a hole that threatens to swallow sustainable food production.

> Reid's comment on the Khan et al 2007 publication received a rebuttal from the authors (Reid 2008 & Khan et al. 2008). Gardner & Drinkwater 2009 is another comprehensive survey; Russell et al. 2009 and Senthilkumar et al. 2009 corroborate the results; Lee at al. 2009 focus on the paddy system. Kimetu et al. 2009 show that after reaching some threshold, restoration of soil organic capital becomes more difficult. For an earlier meta analysis of carbon stocks and land use changes see Guo & Gifford 2002. Bellamy et al.'s overview (2005) of soil carbon losses from all soils across England and Wales 1978-2003 had sounded the alarm-bell already. A great increase in riverine organic carbon transport is part of the phenomenon, with high rates of nitrogen fertilization and N-deposition at large the most likely cause, Findlay 2005. 'Global warming' is consequence, not cause (cp. Davidson & Janssens 2006). For New Zealand: Schipper, Parfitt & Ross 2007. For Belgium: Goidts & Wesemael 2007.

Why did mainline researchers for decades follow the input-intensive treadmill? Quite likely, it was the construction of their own experiments that led them astray. ***First***: we now know that the history of amendment of a soil with manures and/or fertilizer is far more important for soil N dynamics than surmised before (Mallory & Griffin 2007). ***Second***: Satoshi et al. (2009) demonstrate that the uptake of treatment nitrogen from a soil is co-dependent on its pre-treatment nitrogen pools (quality and quantity). ***Third***: Gene expression is markedly different between wheat grown with organic or inorganic fertilizer (Lu et al. 2005; it is a difference that has obvious consequences also for breeding). In short:

***Soil N dynamics and plant N uptake are shaped by the local history of soils and plants. Pre-empting that history to some imagined mineral-N cycle is without scientific foundation*. *It is not allowed to abstract from the local history of a soil*.**

It is apparent for decades that we have to give full attention to organic N, instead of making it conceptually and methodically invisible. ***First***, Power et al. 1986 is one of many publications that demonstrate that total crop (corn in their research) uptake of N is dominated by indigenous soil N. ***Second***, Vanoti et al. in conformance with other authors demonstrate that (higher) mineral-N gifts give a surge in the 'active fraction' of soil organic nitrogen (esp. dissolved organic-N). This surge can explain the dominating influence of soil N on N-nutrition in spite of the mineral-N gifts, as well as the increasing loss of soil N with increase in fertilizer gifts. ***Third***, the so-called 'non-N effect' of a legume in a rotation on yield increase of the next crop is known a very long time (cp. Fu 2000) and requires concepts that are dimensionally richer than the mineral-N approaches allow. When modelling e.g. N availability from organic fertilizers we have to consider all the diverse aspects of local soil and management history, if we want to evade finding an inferiority of organic fertilizers that in fact is not there.

Note that mainline research started from an assumption of soil microbial life sticking to its assigned roles, as conceived by mainline research itself. But surely, microbes follow their own course and react differently to organic or industrial inputs. ***First***, there are clear differences between (wheat) straw decomposition in soils under conventional and organic farming management (Scheller & Joergensen 2008; conventionally managed soil gave evidence of microbial stress). ***Second****,* decomposition of soil organic matter by microbes depends strongly on their N-limitation, free mineral-N decreasing the decomposition (Craine, Morrow & Ferer 2007). Evidently it is important to be careful about unwanted interactions between mineral and organic fertilizer: it is experiment construction itself that can *cause* organic fertilizer to be 'unmanageable'. ***Third***, esp. N-heterocyclic compound decomposition occurs only in soils with low mineral-N concentration (Sims 2006). Increases in heterocyclic-N, esp. purine and pyrimidine bases, in industrial agriculture are evident esp. from N-XANES spectroscopy (Jokic et al. 2004, Leinweber et al. 2007; cp. Smernik & Baldock 2005 and Cortez & Schnitzer 1979). This is likely to contribute to loss of soil health and productivity in industrial agriculture.

We looked already at the N-nutrient paradigm shift. In the words of Schimel & Bennett (2004): '***N cycling is now seen as being driven by depolymerization of N-containing matter by microbial (incl. mycorrhizal) extracellular enzymes. This releases organic N-containing monomers that may be used by either plants or microbes***'. Waterworth & Bray's (2006) '*Enigma variations for peptides and their transporters in plants*' and Rentsch, Schmidt & Tegeder's (2007) '*Transporters for uptake and allocation of organic nitrogen compounds in plants*' attest to the wide perspectives that opened up to researchers.
What is more, research like that of Paungfoo-Longhienne et al. (2009a) '*Plants can use protein as a nitrogen source without assistance from other organisms*', and Adamczyk et al. (2008) '*Wheat seedlings secrete proteases from the roots and, after protein addition, grow well on medium without inorganic nitrogen*' and (2009) '*Degradation of proteins by enzymes exuded from Allium porrum roots: A potentially important strategy for acquiring organic nitrogen by plants*', shows that plants are not just waiting for microbes to deliver them the organic-N. Furthermore, Paungfoo-Lonhienne et al. (2009b) demonstrate that both root form (cluster root of pea, with increased soil exploration) and expression of peptide transporters depend on (quality and quantity of) the nitrogen present in the soil, with low concentration of mineral-N a prerequisite for good expression of both the absorptive root system and peptide transporters.

Instead of mainline research's inert soil- and plant-world that is waiting for industrial supplies, there is the real world of soils and plants that teems with its own activity. In that real-life soil it is perfectly understandable that Ramesh et al. (2009), in field experiments with organic-manure combinations, demonstrated grain yields on a par with mineral fertilizer-based yields (with HYVs), but now concurrent with the maintenance of soil health. In the variety of organic approaches studied, it is not some presumed capacity to deliver mineral-N, but the combination of improved soil quality and a steady supply of (especially) organic N that leads to high yields (cp. also van der Heijden et al. 2006). Yet, most of the former field experiments intending to compare industrial and organic crop growing started from the assumption that the plants profited only from mineral-N, so they focussed only at e.g. 'net mineralizable N'. We now realize that their results are confused, at best (cp. also Geisseler et al. 2009).

The present situation is one of great expectations, thanks to the wide perspectives that opened up. Really promising is biological nitrogen fixation in connection with the re-discovery of organic plant nutrition. Simon et al. (2003) found that '*high values of potential nitrogen fixation were found in the variants fertilized with farm yard manure … the variants fertilized with mineral NPK reached low values of potential nitrogen fixation*'. There is no exaggeration in the title (Dahora et al. (eds) 2008) '*Biological nitrogen fixation: toward poverty alleviation through sustainable agriculture*'. But results obtained within the NPK-cage of post-war decades need re-evaluation (even BNF research suffered from a very short time horizon).

> Under the experimental conditions that were then common, uptake hydrogenases (saving hydrogen for the plant) mostly proved non-functional, and the loss of H_2 was seen as part of the energy costliness of BNF. Yet, even as to intact Azotobacter chroococcum Postgate c.s. (Partridge et al. 1980) discovered that '*Hydrogenase activity developed nearly two generations later than nitrogenase activity during the transition from NH_4^+- to N_2-dependent growth in carbon-limited cultures*'. (For recent research in Azotobacter BNF in the rhizosphere cp. Kaur, Goyal & Kapoor 2003). In cell-free nitrogenase preparations the hydrogenase no doubt was part of the membrane-associated enzyme system removed in the 'purification' steps. (For a recent study refer to Martinho 1995).
> More generally, bacterial endophytes were suspected as (potential) pathogens, instead as the useful plant commensals that most of them apparently are (Rosenblueth & Martínez-Romero 2006). The research program of Johanna Döbereiner - the search for nitrogen fixing endophytes in e.g. grasses, cerals and other crop plants - was frowned upon by researchers in the 60s till 80s., yet, in the present we see e.g. research with Herbaspirillum sp. isolated from wild rice and probed for the diverse contributions to crop rice growth (cp. James et al. 2002, Zakria et al. 2007). Note Herbaspirillum is an endophyte from Döbereiner's oldest research object, sugar cane (e.g. da Silva et al. 2003). Note that with *Oryza longistaminata* BNF endophytes decreased significantly in richness even with low rates of (mineral) N input (Diallo, Reinhold-Hurek & Hurek 2008). With *Oryza sativa* Abeltagy & Audo (2008) found the many unkown BNF endophytes esp. in stems - one more reason to doubt dwarf varieties?

8.5. The world outside (the NPK cage)

With real-life complexity re-discovered, we have to admit that there is no exaggeration when Watt, Silk & Passioua (2006) write: '*The paucity of reliable data underlies the rudimentary state of our knowledge of root-organic interactions in the field*'. Likewise, Jones, Nguyen & Finlay (2009) expose the great uncertainties in our knowledge of '*carbon trading at the soil-root interface*'.

As to the N-cycle in soil, Frank & Groffman (2009) wrote '*Plant rhizosphere N processes: what we don't know and why we should care*' about which they explain:

> '*We review the tight interactions between roots and microbes and discuss why ignoring the significance of these interactions has led to unrepresentative estimates of N availability in intact plant communities and an incomplete understanding of the environmental factors that control plant-available N. We also explain why current standard methods to assess soil N availability do not account for important soil rhizosphere processes*'.

For half a century we pretended to know what happened 'out there', but in fact had no idea even of the effects of our big gifts of mineral-N fertilizer. Our passive view of roots and static concept of microbial activity made us project a world that never was (cp. Chapman et al. 2006). We entertained a closed worldview of a 'pathological' type (according to Kohnstamm 1926) that made us see 'definitive' methods and standards where none were to be had.
An example is the 'standardization' in agriculture-related research of the use of the Kjeldahl N-determination without the chemically obligate control for organic N-compounds that are outside its range. A direct result was that, with the exception of Russian researchers in 1960 (referred to earlier), researchers failed to spot the abiotic nitrite-production and consequent N-immobilization in soils with increasing mineral-N input.

> After earlier systematic studies of N-compounds not yielding to Kjeldahl determination, Dakin & Dudley 1914 gave a definitive account. Some plant physiologists heeded the warning (e.g. Christensen & Fulmer 1927), but especially after W.W.II the Kjeldahl was used as a 'standard method' in agronomics in a way that is illegitimate in chemistry. Fisch 1952 repeated the need for a reductive pre-treatment for most of such compounds, but plant physiologists and agronomists did not heed his warning.
> Yet, in fact the Kjeldahl was known to miss out on e.g. fixed ammonia too: in that case a HF-treatment is needed (Stewart & Porter 1963), a classic treatment in geochemistry. So when Bremner in 1960 stated that also clay-fixed ammonium is determinable by the Kjeldahl he missed the obvious. Note furthermore that Bremner did not endeavour to use the Dumas N-determination as a baseline – in spite of the fact that all truly systematic studies do exactly that. All in, there was ample reason for agricultural researchers in 1960 not to take Bremner's 1960 publication as 'definitive', yet, they did. Note that at least some attentive researchers spotted the difficulties in the next decades, e.g. Vandenabeele et al. 1990.

Abiotic nitr(os)ation of organic matter – especially dissolved organic matter – is fast compared with microbial processes, disproving once more the central role for specific soil microbes in soil N (im)mobilization that is the axis of mainline research's experiment construction and modelling. A quote from Gessner 2005:

> '*Observations at Harvard Forest and other sites suggest that abiotic immobilization of N may be most responsible for the unexpected large rates of soil N retention (over 70% of total inputs), challenging a widely held view that microbial processes are the dominant pathways for N immobilization in soil*'.

It is only with Dail et al.'s (2001) '*Rapid abiotic transformation of nitrate in an acid forest soil*' and Davidson et al.'s (2003) '*A mechanism of abiotic immobilization in forest eco-systems: The ferrous wheel hypothesis*' that this major part of the N-cycle in systems with external inputs of mineral-N received recognition (cp. also McKnight and Cory 2004).

From the growing awareness of nitr(os)ation in soils, El Azhar et al. (1986a & b) started a close chemical investigation (using humics fractions, IR-spectroscopy and HR-chromatography) and found a.o. formation of nitro- and nitroso-groups. Glchner et al. (1989) looked specif-ically to N-methyl-N-nitroruea formation in soils; their research had a follow up in Polish rsearch into nitrosamine formation in grassland soils. Next Thorn & Mikita found a wide range of substituent groups in soil organic matter, with N-nitroso groups definitely among them (Thorn & Mikita 2000). Applying analytical methods that had been available for nearly half a century, Mikita et al. next extended their researches to the products in plants, and once more found a sizeable contribution of nitr(os)ation products (Mikita et al. 2004 & 2005). This is in accordance with the comparatively easy chemical synthesis, under mild circumstances, of a diversity of N-nitroso compounds. Given the known carcinogenic character of most of such compounds, it is quite astonishing that e.g. governments did not respond, up till now. (There is some comfort, at least, from the fact that within chemical research nitrosation reactions are in focus for many years - Williams 2004 is a book format account). Worse still, N-nitrosamines are only part of the nitrate/nitrite carcinogenesis problem.

Research in **nitrosative carcinogenesis** has uncovered some direct relations, as is evident from e.g. Yang, Tabaoda & Laiao's (2009) '*Induced nitric oxide synthase as a major player in the oncogenic transformation of inflamed tissue*'. Helicobacter hepaticus' role in carcinogenesis is an indirect one, but NO is among the real triggers (Erdman et al. 2009). Research elucidated mechanistic details, e.g. Ili et al. 2009 on epigenetic mechanisms and Cantoni & Guidarelli 2008 on DNA strand scission. The detail attained is apparent from Tomko, Azang-Njaah & Lazo 2009 who write '*We speculate that failure to activate the S-phase checkpoint in precancerous cells undergoing nitrosative stress may elevate the risk of transmitting damaged genomes to daughter cells upon cell cycle reentry*'. But note that important direct chemical mechanisms are known since the early 1960s, and still are the subject of high-level research (e.g. Labet et al. 2009).
In Ch.5 we looked already at oesophagical cancer, on which see also McColl 2005, Ara et al. 2008. Takahama et al. 2008 focus on saliva, Ishiyama et al. 2009 is a related publication.

Plants apparently carry pathogen-inducible NO synthase(s) functional in defense responses to pathogens with the help of toxic NO-pulses (Chandok et al. 2003, Floryszak-Wieczorek et al. 2007; but see E.Gas et al. 2009). Yet, some bacteria use an own NO synthase to effect resistance to a broad spectrum of antibiotics, enabling them to survive and share habitats with antibiotics-producing microorganisms (Gusarov et al. 2009). Furthermore, certain Streptomyces spp. use such a NO synthase also in biosynthetic nitration to produce plant toxins (Crane 2008). So there are strong reasons to strengthen plants in their use of NO in defense, and to be very careful at the same time to refrain from strengthening antibiotic resistance and plant toxicity of microorganisms. It is quite likely that our increasing gifts of mineral-N disrupted the delicate balance that is needed for a functioning system of plants and (soil) microbes.

In the animal and human organism the pathogen defense (by macrophages) with the help of NO-pulses is well studied (e.g. Vazguez-Torres et al. 2008, Pekarova et al. 2009). There is evidently a delicate balance that can be disrupted by nitrate in food and feed. Vermeiren et al (2009) report on intestinal NO production by micro-biota carrying out dissimilatory nitrate reduction. It can conceivably induce virulent *Staphylococcus aureus* (cp. Richardson, Libby & Fang 2008). Relations are complex (Enkhbataar, Traber & Traber 2008 give a very good medical discussion), yet, the threat of selection of resistant pathogens is evident.

Yet, recently an offensive was started to have nitrate and nitrite removed from the list of (food) pollutants and have them accepted as nutrients (!) instead – cp. Hord, Tang & Bryan's (2009 May 13) publication in the Am.J. Clin.Nutrition. They first construct a hypothetical high-nitrate anti-hypertension diet and next conclude that daily intakes that are multiples of the WHO's ADI's are 'healthy'... Of similar focus and quality is Calvert & Lefer 2009, as well as Stokes et al. 2009 who apparently use a deficient mouse model.

Note that those authors give us no chemical and enzymological specifics (but Bryan 2009 admits ‘cell and tissue dependence’), while Wolin 2009 and Török 2009, who choose the same subject of vascular function and hypertension, give specifics use-ful to ponder the dangers of seizable nitrate/nitrite concentrations.
Lee et al. 2009 specifies (nitrite-induced) protein tyrosine nitration in a.o. atherosclerosis, while Rodrígues-Mañas et al. 2009 stress that it is especially (nitr)oxidative stress that causes endothelial disfunction, in aged microvessels first of all. Rouhanizadeh et al. 2008 and Namkoong et al. 2008 give an exposition of some of the advanced methods that are required in this field of research.

Anybody consulting e.g. Pácher, Beckman & Liandel (2007) extensive review ‘*Nitric oxide and peroxynitrite in health and disease*’ will be aware that dangers of (nitrate/nitrite leading to) NO stem first of all from its diffusion-controlled reaction with superoxide to peroxynitrite. At NO concentrations consistently higher than physiological, peroxynitrite (in the words of Poderoso 2009) ‘*will lead to nitrosation/nitration and oxidation of mitochondrial and cell proteins and lipids. The disruption of NO modulation of mitochondrial respiration supports, then, a platform for prevalent neurodenegenative and metabolic diseases*’. The physiological NO concentrations are (Hall & Garthwaite 2009) ‘*100 pM (or below) up to approximately 5 nM, orders of magnitude lower than was once thought*’. That is, it is easily conceivable that nitrate/nitrite intake in food leads to tissue concentrations that are disruptive.

Note that careful chemical research substantiates the focus at peroxynitrite - Ferrer-Sueta & Radi 2006, Bartesaghi et al. 2008, Goldstein & Merényi 2008, Galliker et al. 2009. Research that substantiates the low physiological NO concentrations is e.g. Pellegrino et al. 2009 who point to a 1 nM nitrite concentration modulating cardiac contractility in an animal model.

Among the risks those of a neurological character take a first place. One of those is easy to conceive: Salerno & Ghosh (2009) inform us that neuronal NO synthase generates signal pulses, so that ‘bathing’ in (nitrite-derived) NO is disruptive indeed. There is plenty of reason for Kanwar et al. (2009) to warn that ‘*The high sensitivity of neurons to NO is partly due to NO causing inhibition of respiration, rapid glutamate release from both astrocytes and neurons, and subsequent excitoxic death of the neuron*’.
The disruptions are multifaceted because they concern especially mitochondria. At a bio-chemical level there is the inhibition of terminal oxidases of the respiratory chain (Mason et al. 2008). At the genetic/cell cycle level there are epigenetic mechanisms (Foster et al. 2009) leading e.g. to disruptions of developing neurons in Parkinson’s disease (Nott & Riccio 2009), as well as posttranslational modification by (especially) peroxynitrite leading to neurodegen-erative diseases (Lee et al. 2009). At the mitochondrial level there is the ‘*Impaired balance of mitochondrial fission and fusion*’ (Wang et al. 2009) in e.g. Alzheimer’s disease, where (Cho et al. 2009) NO links ß-amyloid with mitochondrial fission and neuronal death. So it is hardly unexpected that recently a strong link was found between nitrate in food and the extreme increases in Alzheimer (de la Monte & Tong 2009).

The literature covers many aspects of the complex subject. Cp. for protein misfolding and NO in neurodegenerative disease Nakamura & Lipton 2009, for 3-nitrotyrosine modified brain proteins in neuro-degener. disease Butterfield & Sultana 2008, Sultana & Butterfield 2008, for Parkinson’s disease and nitrosative stress Tsang & Chung 2009, for impairment of pro-survival proteins in Parkinson’s through abnormal S-nitrosylation Tsiang et al. 2009.

It is necessary to realize that research into the risks of nitrate/nitrite in feed and food requires a level that is simply nowhere in sight in proposals like that of Hord, Tang & Bryan 2009 (who want nitrate and nitrite re-defined as nutrients). The level that is required is clear from e.g. the Methods in Enzymology Volumes 436, 437, 440 and 441 (all 2008). These volumes encompass (self) critical research of a high level and open to informed discussion.

> Hughes's '*Chemistry of nitric oxide and related species*', Goldstein & Merényi's '*The chem.-istry of peroxynitrite: Implications for biological activity*' and Vicente et al.'s '*Biochemical, spectroscopic, and thermodynamic properties of flavodiiron proteins*' set the tone. Analytical contributions are of a similarly high level, e.g. Pouvreau et al. on Clark electrode measure-ments (of NO etc.).
> For advanced research in (generally disruptive) **protein nitr(os)ation** see: Bartesani et al., Salzano et al., Butt & Lo, Shao & Heinecke et al., Bigelow & Qian, Rabbani & Thornalley, Nuriel et al., Sharov et al., Rebrin et al., Bregere et al.

The carcinogenous consequences of nitrate/nitrite in food and feeds are apparent a long time, at least since Perutz (1962) publication on base-level transformations in DNA. Since then careful research corroborated this direct chemical information (e.g. Labet et al. 2009). New is only that biomedical NO-research since about 1990 has made us aware that also the disrupt-ion of physiological mechanisms of the organism itself can lead to cancers (cp. Yang et al. 2009: '*Induced nitric oxide synthase as a major player in the oncogenic transformation of inflamed tissue*').
On the other hand, quite unexpected was the discovery of connections between the rise of (antibiotics) resistant bacteria and reactive nitrogen species. Nevertheless, the subject is well researched by now (cp. e.g. Crane 2008 '*The enzymology of nitric oxide in bacterial pathogenesis and resistance*'). There are many sides to the subject, and that in itself gives us some certainty that we indeed hit a chief problematic consequence of nitrate/nitrite in feeds & foods.

> Gusarov et al. 2009 show that endogenous NO plays a role in-vivo in protecting bacteria against antibiotics that, of course, are products of soil micro-organisms. Microphages use non-physiological NO-pulses to kill pathogens (Vazquez-Torres et al. 2008), but then it is crucial not to enhance NO-detoxification mechanisms in bacteria, and apparently we did just that with the nitrate/nitrite load of feeds and foods (Kuwahara et al. 2009 on *Helicobacter pilori*; cp. also induction of nitrite transporter in pathogenic *Salmonella*, for which see Das, Lahiri & Chakravortty 2009). Note that a first consequence of rising NO concentrations that are not yet directly suppressive is induction of (self-protective) biofilm formation by patho-genic bacteria (Zaitseva et al. 2009). Further likely consequences are induction and genetic enhancement of the mechanisms with which pathogens counteract the reactive nitrogen species, like enhancement of base excision repair (Richardson et al. 2009). The volume of research on NO/reactive nitrogen species and (the rise of) pathogenic bacteria is impressive by now. Examples: Thomson, Stevanin & Moir 2008 on *Neisseria meningitides,* Nobre, Goncalves & Saraiva 2008 on *Staphylococcus aureus*, Pickford et al. 2008 on *Campylobacter jejuni*, Ascenzi & Visca 2008 on *Mycobacterium tuberculosis* and *M.leprae*.

As to plants, 'NO'-research is actively pursued, but with far more modest financial means than those available to the biomedical research circuit. Still there are some specific results. E.g., the growth of cell-walls and tissues is 'directed' by very low concentrations of NO, and can easily be disrupted by higher concentrations (e.g. Correa-Aragunde et al. 2008). Likewise, seed dormancy can be broken by NO (Liu et al. 2009), and on-ear sprouting of cereals fertilized with mineral-N can be an unwanted result.

> As to pathogen resistance of plants, Chaki et al. (2009) found about sunflower-mildew interactions that in contrast to resistant plants, susceptible cultivars show a rise in protein tyrosine nitration and in S-nitrosation that does not derive from endogenous sources.

At present there is some unceratainty in regard to NO-syntases in plants, after a surmised Arabidopsis NO-synthase proved to have another function – cp. Crawford 2006, Moreau et al. 2008, Gas et al. 2009.
But note that, thanks to advanced research, there is no doubt about the presence and importance of NO and reactive nitrogen species in plants – e.g. Hebelstrup et al. 2008, Vandelle & Delledonne 2008, Sandalio et al. 2008, Igamberdiev & Hill 2008.

All in all, after a long period in which it was assumed that mineral-N could do no harm because it was emphatically stressed that it was the only ‘natural N-nutrient for plants’, we are now faced with a true avalanche of potentially vicious consequences of our fertilizer applic-ations. Much of it could have been apparent in the 1960s already, if we had only (1) shown respect for the expertise of the ‘farmers of forty centuries’ and (2) used the analytical methods that were available by then. This testifies to the **petrification** (*sensu* Mannheim) of mainline agricultural R & D that is incumbent on its policy-related character. We chose to ignore the knowledge and experience of an immense number of able agrarians, and then did not pay attention to those scientific developments that could have shown us what our ‘fertilizer-progress’ did to soils and plants.

Yet, one thing is certain: mainline agricultural researchers of those years were humans too. As indicated before (the example of Ghhonkar and Allison), we will have to look first of all at the broader socio-cultural setting of the post-war decades if we want to understand the *tunnel vision* of agricultural research in those years.

8.6. Agricultural research on Goli Otok

(On Goli Otok the ‘Denuded Island’ Tito kept his political prisoners)

As indicated before, post-war years were not conducive to a balanced approach to (e.g.) agricultural research. The focus on industrial fertilizer was directly related to the recent war and not to a long-term view of agriculture. Still, the question is why researchers, after an early post-war focus on fertilizers, abandoned ‘organic agriculture’ completely in spite of the fact that (1) it had been the core element of sustainable agriculture up till then (2) there were many exciting developments in bio-analytical chemistry that gave them the opportunity to take a fresh look at the organics of ‘organic agriculture’.

Part of the answer is the institutional specialization of the accelerating agricultural research in post-war decades. Researchers were united in the great expectations of specialized research that was central to the ideology of those years, but it was this far-reaching specialization that effectively hindered an integrative view. In spite of the true enthusiasm, research was consistently directed from the policy centres. There was discussion, but only within the specialist institutes and within the parameters set by government/industry policy.

The question presents itself why most of the researchers within agricultural research institutes developed a kind of ‘tunnel vision’. First of all, quite a few were simply swept along by the infectious optimism and enthusiasm of the leading technocrats. Others were inclined to ‘go with the flow’: it requires a bit of courage and character to go against the general trend. And then, one needs an ‘open mindset’ to look at developments outside the ruling paradigm. The following quote from Heschel (1962 p.xi/xii) gives a feel for the problems that we all have in ‘seeing’ something new even if it is right under our nose:

> '*What impairs our sight are habits of seeing as well as the mental concomitants of seeing. Our sight is suffused with knowing, instead of feeling painfully the lack of knowing what we see. The principle to be kept in mind is to know what we see rather than to see what we know.*
> *Rather than blame things for being obscure,we should blame ourselves for being biased and prisoners of self-induced repetitiveness. One must forget many clichés in order to behold a single image. Insight is the beginning of perceptions to come rather than the extension of perceptions gone by. Conventional seeing, operating as it does with patterns and coherences, is a way of seeing the present in the past tense. Insight is an attempt to think in the present.*
> *Insight is a breakthrough, requiring much intellectual dismantling and dislocation. It begins with a mental interim, with the cultivation of a feeling for the unfamiliar, unparalleled, incredible. It is being involved with a phenomenon, being intimately engaged to it, courting it, as it were, that after much perplexity and embarrassment we come upon* ***insight*** *– upon a way of seeing the phenomenon from within. Insight is accompanied by a sense of surprise. What has been closed is suddenly disclosed. It entails genuine perception, seeing anew*'.

Also in post-war decades there were researchers able 'to see anew'. A famous example is Barbara McClintock's unbiased approach of corn plants and their genetics. It is significant that it took decades before her contributions received recognition - though they deserved it already in 1950. In plant breeding and genetics, as in agricultural research, those 50s, 60s and 70s were not conducive to 'seeing anew', especially not when for a time reductionist science (à la Watson, Crick & Monod) caught nearly everybody's fancy. For reductionist science accepts only what 'fits' within its own framework and loathes the 'intellectual dismantling and dislocation' that is the prerequisite of 'seeing anew'.

Now note that this rigid adherence to its own 'established' framework is part-and-parcel of its claim to have 'no limits': 'science' is supposed to go from victory to victory, instead of being puzzled by new phenomena. There is therefore a curious correspondence between the closed character of reductionist 'science' and its refusal to accept limits.
What is more, it is immediately apparent that people can make such a claim, and can even make it part of the official doctrine, but that it is all to no avail as soon as they leave their jurisdiction and are rather powerless - e.g. within soil and soil life. The only way left to convince themselves and their fellow men is condensed in the following **proposition**:

> ***'Limitless science' is the product of a science policy that refuses to accept the limits imposed by life itself***.

In a way, that is easiest when dealing with human beings. Technocracy was greatly strengthened by the war and the local farmer faced policies, laws and regulations issued by the omnipotent centre, which had dismantled local authority.
Still, landscapes are diverse, soils are hierarchic and heterogeneous, and plants are individual and explorative. Up to a point, landscapes can be uniformitized by stripping them of diversity and heterogeneity (re-allocation works). Plants can be made unifirm by e.g. pure-line and hybrids breeding, but that does not take away their individuality and active interactions with their local environment. Soils cannot really be made uniform, but for a time soaking them in an industrial fertilizer solution can make for a substitute.

The point is, of course, that this ‘powerful’ drive to achieve uniformity is misguided. For crops keep being dependent on the local ecology (even if it is impoverished), and they are dependent on organismic activities in the local soil, and on plants acting ‘intelligently’ (Trewavas) on the spot. Imposing a model (and regulations) that denies those complex interactions does not change the fact that they are fundamental to agriculture and food production.
But - it ‘helps’ to lose sight of the farmer, who is locally active in guiding those decisive interactions. For the technocrats wanted ‘science’ to be limitless, but there was this limit of flesh and blood, the farmer. Of course, just denying political power to the farmer was hardly new, history tells us one long story of that denial. New was the denial of his ingenuity and expertise, declaring it inferior to the technocrats’ ‘scientific’ approach.

The next step was denying the potential of the farmer’s local assets – plant, soil, and ecology. This was a remarkable step indeed, because the versatility of those assets does not depend on consent of the government and its experts. All they could do, in fact, was deny the farmer the **access** to those assets - by making them dysfunctional (local ecology), by prohibiting their use (local varieties: breeders’ law), or by declaring organics-based farming too expensive (imposing agro-economic policies that denied the need for soil health maintenance). But note that the government and its experts were convinced that it was all ‘for the good of the cause’. Everything was done to make the local setting receptive to those wonderful inventions of the new science: mechanical operations – industrial fertilizer – centrally bred plant varieties.

It is clear that in real life we cannot do without the local assets (local ecology, soils, and plants), and we need the local farmer to explore their local potential. But the technocrats will try to stretch the limits and approach those assets as mechanical entities that thrive on their industrial inputs and mechanical operations. From the very start this implied that they had to imagine that maintenance and renewal would simply follow their mechanical and industrial inputs. That attitude towards maintenance and renewal was scientifically and technically absurd. Its nearest equivalent is not technology, but hard-core slavery that strips man of everything except the absolute minimum of food and shelter needed to maintain his ‘labor power’.

The *right to life* for soils, plants and farmers can be handled similarly, by reducing them to cogs in the production machine. At face-value, by opting for a reductionist approach to research, the technocrat can forego difficult questions about the dignity of soils, plants and people. But in fact the results are abortive: it is the careful re-positioning of research results in the higher dimensions where real-life soils, plants and farmers are situated that decides about real-life expertise. Yet, the technocrats were convinced that the choice for a reductionist approach, in which those complex questions are hardly dealt with, gives us the freedom to design farming unencumbered by all the intricacies of ‘traditional’ farming.

The technocrats were prepared to go a long way to implement their ideas in agriculture. They treated plants, ecologies, soils and soil organic matter as if they were receptive to the technocratic approach. Local soils and plants had to be generalized-away and the technocrats were sure they would succeed.
Yet, the individual character of plants was common knowledge to farmers and to researchers like Barbara McClintock (corn genetics). Trewavas (1999, 2005, 2009) recently demonstrated

(a) the individuality of plants also on a biochemical enzyme level
(b) the intelligence of plants and
(c) plant behavior.

Our ‘standard’ plant is an artifact, even on a biochemical level…

If the subjects of their research did not give them the certainty that guided their research, what did? As indicated repeatedly, their expectations of S & T were those of the culture at large, a S & T able to give both nature and society a complete make-over. Especially after everybody lost his head in the aftermath of the Sputnik (1957), it was easy to remove the last obstacles for technocracy, for it promised us the Kingdom of Man.

Intoxicated, as all were, by the ‘dream of the age’, researchers in the long 1960s re-conceptualized humics after the example of an industrial polymer (Flaig 1966), and soil micro-aggregates as pseudo-chemical/mineral entities (Edwards & Bremner 1966). It was not the research subject itself that forced those reconceptualizations on the research world (so much is clear also from Flaig 1966; cp. Hatcher & Spiker 1988). Still researchers were of one accord, and for the time being scientists could try out their most advanced methods on this lifeless soil. That very effort was then one more reason that we were sure great results would follow.

Yet, it amounted to doing *agricultural research on Goli Otok*, the island that had since long been *denuded of its soil*. It is only when we caught sight again of distant green islands that doubts could arise about our research frame. But note that our rigid reductionism prevented communication with the inhabitants of other (research) worlds. We had not just withdrawn to Goli Otok, but we lived there in confinement, with our big institutes the jealous guardians of the reductionist order.

For it is **institutional history**, of course, that we are considering. Because of the accelerated growth of bureaucracy-directed institutes, agricultural policy, research and extension became centralized as strongly in western countries as in the communist bloc. The new experts did not doubt their assignments – functional rationality dominated their thinking as well as that of their principals in the bureaucracy (see Coolman 1972 for an example) – and so the unity between experts, bureaucrats, and their followers among farmers was impressive. It was so impressive, that it was mistaken for a ‘Green Frontier’ by outsiders. Here we will just give a summary outline of these developments in the leading country of the post-war decades, the USA.

In mid-war US, under the war administration, the (young) sociological and historical research into real-life farming systems got thwarted. As a result Long (1949) was a lonely voice, when he demonstrated links between real-life sociological information and economic information about farm labour and the scale of farming. Long e.g. took life history and age of farmers seriously, and did not use the term ‘agricultural labour’ without reference to age or life phase. That is, he did not accept the prevalent approach towards labour in industry. But most authors, by far, did and approached farming with thoroughly ‘a-social’ and ‘non-historical’ concepts and methods (compare the many contributions to e.g. Jesness 1949 and Halcrow 1955). They were proud to start from a ‘scientific’ rupture with ‘traditional’ farming and its local-ecological resources.

In the US, from mid-war on, the ‘industrial’ point of view was the politically dominant one. It equated farming with the factory process, located in isolated buildings and working with external resources. Implicated in that view was the ‘externalization’ of the social and ecological costs of the factory process. So this view was ‘twice wrong’: both in its easy equation of farming with the factory process, and in its approach to that factory process itself (where Taylor’s constructs were taken at face value).

Hardly a miracle that 'agricultural' research, dominated by this view, arrived at results that only are rarely 'down to earth'. Note that in all politics loomed large, but that it is not sufficient to explain the radical change in mentality. The unequivocal unanimity indicates ideological alignment: most authors agreed with the official policies because they agreed with the concepts of progress and modernity that were the hallmark of these policies. Remember Long's colleagues were mostly 'liberals' in the American sense, people who were proud of their critical-constructive participation in society. Yet it was those same people representing the academic world of the US who would give a decisive impetus to America's single minded thrust towards High Modernity (including its propagation elsewhere), while discarding any and all historic and contemporaneous alternatives.

Significantly, they did not 'see' those alternatives because most of them had never come accross them, neither in their academic education, nor in their research & advisory careers. As a result the best-informed authors from elsewhere – Boeke and Timmer in the Netherlands and Furnival in the UK among them – got evaluated as 'not relevant'. Especially when they wrote about the diversity of rural economics and sociology, and when they professed that they could learn from the traditional farmers. For a time even the great Russian agricultural economist Chayanov was completely unknown to the new 'experts'. Post-war **policies, therefore, were built on ignorance**. The contributions to e.g. Heady et al. (eds) 1956 show us that agricultural policy was out of touch with the broader agricultural realities. For years at a stretch, neither the historic-sociological, nor the ecological aspects of real-life agriculture were considered in shaping agricultural policy...

As it was, World War II caused changes in agricultural policy in Europe that were at least as rapturous as those in the US. Before long it was decided to reform agricultural research & extension the American way. Increasingly from about 1950 on, it was the American model that was followed. The single mindedness of it all, in countries like the Netherlands, is striking (e.g. Penders (ed) 1956). Once more, there was ideology behind it, not just politics. With such a near-religious 'conversion', the change to expert-centred, top-down agricultural research & extension was soon complete. The result was impressive - institutionally. Yet it was so 'solid' that signals 'from out there' could hardly penetrate....

It stands to reason that this 'short story' only sheds light on some sociological aspects of the course of post-war agricultural research. Once again: its researchers were human, just like us, and not the *l'homme machine* of the reductionist research frame. Though most of us allowed ourselves to be swept along by the ideology of High Modernism, some people refused and did not tire from pointing to the fact that it was contrary to real life. Schumacher was one of them and the following quote helps to reintroduce the history of people and societies to the lifeless research & extension world (Schumacher 1961, emph. mine J.V.):

> '***we should ask the much simpler and much more profound question: Why is it that people are not helping themselves? What has come over them?***
> *On the whole, throughout history, all healthy societies have managed to solve their problem of existence, and always with something to spare for culture. Grinding poverty, with malnutrition and degradation, not as a result of war or natural catastrophe – this is a most abnormal and, historically speaking, an unheard phenomenon. All peoples – with exceptions that merely prove the rule – have always known how to help themselves; thet have always discovered a pattern of living which fitted their peculiar natural surroundings. Societies and cultures have collapsed when they deserted their own pattern and fell into decadence, but even then, unless*

devastated by war, the people normally continued to be able to provide for themselves, with something to spare for higher things. Why not now, in so many parts of the world?'
'Poverty may have been the rule in the past, but misery was not. Poor peasants and artisans have existed from time immemorial; but the existence of miserable and destitute villages in the thousands and urban pavement dwellers in the hundreds of thousands – not in wartime or as an aftermath of war, but in the midst of peace and as a seemingly permanent fature – is a monstrous and scandalous thing which is altogether abnormal in the history of mankind'.
'For fruitful action, the whole of man has to be recognized. If this is not done and action is based solely on economic calculations as laid down in elaborate central plans, the only possible result can be coercion from the top. But what shall it profit? If coercion succeeds, freedom is lost; stultified by apathy and sullen disdain, the people sink ever deeper into misery. The alternative to coercion cannot be found when spiritual realities are dismissed as being of no account or treated as merely subservient to economic aims. It cannot be found when the people are considered as objects to be drioven, cajoled, or manipulated. Perhaps the best – perhaps even the only – effective slogan for aid is: "Find out what the people are trying to do and help them to do it better"'.

Yet, capitalist and communist society alike were ready to deny the relevance of succinct statements like Schumacher's. Both were commited to 'secure' progress in agricultural research by denying the essential (character of) contributions of farmer, soil and plant. Post-war agricultural research had its character determined in the years in which central planning by government and big industry was considered to secure progress for society at large. Once the problematic effects were visible enough, some planning concepts had to be withdrawn, but the concept of power from the centre thanks to 'superior' research was kept alive tenaciously. After all, the power position of the big and the expectations of the little people were dependent on the concept. One of the results was that an industrial agriculture that was not true to the life of soils, plants and farmers could go on growing.

We reviewed some of the results, and they are are hardly encouraging. It is well to ponder the connection of those results with the first decades of industrial agriculture, in which a great crowd of farmers saw their land taken away from them. Post-war agricultural research as an institutional project was hardly a neutral part of it, because it consistently robbed the small farmer of his self-respect. In the same vein it denied that soils and plants have an own contribution to crop growth that cannot be generalized away. It consistently treated farmer, soil and plant as subservient to its central directives.

Within those institutes researchers conformed to the 'dream of the age', just like most people outside. Yet, they were humans, and theirs was also a decision not to look and see. We will look at some examples of that refusal that pertain to mainline research's decision to stay aside from exiting new developments in bordering research disciplines.

8.7. New methods and perspectives – outside agricultural research

What if the field of agricultural research had not remained deprived of e.g. the chemical-analytical methods that were ready at hand in post-war decades? At the very least it could have safeguarded responsible use of mineral fertilizer… But as it was, the consistent neglect of the new methods, even of paper and thin layer chromatography and of electrophoresis, resulted in an *island position of agricultural research*.

For the **chromatographic and electrophoretic methods**
see Wieland 1948, Cramer 1953, Lederer & Lederer 1953, Lederer 1953/57, Linskens (Hb) 1955, Tiselius 1957, Helge & Laurell 1957, Hais & Macek (Hb) 1958, Smith (ed) 1960, Randerath 1962, Blackburn 1965, Pelick et al. 1966. Smith & Seakins (eds) 1958-1976 had several reprints in the 60s and was widely disseminated
Likewise, for the series '*Methods of Biochemical Analysis*' the 60s was a very fruitful decade (consolidating e.g. amino acid and peptide analysis, Weinstein 1966, Jones 1970). Also the famous series '*Advances in Chromatography*' had its origins in this decade. The development of those methods for the separation of purines, pyrimidines, and other components of nucleic acid hydrolysates by Chargaff c.s. – e.g. Vischer & Chargaff 1947, 1948 – was the entrance to molecular biology (Jaenicke 2007). In Chargaff's words (1978 p.91): '*It was a modest beginning…but we could separate and identify as little as five micrograms of each substance. I am not sure whether, before our work, even the millionfold quantity would have given equally reliable results*'.

As indicated already in Ch.7, post war decades saw an accelerated development of analytic methods which were utilized in exploratory biochemistry and the applied chemistry of amino acids, peptides and related organic compounds. And so not only in e.g. animal physiology and in medicine, but also in plant and soil sciences researchers took advantage of these methods (cp. the array of methods presented in Paech & Tracey (Hb) 1956). As to the biochem-istry of N, the spirit of discovery then hovering over the wider biochemical research community is quite apparent.

A celebratory volume to which first-rate researchers of those years contributed is Toivonen et al. (eds) 1955. It gives us a picture of exploratory researches of those years:

- the use of isotopes in unraveling biochemical pathways (Reio & Ehrensvärd 1955; Beloff-Chain et al.1955)
- advances in synthetic peptide and nucleotide chemistry (Schwytzer & Iselin 1955; Todd 1955),
- the isolation and characterization of N-compound classes from microorganisms (Gendre & Lederer 1955; Stacey 1955)
- mitochondrial amino acid metabolism (Rautanen & Tager 1955)
- the enzymology of N-compounds (Ott & Werkman 1955).

Not all of those researches would see the wider recognition they deserved (e.g. Lang & Siebert's 1955 publication got into oblivion in the 60s as a result of the dominance of the Watson-Crick dogma). Others would have only a gradual follow-up (e.g. Grabar's 1955 immuno-electrophoresis). Yet for all to see there now were a great many results on N-compounds in biota (Steward, Zacharias & Pollard 1955; Peterson 1955).

Peterson 1955 proved most of the new antibiotics to be peptides and other N-compounds. Micro-organisms excreting these antibiotics also released considerable quantities of other N-compounds into the medium, so a close examination of total excreted compounds was indicated. Note that antibiotics were largely produced by soil microorganisms. In antibiotics discovery, during and after the war, Selman Waksman, the great soil microbiologist, had a central role.

The relevance for agriculture to have a closer examination of the compounds excreted by soil micro-organisms and plants was evident. Indeed, researchers like Virtanen and Miettinen

(refs. see ch.7; Miettinen 1955) explored part of this field, and investigated possible interact-ions of plants and such organic compounds. *Yet agricultural research at large stayed aloof.*
And yet, in the broader research community, both research into antibiotics and into human hormones soon led to a lot of research into peptides (the 5th European Peptide Symposium was already held in 1963 and the 3rd American Peptide Symposium in 1972). Developments in peptide synthesis and analysis were a great help to researchers, and physiological and pharmaceutical peptide research was actively pursued.

Peptide research: Steward et al. 1955 review the results obtained with the new chromatographic methods. Next Steward & Bidwell 1962 and Steward & Pollard 1962 give an account of consecutive results. Their methods were widely known and accessible: for the post-war organic chemistry of peptides, incl. chromatography and electrophoresis, see Kopple 1966, for the wider research in peptides etc. Schröder & Lübke 1965, for consecutive research in peptide synthesis Gross & Meienhofer (eds) 1979. On developments in peptide research with physiological/pharmaceutical focus: Gross & Meienhofer 1979, Eberle, Geiger & Wieland 1981. Higgins & Payne 1980 give an overview of the research, outside the agricultural research circuit, of esp. the 70s on amino acid and peptide utilization by plants.

In fact, developments in the 50s were thus lively that, time and again, researchers outside the agricultural research circuit took a close look also at plant amino acid and peptide uptake and transport. Leading research like that of F.C.Steward c.s. brought remarkable results, so active exploration by agricultural researchers would have been perfectly normal.
Yet *food* peptide research only of late started to grow, while as to *feeds* peptides still receive only some incidental attention. Within main-line agricultural research, plant peptide uptake – or in-planta peptide signalling – up till the present scarcely receives any attention at all. Peptide uptake discoveries, and related discoveries that biochemists were making till the beginning 60s, at that time were neglected by main-line agricultural research, with broad consequences also for agriculture related plant physiological research. In short, an exciting field of research came to a virtual stand still.

Parallel with amino acid & peptide research, research in e.g. tetrapyrrole pigments (esp. for haem-proteins like chlorophyll) and purine & nucleotide research profited from broadly the same improved methods in post-war decades (e.g. Henderson 1972). Such researches in turn were of great importance for research in the functions of ATP and related compounds (Florkin 1975), for nucleic acid research, and for chlorophyll research (so for photosynthesis research). This provided a lot of results that were very interesting to agriculture related research, yet these results received only scattered attention in that field at best.

Food/feed peptides: Matthews 1991 gave a thorough overview of earlier research esp. in animal peptide-uptake, and of the curious 'stand still' for some three decades, from beginning 60s till about 1990 (cp. Mellander 1954 for a highly interesting example from the 1950s). For the recent food peptide research see e.g. Grimble & Blackwell (eds) 1998, Webb 2000, and Ganapathy et al. 2001. It is quite well possible that the lack of an explorative attitude in agriculture-related science cooperated with the Watson & Crick dogma - stressing strictly determinate de novo peptide and protein synthesis – to create the decades of neglect within the agricultural research institutes.

In all of these divergent fields, research of lasting importance always has one important characteristic: it has an eye for the broader connections. This is the material side of the fact that the **sciences**, which **are always communal efforts**, at least since Bacon do not depend on an 'experimentum crucis'. Convincing in the sciences is the 'whole story', as it originates in a historical setting and convinces the majority of those familiar with the research fields covering the subject at hand. Kohnstamm 1927 already gave a lucid exposition of this **social character of scientific laws** (Kohstamm 1927, IIIA, Ch.2):

> '*A natural law is a result at which a researcher – after serious and solid consideration of as many as possible of the data at his disposal – arrives, on account of a decision to do, for the time being, as if things stand firm, about which he is sure that he does not know for sure that they stand firm, and in which decision he is followed by a sufficient number of his expert colleagues*'.

This close interaction of social and material aspects makes each specific science greatly dependent on the state of its tradition.…

> Enzymatic studies, for example, do not figure in isolation but are part of wider investigations (cp. Chadwick & Ackrill 1994). And in kinetic and mechanistic studies 'curve fitting' of data to some ready-made mathematical function is of limited significance only (the ever finite number of measurements will always allow an infinite number of 'fitting functions'). Instead they depend strongly on wider chemical a.o. investigations that give positive clues as to intermediates etc. Summarizing: really decisive research always strives for integration.

If we consider the role of the 'new' methods in diverse science traditions, several major biochemical fields, like that of free energy sources in organisms and photosynthesis, had already experienced decisive developments before the war (Florkin 1975; Hill 1965). And yet also here the post war synthetic and analytic method developments introduced techniques researchers badly needed, a.o. to prevent their intricate research projects from becoming too isolated. In other fields, like that of phyto-chemistry, these improved methods were at the very centre (Haslam & Cai 1994 p.41), and researchers immediately adapted them to so-called secondary metabolite research in plants (Paech & Schwarze (Red.) 1958, Geissman 1963).
Now as the reviews from the 60s indicate, **the agricultural research community had easy access to these developments**, **but it did not use them**. Although their colleagues outside the agricultural research circuit used those methods (cp. e.g. van de Veerdonk's 1956 and Hofstra's 1966 PhD theses), its members left those methods unused.

> The field of **secondary metabolite research** grew immensely in a few decades (Wink 1999a, 1999b). As indicated already, of those secondary metabolites, phenolics are of primary importance in rust resistance (e.g. Southerton & Deverall 1990a,b, Carver et al.1994; see Zhang et al.1997 also for refs. on cytological and histological studies).
> Their role in broader disease resistance has been an active research subject from at least the 50s on and has since then been repeatedly reviewed - e.g. Rohringer & Samborski 1967, Kosuge 1969, Friend 1985, Harborne 1985, Nichelson & Hammerschmidt 1992, Matern et al. 1995. High mineral-N fertilization, by obstructing the formation and/or use of diverse phenolics (at the right time and the right place), in general promotes rusts, mildews, and many other plant diseases (Sander & Heitefuss 1998 a.o.).
> Disciplines that are important for feeds & foods, and for medical & toxicological reasons, a.o. that of alkaloid research, received increasing attention outside the agricultural research circuit proper. E.g. Labadie (red) 1980 gives some early results; Wink, Schmelzer & Latz-Brüning 1998, Kumpulainen & Salonen (eds) 1999, Yang et al. 2001, and Ross & Kasum indicate some of the progress made since then. Of decisive importance in the various realms of research are the analytical method developments (e.g. Waterman & Mole 1994), that allow a closer investigation of facts like the specific health qualities of e.g. red grapes/wines, Borbalán et al. 2003 and a greater specificity in designating bioactive compounds of e.g. rosemary (DelCampo et al. 2003) and black currants (Ehala, Vaher & Kaljurand 2004).

Not even their novelty can be an excuse for the fact that these methods were not used in main-line agricultural research. For the importance of these methods in the fields indicated, that all have an inherent interest for the agricultural researcher, was clear from their early introduction on. The pre-war versions of chromatography already had eminent applications in e.g. natural product chemistry, and as a result we find an early and eminent review of the accelerating developments in what was probably its leading post-war journal (Wieland 1948).

It should be kept in mind that both organoleptic and medicinal and health effects of many foods & feeds, and especially of many fruits and herbs, had been known for ages. That is also the reason that doubtful influences of fertilization were soon apparent to people conversant with the uses of such plants. The frequent loss of flavour with plants like black mustard for example, caused by mineral-N fertilization, was quite evident (Gershenzon 1984 p.278, refs). Indeed a broad public testified to its concern with the environmental loss of herbs as a consequence of mineral-N fertilization (part of a overall loss of biodiversity). As they expressed their concern with the loss of taste with herbs, fruits and vegetables cultivated with fertilizers. And it is certainly not scientific to deny fact finding to the ‘layman’ who evidently has an extensive practical expertise in a certain field...

A separate chapter is the great increase in cyanogenic glycosides, resulting from mineral-N fertilization, in important foods like sorghums. As this was discovered early on (Nelson 1953) the neglect of this phenomenon in the all-out propagation of the abundant use of high mineral-N by the majority of agricultural researchers and advisors, is rather astonishing. The more so because this same problem affected a wider range of food crops (for e.g. cassave, see Lancaster & Brooks 1983).

8.8. No antenna?

So when cancers took their increasing toll in the western world, knowledgeable people had ample reason to wonder about the changes in food qualities that possibly were related to the sudden increase in the use of industrial fertilizer. Recent summaries like Yang et al. 2001 still point in the same direction, and we saw already that in some specific instances the relation is very convincing indeed.

The increase in the use of fertilizer was sudden enough. In e.g. the Netherlands the amount of industrial N-fertilizer used for grass land more than doubled from beginning 60s to beginning 70s (when it totalled 200 kg N/ha on average, Van Diest). The high nitrate percentages were only too apparent by then – if only from nitrogen oxides production by silaged feeds. Moreover, earlier research had indicated high nitrate contents of fertilized grass and its undesirable animal health consequences (Sjollema, in van der Plank 1942 p.170). Small wonder that the farmers and the public at large started asking questions about the consequences for e.g. cows, diary products and meats. As to nitrate accumulation in vegetables, investigations already started in the 50s, the phenomenon by then being observed time and again, by reputable researchers.
Then research uncovered some disquieting prospects indeed. For example, fertilizer-induced nitroso compound formation, with most of these compounds carcinogenic, was spotted early on. Relevant research was reviewed repeatedly (e.g. Sen 1974; refer to Ch.5), so what prevented mainline agricultural research from responding?

An important reason was the contraction of research methods to those ‘fit’ for mineral fertilizer. We looked already at Bremner’s regress from specifying to non-specifying methods – for who needs elborate methods when only the mineral nutrients are important? Then when in the 60s Bremner c.s. looked at nitrite interactions with soil organic matter, they did not speciate anything, but from their application of non-speciating methods infered (wrongly) that nitroso-compounds were quite unstable in the soil environment. Following this same track, main-line researchers like Iwema & ‘t Hart (1972), though reporting some worrying consequences of high-fertilizer feeds, applied none of the analytic techniques that could have helped to identify dubious products of feed-nitrate/nitrite interactions (either in the field, in silage or in the rumen). The lack of specifying power of the methods they did use must have been clear from the outset, but apparently they were convinced there was nothing of importance to specify.

> Mainline agricultural & food research was also adamant in its denial of differences of taste, health and keeping quality between high-fertilizer and organically grown crops (cp. Beeson’s denial of vitamin differences). Yet, use of the post-war methods allows identification of important differences – e.g. Ren, Endo & Hayashi 2001.

Yet, as to the new methods (with specifying power) available, they were being used, for example, in research to determine the relations between alkaloid biochemistry and plant N-nutrition. This was a subject that for toxicity a.o. reasons was definitely relevant to agricultural researchers (see refs. Waller & Nowacki 1978 Ch.3). Note that from alkaloid research the relation between nitrate fertilization and nitroso-alkaloids in tobacco was apparent a long time (see Wilkinson & Nowacki 1994), with cancers as the outcome (from DNA adducts especially, Hecht et al. 1994). So there were certainly clues, and the methods needed were decidedly available to main-line agricultural researchers. Unless, of course, institutional politics motivated by functional rationality decided about the experimental set-up.

> Another example: research into **phyto-alexins,** plant-defensive compounds, saw a fast post-war growth (Cruickshank 1963) that continued unabated into later decades (Bailey & Mansfield (eds) 1982; Daniel & Purkayastha (eds) 1995; Harborne 1999). E.g., resistance to barley powdery mildew is connected with phyto-alexin production (Oku & Shiraishi 1995) and this evidently is one of the processes hampered by increasing use of fertilizer.
> Recently research into phytoalexins with e.g. antibacterial properties experienced a strong growth (Reichling 1999; Thomma et al 1999).
> But as indicated, industrial N-fertilizer interferes with the plant provision of many phyto-alexins (e.g. Close, Davies & Beadle 2001). In connection with the use of fertilizer gifts as related to leaf nitrogen (Dustin & Cooper-Driver 1992), attention has been paid esp. to phenyl-propanoids (e.g. DiCosmo & Towers 1984; Gebauer, Strain & Reynolds 1998; Graglia et al. 2001).

As it is, main-line agricultural research simply did not ‘receive’ signals of this kind. **It was as if the plant components indicated did not exist within the main-line agricultural paradigm**. For decades, and up to the present, crude methods that (at best) are indirectly connected with plant metabolites and structural elements were chosen, like ‘neutral detergent fiber’ NDF. But then, adhering to a ‘standard’ that lacks specifying power, research will never be able to ‘see’ the relevant components and relate them to the structures involved.

> Research into **anthocyanins**, especially known because of their roles in leaf & flower colour, and into other **flavonoids**, e.g. those known for UV protection, stress response and/or signalling, uncovered an ever increasing diversity of compounds & functions

(Blank 1958; Harborne 1988, 1994; Gould, Lee & Callow 2002). In the course of these investigations the importance, for the health of humans and animals, of flavonoids in foods & drinks, was dem-onstrated. At present it receives increasing attention (see Anderson & Markham (ed) 2006).
But there is a mostly negative connection between anthocyanin and/or flavonoid biosynthesis and large amounts of mineral N-fertilizer given to plants. In this connection the sudden attention, within the food industry, to 'functional foods' and to pre/pro-biotics (e.g. Special issue of the Br.J.Nutrit.1998) leaves one with mixed feelings. For the food ingredients that are advertized were diminished chiefly due to the industrialization of agriculture (esp. the use of large amounts of fertilizer gifts). And now this industry is offering these ingredients to the consumer at an extravagant price?!

The examples given will suffice to indicate the great importance of the newer biochemical and phyto-chemical methods & results for research into plant nutrition, as well as for feed & food physiological research. Yet, so far agricultural researchers have only made a very partial use, at best, of those methods & results. Even when using stable isotope methods they still neglect biochemical speciation - in spite of the fact that biochemists are their neighbours on the same agricultural university campus.

8.9. The influence of broader reductionist research programs

As indicated before, this neglect of methods is puzzling not only in the present, but also in post-war decades. But in those decades there was an important difference with the present: reductionist research strategies, e.g. in the Watson-Crick-Monod type molecular biology, were on the rise, and that in itself made it attractive for neighboring disciplines to follow their example. Still the choice for a reductionist program was a choice also then, not a *fatum*. Some biochemical examples illustrate the point.

The biological importance of mixed-function high-molecular compounds like glyco- and lipo-proteins, and of specific oligo-saccharides, was already intimated in the post war decades. Stacey 1955 as to mucoproteins writes (p.269):

> *'In the group there must exist some remarkable molecular structures with all the complexities of both proteins and polysaccharides. Mucoproteins include such important substances as serum mucoids, urinary mucoids, hormones of the anterior lobe of the pituiray, submaxillary mucin, components of colostrum, etc.'.*

About the colostrum he continues:

> *'Regarding the mucoprotein of colostrum attention is drawn to the recent important work of R.Kuhn and his colleagues. We investigated and ovo-mucoid some years ago in these laboratories and found that the N-acetyl glucosamines all formed end-groups in the carbohydrate portion of the macromolecule. We found a serum mucoid with a very similar structure. These structures need re-investigation using the newer techniques'.*

With a compound class relating both to lactation and ovulation it is hardly a miracle that research continued. And indeed Kent 1967 gives an impressive overview of animal glycoproteins.

So when Watson & Crick drew the attention of all to the DNA, one of the most experienced biochemists of those years, Erwin Chargaff, warned that other macromolecular compounds in the cell were at least as complex and specific as DNA, that is, they were extremely rich in 'biochemical information', yet were hardly 'determined' by the DNA. Chargaff's warning was of no avail: for some decades a disproportionate amount of attention (and finances) went to DNA-research. Then in the 80s, parallel to increasing doubts about the ‘despot DNA’ (because of intron splicing, post-translational modifications, etc), interest in the other macro-molecular compound classes grew again.

In the 90s ‘glycobiology’ (re)gained its rightful place, a place at an ‘information level’ comparable to nucleic acid research (as Laine 1997 brought into the limelight with his ‘*The information-storing potential of the sugar code*’). It was evident that many vitally important, biological functions of ‘proteins’ in fact do depend on the sugar sidechains of glyco-proteins, in plants as well as in animals.

Some collective volumes in **glycobiology**: Montreuil, Vliegenthart & Schachter (eds) 1995; id. 1996; Gabius & Gabius (eds) 1997; Dwek & Butters (ed) 2002. For biological functions of glycoproteins’ oligosaccharide side chains see e.g. Rudd & Dwek 1997, Lee & Lee 2003, Kimura 2007.
Fötisch & Vieths 2001 focus at allergenic glycoproteins (e.g. pollen allergy), Yamashita et al. 2002 at a glycoprotein wheat allergen, Cheung & Wu 1999 at the role of certain glyco-proteins in plant sexual reproduction, Albert et al. 2004 at glycosilation in plant defense. Glycoproteins play primary roles in root contact and nodule development & functioning with resp. Rhizobia, Actinomycetes, and Azospirillum (Brewin & Kardailsky 1997; Berry et al. 2002; Skvortsov & Ignatov 1998 plus Burdman et al. 2000).
Significantly, ammonium accumulation is bound to bring changes also in glycosylation patterns of glycoproteins (Valley et al. 1999, Gawlitzek et al. 1999), and that is bound to have consequences for (a) allergenicity (b) sexual reproduction (c) biological nitrogen fixation. Yet, the 1998 volume of *Advances in food and nutrition research* advised inquiry into the possible close resemblance of parts of amino acid sequence with known allergy-inducing sequences as an *exclusive* allergy ‘test’ of novel proteins. Once more an example of neglect of an important research field by agriculture-related researchers.

Without any doubt the unbalanced attention for ‘DNA’ in the 60s and 70s had its cause both in the extreme reductionism of those decades, that was obsessed by the idea of finding ‘the secret of life’ in a one-dimensional macromolecule, and in the institutionalization that ‘despot DNA’ received at the hands of administrators, editors and financiers. The resulting paradigm, built from ‘convictions’ as well as ‘institutionalization’, caused those researchers who yet wanted a broader biochemical approach, to face difficult decades, and much of their labours was done in the margins of their disciplines.

Doubtless agricultural research has a far wider, and more often than not, much more complex scope of research than molecular biology. But is it really inconceivable that similar mechanisms have been at work here? In other words: is it conceivable that a historical selection of a certain paradigm took place, that was intertwined with dominant opinions and policies, and that was subsequently institutionalized by editors, research directors and financiers?

In fact we came across such a candidate paradigm repeatedly already: the conceptual and institutional subservience to the “mineral nutrition” mindset leading to ‘research in a conceptual N-P-K box’.
If the biochemistry and physiology of the plant grows “naturally” from those industrial ingredients. If the “well-bred” plant’s only option is to gratefully use just those ingredients. Then the part of the agricultural researcher is limited. For then it consists chiefly in creating protocols, including models, for the application of those ingredients.

But once a variety of other nutrients and metabolites enters the picture, e.g. organic-N nutrients, in-planta peptides, glycoproteins, nitrated proteins, and the active roles of plants and micro-organisms are acknowledged, research needs wider concepts and methods.

Returning now to the post war decades, we will try to identify some of the origins of the attitude indicated, an attitude that, as to the use of a.o. the wider biochemical methods was one of neglect. To that end we will also look at some Ph.D. research projects from these decades (given the fact that Ph.D. theses as a rule give us more information than is available in the articles in journals).

8.10. Examples not followed

The spirit of discovery manifested by e.g. Toivonen et al.(eds) 1955 evidently affected other researchers too. The ready application of the new methods in medical research, for example, is evident from Verleur's Ph.D. thesis,

> *'A standardized method of paper electrophoresis and its value as a clinic-diagnostic expedient in its application to lung carcinome'* (Verleur 1958, in Dutch).

Building on quite a number of post-war paper electrophoresis developments, Verleur's primary goal was to specify the many details of the clinic-diagnostic method, which, more often than not, had remained unspecified. His aim was to arrive at a practical clinic-diagnostic method, applicable especially if the X-ray diagnosis did not yield clear results. And indeed, he managed to develop the various details so far that such an application became possible, provided, of course, that the diagnostician was aware of the limitations of the method.

Researchers in the Netherlands eagerly followed the new methods developments. Because their research groups already had experience with pre-war versions of those methods, they were able to participate in these developments in a critical and informed way. That is proved by e.g. Venekamp's exemplary biochemical Ph.D. thesis (Venekamp 1955)

> '*The metabolism of amides and amino acids in etiolated seedlings of Lupinus luteus L.*'

As is evident from many experimental details, Venekamp was able to draw from both pre-war and post-war research experiences in his country. He was actively involved in method development, as is shown e.g. in his adaption of starch-filled chromatographic columns, as introduced by others, for amino research. His extensive discussions about his extraction and determination methods certainly encouraged other researchers to enter the field. Considering the still limited size of the research community in those years, in the Netherlands as well as in most other countries, these research methods ready-made for exploratory endeavours could hardly have been missed in those post-war decades.

That is certainly the case if we think of the exploratory intent of Pol's research for his Ph.D. thesis

> '*Some correlations between different components of the potato and the variation in composition resulting from fertilization*' (Pol 1960, in Dutch).

For Pol studied at the same university as Venekamp, profited of the thorough laboratory training that was the norm there, and was evidently taught by many of the same professors as Verleur (the great organic chemist Coops among them). Indeed, he has a good discussion of several of the methods that he uses for his vitamin determinations. Likewise, he is on the alert for sources of variance showing up in the starch determinations (l.c. p.54).

Yet, he does not use the (bio)chemical methods that became available after the war, when scrutinizing the influence of fertilizer applications on the N-chemistry of the potato. The concept of ‘crude protein’ that he uses instead has no biochemical meaning – and there is no reason to think that he was not informed of that fact. When he effects some separation of his ‘crude protein’ in a ‘protein’ fraction and a ‘non-protein soluble compound’ fraction, he attempts no (bio)chemical speciation within those fractions. So who or what stopped him from applying the (bio)chemical methods that were part of his training?

Why didn’t he compare the *exclusive* use of green & farm manure with the application of industrial fertilizer, but nearly always added mineral fertilizer? From his review of the relevant literature we see that Pol evidently knows (see e.g. p.19) about studies pointing to the advantages of using farmyard manure, with the *exclusion* of industrial N-fertilizer. So why don’t we find this knowledge processed into the setup of own research?
And why did he not take a close look at the multi-level interactions of the ‘traditional’ fertilizers with the hierarchical soil? A comparison with the main-line picture, in which industrial fertilizers are one-dimensionally ‘feeding’ plant roots (only concentrations in solution are considered), would have been profitable indeed. And half a century of soil science stressing the soil’s living & hierarchical character hardly can have escaped his attention. Yet he does not breathe a word about it.
In his extensive discussion of the professional literature Pol demonstrates (see e.g. p.19) that he knows about research - both pre-war (he mentions Ydo 1937) and post-war (referring to e.g. Mulder 1953) - that indicates important qualitative differences, as to crops and their cultivation, of organic and industrial agriculture. So why didn’t he probe the causes of those differences, and start with a research design that would allow him to build on the results of those former research projects?

At the start of his PhD research, Pol got involved in institutions in which it was presumed that plants use exclusively ammonium and nitrate as N-nutrients, and that these two nutrients, provided they are given judiciously, simply speed up growth and enlarge biomass. Within this paradigm soil organic matter, when considered from the point of view of N-nutrition of plants, is just another source of ammonium and nitrate. Because of its extreme conceptual poverty, any qualitative differences between (the products of) ‘organic’ and ‘industrial’ agriculture are outside the paradigm (‘anomalies’ *sensu* Kuhn). Indeed, we see that other researchers, who worked within the same paradigm, simply denied such differences (Iwema & ‘t Hart 1972 p.321, Van der Molen 1974).
But of course, Pol and his colleagues were human beings, they did not function like robots. The incompatibility of their paradigm with information coming in from research and practice meant that they had ample reason to take a fresh look at the *assumptions* contained in their research designs. In order to be able to do just that they needed to devise *exploratory research* that could actually help them widen the scope of their research. Inside the laboratory the application of biochemical-analytical methods could very well have been part of that exploratory approach, and outside it the interviewing of experienced farmers a.o. connected with ‘organic’ agriculture .

> **Ethnographic research** had certainly become more fashionable since the war, and it was urgent, as industrial agriculture threatened to erase memories of organic types of agriculture. That type of field research had been standard in the Interbellum already in e.g. ethnographic musicology (Bartok & Kodály in Eastern Europe, Lomax in rural USA), as well as in geography (e.g. Carl Sauer). As to folk music, any educated person knew that it greatly influenced contemporary music.

The field researches had not just an archival role, but were essential in shaping ongoing traditions. In other words, field research was part of the wider scientific research paradigms of those decades.

Yet, as it was, main-line agricultural research made the easy assumption that traditional farming was not ‘scientific’ and that therefore it was no use interviewing (or cooperating with) traditional farmers, when seeking to advance ‘scientific’ agriculture. Remember that also the reputable American agricultural economist Theodor Schultz, after admitting to ‘traditional’ progress thanks to farmer-centred developments, subsequently concluded without any proof that in our ‘scientific times’ tradition had to be left behind... But, as a matter of fact, all this applied to the US. As to Europe, in 1927 Kohnstamm had not hesitated to call such easy assumptions ‘quasi-scientific’ and a troublesome leftover from 19th century positivism (Kohnstamm 1927 Book II Ch.II, e.g. p.144). In spite of such a high-level rebuttal, Schultz’ opinion got widely shared in Europe as well and, puzzling enough, especially in the Netherlands.

Pol failed to implement and develop any of the available methods for the kind of exploratory research that was needed. Still, in spite of these omissions, Pol got some remarkable results. “Nature speaking”, once more. So how did he interpret this “Voice of Nature”?

The decidedly lower storage life of mineral fertilized potatoes, as compared to the organic fertilized ones, was one of those remarkable results (and in keeping with the results of other researchers). It was also quite evident that the qualitative differences in ‘crude protein’ were a primary cause (figs 6 & 7, p.57). Qualitative differences between ‘crude protein’ compos-ition in mineral and organic fertilized potatoes had been proven already by e.g. Mulder & Bakema (1956). So why did Pot refrain from building on their results in his own research strategies? And why did he refrain from giving any recommendations as to follow-up research that could have shed some light on these matters? Keep in mind that storage life was shorter and that ascorbic acid and vitamine B-12 contents were lower with the mineral fertilized potatoes as compared with the organic fertilized ones. Pot had ample reasons to stress the need for exploratory researches!
As it was, Section 6 *‘Conclusie en discussie’* offered quite a lot of information on important differences between the properties of organic versus mineral fertilized potatoes. Yet, Section 7 *‘Samenvatting’* (‘Summary’) mentioned hardly any of them. The big differences in storage life and ‘crude protein’ that had been listed were only mentioned covertly, without specific-ation. There were no recommendations as to follow-up research. The thesis had only a Summary in English, which was just as devoid of results as the Dutch Samenvatting and did not reveal any of the important differences.

The best one can say about this research, which belonged to the top level of agricultural research in those decades, is, that it was unfit for exploratory research and so hardly useful in answering serious questions about the consequences of industrial agriculture (as to its use of industrial N-fertilizers).

The research was **financed** by the ‘*Foundation for population research in the reclaimed Zuiderzee polders*’ (l.c. preface). This was a government-directed, High Modernity-minded financier that would not have accepted any recommendations as to follow-up research into the lasting qualities of e.g. ‘traditional’ farming (see e.g. Venstra 1955).

8.11. A confusing paradigm

As stressed before, at the start of the 50s it was not just some isolated researcher who eagerly explored plant constituents (and uptake) with the new (bio)chemical methods. On the contrary: at the heart of Toivonen et al. (eds) 1955, to which a number of the best researchers of those years contributed, the most extensive contribution by far is

> *'Nitrogenous compounds in plants: recent knowledge derived from paper partition chromatography'* (Steward, Zacharius & Pollard 1955)

So it stands to reason that a number of the researchers indicated, extended this exploratory research, with more and more interesting results (cp. Holden (ed) 1962). And yet those intriguing plant research projects got marginalized by main-line agricultural research.

Research that is truly exploratory in character explores the fringes of the dominant paradigm. Of course, there is also much research that chiefly aims at answering questions withín the paradigm, as e.g. Hofstra's 1966 Ph.D. (Groningen) thesis

> *'Amino-acids in the root and bleeding sap of tomato plants'*.

In fact its chief research goal was to investigate where the organic-N compounds in the xylem sap originated, in the root or in the leaf, and what the differences were between ammonium and nitrate in this respect. With its limited research goal, the choice for mineral-N nutrition was fitting, for it enabled Hofstra to demonstrate quite easily e.g. the mineral-N assimilation in the root.

Still there is one point that needs to be emphasized. Researchers like Hofstra sometimes used organic feeding of the plant; Hofstra herself used succinate feeding in some of her experiments. These researchers did not deny the possibílity of organic-N feeding. That would not have made sense anyway: too many researchers had in effect used such organic-N feeding already (e.g. the asparagine feeding applied by Michael 1935 and by Kabos 1936). Indeed, Pot himself referred, in his extensive discussion of the literature (Ch.2), to many plant (or tissue) feeding experiments with organic compounds. Yet, it was all of no avail. Pot's institutional paradigm was rigid, and explorations outside its extremely narrow confines were evidently not allowed.

In Lackamp's 1965 Ph.D. on grass research we meet this paradigm and its restrictions once more. A perusal of his thesis

> *'An investigation into variability and heritability of the crude-protein contents in English ryegrass'*

immediately reveals that it is not about a clean & controllable laboratory subject. If anything, the specimens of his clones of ryegrass still show a great variety of individual traits (e.g. §5.5). The variations cannot be toned down by transferring the clones to fully homogenized and equalized soils in pots (on the contrary, the variation only increases, see §5.4).
Lackamp, who demonstrates a strong practical breeders' sense, concludes from all the various results that '*the great amplitude possible from the normal centre*' – as to plant properties – '*is exactly what is required for the resilience of the plant under all possible conditions of growth*' (l.c., Summary). Then he warns '*It is impossible to break correlations that are a vital part of the plant*' and reiterates the conclusion that as a rule protein content and total dry matter are inversely correlated. In fact he ends rather in minor key, when he intimates that the scale of selection that would be required to do follow-up research, is even beyond the scope of most commercial breeders.
So far, so good. But if we look at all that he leaves out, the picture changes. A few examples:

First of all, his research is ‘product research’ indeed. Surely mixed meadows, akin to many natural grasslands, had been found very productive too. Their mixes with legumes definitely required care, but quite some such possibilities were known where the biological nitrogen input was quite sufficient, also for the grasses and herbs. An evident advantage was that health-promoting herbs (animal feed) could still grow in such mixed meadows (Grünigen 1949). Moreover, many pests and plagues had a reduced incidence.
Yet Lackamp does not mention any of those possibilities. His research is in an important way not scientific research, but product research: it gives no clues as to other possibilities than the selected one. Note that grass variety selection to give ‘higher crude-protein’, with increasing mineral-N fertilizer gifts, is considered a worthwhile aim. Yet, a discussion of grass disease and animal feeding problems, related to the increase in N-fertilizer gifts, is ‘not done’.

Not only is Lackamp’s research unrelated to a wider ‘ecology of grasses’, its design even has no bearing on soil and soil life. The only reference to soil (micro)life is the explanation given for the curious phenomenon that parts of the same clone, when transferred to nearby soil (with properties that are strictly similar to the original soil), soon develop twice the protein content (§5.5). The fresh soil differed only in one point: it had not been used for the rye grass before, with its accompanying mineral fertilization. In other words, the absence of a previous ‘fertility-enhancing’ treatment induced the superior behaviour of the clone. Enough of a reason to look if meso-fauna in the ‘old’ soil could have been a cause of the lower results. But it proved not to have been the case.

By 1966 Lackamp’s experience (with transplantation to ‘fresh soil’) had in various ways been made by others too. Many farmers had experienced that the lush growth resulting from mineral-N subsided during the next years, and quite a few had noticed a deterioration of quality. Yet main-line researchers at best compared ‘fertilized’ with ‘unfertilized’ pastures.

> It is instructive to compare the researches of Grüniger (1949), from Liebefeld-Bern, with those of ‘t Hart and de Vries (1949) from the Netherlands. Grüniger gives ample information on the importance of botanically rich meadows, with grasses, herbs and clovers, especially for the health of ruminants. In complete contrast, ‘t Hart and de Vries limit themselves to experiments with mineral fertilizer only. They do not consider the botanical diversity, but use a measure derived from the dry yield of a grass instead. They call it ‘grade of quality’, but their only focus is on short-term yields with high fertilizer gifts (p.10, 16).

Comparison with e.g. the production of N-fixing mixed meadows was not easy. To lay out and maintain such mixed meadows required knowledge and experience from outside their paradigm. Operating safely within it, researchers made only very limited comparisons. And yet, they were boasting about ‘greatly increased yields’. Speidel & Weiss, both from the *Hessische Lehr- und Forschungsanstalt für Grünlandwirtschaft und Futterbau*, did exactly that (1971 S.71). Even though they pointed to the eradication of legumes by the industrial N-fertilizer in this same publication.

Speidel & Weiss also indicate the diminishing root growth, due to the use of increasing fertilizer gifts, that many farmers had found troublesome already (grass pulled out at grazing), but refrain from commenting on it. Later Dilz (1978 p.87) stated that this sod weakness is ‘*probably not directly, but by way of intensification [of cattle density] connected indirectly with N-fertilization*’. In that way a connection was denied that was widely known by then: the mineral-N fertilizer as a rule caused quick shoot growth, partly at the expense of root growth.

Now Dilz' paradigm is not that difficult to trace: he was employed by the Agricultural Bureau of the Dutch Nitrogen-Fertilizer Industry and stationed at the Institute of Soil Fertility. We see here the curious government-industry 'cartel' that is a distinct feature of the N-fertilizer (explosives) industries. Yet the fact that his background could easily lead to prejudiced conclusions was overlooked and his way of contrasting 'clean' mineral-N fertilization with 'difficult and variable' organic fertilization was accepted also by other 'experts' (even up to the present). **The real world of farming was exchanged for a 'clean' and supposedly constructable world of industrial nutrients.**

And so not only Speidel & Weiss, but also other researchers were even inclined to reconstruct 'grass land ecology' as such, but now based on experiments with 'well managed' grass lands receiving sequential levels of fertilizer. Van den Bergh 1979 (following de Wit) is one of them. He even states that *'there are very few examples of long-term observations of the botanical composition of old grassland',* and apparently finds that statement sufficient to build a new 'ecology' with the help of a sequence of fertilizer applications. But then, quite in contrast with Van den Bergh's statement Meisel 1969 gives plenty of information on meadows in north-western Germany, a region with close parallels in the Netherlands. It is quite sure as well that ample research had been done on other types of meadows too (e.g. in hills and mountain regions in Germany - Hundt 1964). So what exactly made these main-line researchers disregard the historical concepts and methods pertaining to real-life grasslands?

8.12. The cost of neglecting tradition

It is clear by now that a research community had come to dominate the scene, that only focussed on the abundant use of fertilizer, with concepts and methods that left no room for alternatives. With industrial fertilizer conceptually 'naturalized' by this research community, a **careful research into e.g. soil microbiological and plant pathologic aspects of the excessive use of fertilizer, as compared with stimulating crop growth in legume rotation and/or with farm manures, was deemed irrelevant.**

In the same way, with only industrial fertilizer regarded as 'natural', it is small wonder that soil micro-morphology was considered of secondary importance by mainline specialists, and soil microbiology chiefly relevant in terms of its supposed role in the process of providing industrial nutrients. As a result, main-line researchers were content to treat 'soil organics' as a Black Box. They were interested only in the presumed process of 'mineralization', the process presumed to make industrial nutrients available to the plant.

> They evidently thought it largely superfluous to get involved with 'side-tracks' like micro-morphology and microbiology. Indications of the importance of those disciplines for agri-culture - e.g. Kubiena 1938 & 1948, Jungerius 1957, Babel 1971, Bal 1973, Jongerius 1973 – were not denied, but the disciplines were considered 'immature' untill they would allow the same 'precise' applications that were 'evident' from mineral fertilizer-based cropping and modelling experiments. Untill then the disciplines were considered 'of academic interest' only and just allowed a trickle of finances – untill they would have proved their practical value in the way the PKN-approach to cropping had done (in the opinion of bureaucracy). But note that soil micromorphology and soil (micro) biology are exactly those scientific disciplines that could have offered connections with traditional, 'organic', agriculture…

We saw already how main-line agricultural researchers were - for N-nutrition especially - always thinking in terms of 'sand plus mineral solution' as the conceptual standard. Except

for the anchoring of the plant roots, this amounts to an essentially one-dimensional model that lends itself to easy extensions (e.g. Greenwood et al. 1971). Within this model all real-life characteristics of soils – their hierarchical structure, their heterogeneity at all levels, their baffling microbiological diversity, the multi-level plant-microbe and plant-soil interactions – are in effect reduced to the sole dimension of 'mineral solute concentration'. It is this reductionism that, because of its extreme conceptual poverty, severs every link with traditional and real-life agriculture.
Without this extreme reductionism only research based on respect for the farmer would have made sense. That reductionism could nevertheless prevail is because mainline agricultural research, in its affiliation with government and industry, subscribed to Theodore Schultz' theory of a necessary break between 'traditional' farming and 'scientific' agriculture. This was a theory that made a sensible comparison of 'traditional' and 'industrial' agriculture superfluous. It simply referred to traditional agriculture in disparaging terms, in spite the fact that its farmers been the mainstay of food provision up to the time that Schultz c.s. issued their decree.

To American economists like Schultz, the specific breeding developments starting with high-fertilizer corn hybrids suggested the 'scientific' character of 'industrial' agriculture. And that in contrast with the 'pre-scientific' character of 'traditional' agriculture. That these corn-breeding developments actually were an expression of breeders' impatience evidently did not occur to Schultz c.s. These breeders had chosen to focus at easily traceable responses of corn varieties to ever-higher fertilizer gifts, as a way to get around the local soil, farmer varieties, and ecology. The big variations showing up in the responses of corn varieties to low fertilizer gifts and to different environments were lost to them. With their excessive fertilizer gifts leaving them only one degree of freedom, they caused great impoverishment, both conceptually and methodically, in corn breeding after the war (e.g. Viets & Domingo 1948).

In the same way as with corn, clover-wheat rotations and the like had clearly shown capable of giving high grain yields, with mineral fertilizer playing a more negative than a positive role. Already early on there was no doubt about the importance of G x E interactions in crop yields (e.g. Black, Nelson & Pritchett 1946). Yet such interactions required rotation-based and similar 'organic' agriculture for their exploitation. That is, these interactions belong to agricultural systems in which local knowledge and experience is of decisive importance. In contrast, an 'industrial' approach that disregards local experience, and local soils & ecologies, is bound to choose the high-fertilizer track. That is the only one that shifts the power to laboratory and government and/or business: derived from the 'laboratory ecology' of industrial nutrient solutions, it excludes any and all local degrees of freedom. Of course, this shift in power only materialized where farmers were dissuaded from using their local resources. And that was a process in which government everywhere played a dominant role.

As it was, the post-war US impatiently chose for 'big solutions' (reminiscent of its war industries), and most other countries then looked at its 'accomplishments' with great envy. In this peculiar episode of history, the theory of Schultz c.s., though hardly convincing, still seemed feasible, because the world at large impatiently pursued big 'solutions'. The means chosen implied a strong, even desperate, faith in reductionism.

As to the latter, remember that faith in a constructable world was the mainstay of post-war society, in the West and elsewhere. One has to consider the existential needs, as well as the politico-religious promises of the big powers of those times, to find the sources of this faith. **But science & technology, being limited human enterprises, cannot offer the 'certainties'**

that this faith was looking for. Yet, in post-war years it is exactly the faith in an 'unlimited' science & technology that all the countries that were eager to 'grow' embraced. But of course, for this faith to have substance, nature itself had to be viewed as constructable.

Research that boasts of its rejection of centuries, or even millennia, of farmers' experience, is bound to start using concepts that lack foundation. Lackamp uses 'crude protein' as a solid concept, but his discussion of it (and of its relation to true protein) is inferior from a scientific point of view, even in those years. For in the 60s it is widely known that a 'crude protein' determination in itself is hardly a meaningful concept: it only indicates an unknown mixture. And then, Lackamp's non-discussion of the specific design of the Kjeldahl N-determination on which he depends for his 'crude protein', is even less commendable (a vast number of chemical publications was available – e.g. Kolthoff & Stenger 1947 p.173f.). That is, Lackamp **constructs 'certainties' by not discussing his concepts and methods**.
His results suffer from it, of course, for now they are repeatedly of a questionable quality. Worse even, his research goal now is indeterminate. Yet his supervisors agreed with his non-consideration of relevant science. Lackamp and his professors could definitely have done better, by using recent methods developments in analytical and phyto-chemistry to establish *biochemically* meaningful concepts and research goals. They likewise should have linked up to 'traditional' farming to arrive at meaningful *agricultural* concepts & goals.

For 'tradition' in farming was indeed versatile enough. We saw how Yanni c.s. recently illustrated the, from an agriculturally point of view, well-adapted character of the 700 year old rice-berseem clover rotation in Egypt (e.g. Yanni et al. 1997, 2001). In the same vein, inter-cropping of beans and maize was the rule in e.g. Mexico and Peru, where a.o. centuries of farmers' care could be observed in well functioning, but delicate, bean-*Rhizobium*-maize associations (Piñero et al.1988, Pineda et al.1994, Souza et al.1997). From research focussing at the possibilities of local farmers and their crop varieties, the soil biological and agricultural qualities of such systems have become clear enough (Gutiérrez-Zamora & Martinez-Romero 2001). Yet, when Rockefeller researchers started to 'develop' corn growing in Mexico during the war, the first thing they did was spoiling those associations with e.g. their use of fertilizers (e.g. Colwell 1946). 'Modern breeding' as a whole was blind to these and similar associations and symbioses, and disrupted them severely, also by pushing its non-adapted varieties, the HYVs, with the strong arm of government or business (Martínez-Romero 2002).

> That similar associative systems can be developed also today is clear from the research of Howieson c.s., in which acid-tolerant rhizobiae from the Mediterranean basin after due effort allowed some one million hectare legume culture on acid soils in south Australia. Similarly selection of acid- and Al-soil adapted rhizobiae introduced well-adapted soybean culture to the vast Brazilian Cerrados.

Working within a paradigm that was not adapted to 'traditional' farming, researchers like Lackamp used concepts & methods dubiously geared to their field of research. Their 'chemical' concepts & methods were (and are) of a crude kind, if valid at all, and not helpful in evaluating new developments like the use of excessive fertilizer gifts.

For a close, some last examples:
D.I.H.Jones (1970) does research into '*The effect of N fertilizers on...*', but is pre-determined to accept only positive results and skips testing the effects of high nitrate contents in the grasses (l.c. p.520).
And Blaxter et al. 1971 present their research as '*The effects of nitrogenous fertilizer on...*',

but do not even mention the possibility of grass-nitrate interactions. Research into such interactions is not only perfectly possible in those years, but urgent too, because of the steep rise in mineral-N applications.
As a matter of fact, by performing the obligatory step of reduction before the Kjeldahl assay – obligatory because it was known that e.g. nitro-compounds escaped Kjeldahl determination - some Russian researchers indeed do look for nitr(os)ation products and other interaction products. But being the only ones taking this effort – which is chemically the only commendable one - their work is not noticed (Tschurtchin et al. 1960). Only recently did this situation change: researchers from outside the agricultural research field started the investigations – and found a.o. nitr(os)ated proteins.

8.13. Not to explore is a peculiar choice

The post-war decades in agricultural research offer us an example of tunnel vision. Yet, there was an exception: the research of Hughes in the last half of the 60s could have been the start for main-line agricultural research's use of well-adapted methods. Taking *'the non-protein composition of grass silages'* as his research subject, Hughes actually applied chromato-graphic methods to investigate amino acids (also from hydrolysed proteins) and volatile & non-volatile amines in silages (Hughes 1969, 1970, 1971). Considerable concentrations of putrescine and cadaverine were among his most noteworthy results.

And yet Hughes also refrained from investigating the interactions of grasses with N-fertilizer. By applying a regular Kjeldahl method (for total N) he must have missed e.g. many of the reaction products of nitrates (from fertilizer) with plant organics (see for a source easily accessible to Hughes c.s. Kolthoff & Stenger 1947 p.173f.). Next he refrained from analysing the 'non-protein nitrogen' because it was only a minor part of the total - and because of '*the highly specialized procedures necessary to make a detailed study of their composition*' (Hughes 1970 p.427). The latter statement is hard to justify: researchers like Stewart and Miettinen had shown the way already by using rather simple paper chromatographic procedures, and everybody could have known that they were at the centre of their field (in the 50s and early 60s).

Apparently Hughes' research received no follow-up because others concluded that it was 'definitive' instead of 'exploratory' - not 'we need closer investigations' but 'we now have the answers'. And indeed, the way Hughes published his results did allow such an interpretation (by people who were inclined to be biased anyway). From then on about all agriculture related research stayed within the NPK-paradigm. That is, it chose to do without any really explorative element at all, neither conceptually nor methodically, and its researchers consistently failed to use the newer chromatographic and related methods.
But then, as indicated, even Hughes did not explore e.g. the impact of the recent agricultural methods on plant metabolism and food & feed quality. In fact it seems that even he was convinced of the 'natural' character of those recent agricultural changes, as he perceived this 'naturalness' as a logical consequence of the 'natural' character of the industrial nutrients...

And so with the partial exception of Hughes' work, agricultural research approached 'industrial' crop growing and the like for decades as needing *extension* only, no *exploration*. In a peculiar way it seemed to work with the assumption of a 'world without surprises', which needed no exploratory research and methods development, but allowed the application of 'standard methods' instead. Just like an outworn factory process...

We do meet an example of this kind of research in Deinum's 1966 thesis *'Climate, nitrogen and grass'*. He proudly uses the 19^{th} century Weende system as his base to determine the nutritive value of grass and other forages, without first exploring the limits and shortcomings of this system. In doing so, he causes his own research to have dubious results, at best. Deinum states that with high nitrate gifts, the plant needs abundant light to use it, instead of accumulating it. Yet he does not investigate the biochemical a.o. consequences. He is satisfied with referring, on others' authority, to a light-dependent nitrate-percentage above which accumulation instead of use (by the plant) will prevail.

Deinum indicates that his 'crude fibre' consists of (bio)chemically and biologically heterogeneous (classes of) compounds, yet refrains from exploring methods that would allow meaningful speciation of this 'crude fiber'. The result is that his research yields some relations of questionable meaning (e.g. 5.4.6). Furthermore, plant constituents that by the 60s were well-known (e.g. anthocyanins), do not figure anywhere in his research. Yet, by the 60s some far more specific, and not too difficult, determinations could have been used that would have given him results that wére biologically meaningful (see e.g. Coulson, Davies & Lewis 1960a, 1960b; Davies, Coulson & Lewis 1964a, 1964b).
Deinum's 'crude protein' determinations fall victim to the questionable meaning of the concept. Just a simple example: the 6,25 factor used to get from Kjeldahl-N to 'crude protein', by multiplication, has been proven many times to be (far) to big, and to depend on circumstances that need specification. Furthermore, to be able to distinguish between qualities of protein, much more specific and precise methods of determination are needed.

As it is, Deinum seems to be the victim of the complacent attitudes towards research displayed by his supervisors and their institutions. In all probablilty, by the mid-60s their institutional research paradigm had increasingly been walled-off, both from newer concept & method developments, and from information from & cooperation with 'traditional' farmers.

8.14. Perspectives foregone

Richard Bradfield in 1946, at the general meeting of the Soil Science Society of America, applied the contemporary physical chemistry of soil-plant relationships to the question '*where are the new discoveries in soil leading?*' When we read his report, it becomes crystal-clear that his research paradigm, which he evidently shared with a good number of other soil scientist, had not yet been walled-off.
Contrary to the 'industrial' point of view Bradfield stated that the '*experiments which failed to show a profitable response to the use of fertilizers*' were often a '*cause for rejoicing*'. An important reason for that opinion was, as we saw already, that those fertilizer experiments which not just wiped out all differences with a lot of fertilizer, had always found big variability in results. Especially big were the G x E interactions, as to yields, something inviting the boosting of yields by judicious use of local soil and plant qualities. Leading researchers like Bradfield were not impressed with many of the 'fertilizer trials'.

Harper 1945 mentions a great number of fertilizer **trials that did not show a profitable response to fertilizer**. Skinner et al. 1937 mention in passing (p.16) that the efficiency of the new fertilizers decreased in time – a painful reality experienced by an innumerable number of farmers in post war decades. Depletion of soil fertility by most fertilizer treatments except for animal manure application is clearly indicated by Smith & Vandecaveye 1946.

Yet in spite of the extensive evidence for a limited applicability of industrial fertilizers, from the very start of their introduction, the outlay of most experiments was fitting to 'product research' only and not to an in-depth comparison of organic vs mineral fertilizer use (e.g. Skinner et al.1937). From the very start these were experiments that focussed the farmer on the use of industrial fertilizer, discarding any and all alternatives.
This in spite of the fact that for decades researchers had intimated direct weathering of soil minerals by plant roots - something that since then has been demonstrated for K-minerals by e.g. Hinsinger et al. 1991.

Bradfield reminds his readers also that the '*solid-phase feeding*' of plant roots had been proven time and again. Something we find also with Lehr (1940, Un. of Utrecht) in the Netherlands, who stresses that

> '*because of the lack of contact with soil particles, the supply of less well-soluble compounds (e.g. Fe-compounds) passes off difficultly*' (l.c. p.22).

We indeed see an (as yet) industry-independent mind at work in Bradfield's words (l.c. p.6):

> *'The reserves of plant nutrients in the soil beneath our feet are enormous in most of the arable sections of the world. For example, there is more potash in the top 4 feet of soil in the State of New York alone than in all the known reserves in the United States'.*

Solid-phase feeding of roots: some recent references are Jongmans et al. 1997; van Breemen et al. 2000; van Breemen, Lundström & Jongmans 2000; Berthelin, Leyval & Mustal 2000; Valsami-Jones & McEldowney 2000; Puente et al. 2004; Douglas 2005; Lower et al. 2005; Davis & Lüttge 2005. Exquisite is the recent study of rock weathering by desert plant roots, Puente et al. 2004 and Puente, Li & Bashan 2004.
Related is Lower et al. 2005 on metal reduction and Nealson & Little 1997 review of solid-state respir-ation. Important for the subject are also: Nedwell & Gray 1987 and Mills 2003 on microbial life in soils and sediments; Costerton et al. 1995, Davey & O'Toole 2000 and O'Toole et al. 2000 on microbial biofilms; Rawlings 2004 on microbial ore leaching.

Bradfield's independence is closely connected with his conviction that environmental factors yet unknown, play an important part:

> *'I often have the feeling that many of our research workers feel that their job is done when they complete their analysis of variance and establish the odds of significance for their experiment. This is often done even in cases where the variation in yield due to factors which are uncontrolled or are unknown is of the same order of magnitude as the factor or factors being studied'.*

He then encourages his readers to regard the statistical analysis

> *'as a guide to unsolved problems regarding the factors in the environment which are exerting an important influence upon the growth of the crop'.*

In the same issue of the 1946 Proceedings (of the SSSA), the soil microbiologist Norman takes a close look at recent questions and advances in his field of research. As to the host of soil microorganisms that are evidently present, even if they hardly appear when feeding the soil community with easily degradable 'energy materials' (e.g. glucose, mannose), Norman remarks:

> *'It may be that the simple carbohydrates that have mainly been used in such studies are poorly utilized by these organisms, which in the soil must be dependent on far less available sources. It is more probable that this group can slowly utilize the resistant lignin-derived and nitrogen-containing organic residues that remain when plant materials undergo decomposition, in which case their agronomic importance is much greater than the limited amount of attention they have received would indicate'.*

It is quite clear that both Bradfield and Norman are open minded as to the natural resources at the farmer's disposal, and not inclined to believe that the fertilizer industry has superior resources available (neither qualitatively nor quantitatively). In their opinion research still has a function in helping the farmer to disclose these (free) natural resources. Surely the main-line agricultural research paradigm that is soon to dominate any and all institutional research was not of their making. Yet, as to soil microbiology, with its possibilities in cooperation with the farmer to disclose the resources in the farm soil, Norman has to warn his readers:

> *'It is not a subject that is well supported at present and indeed the funds and facilities available for this type of work have probably changed little in the last 20 years'.*

Indeed, this situation did not change greatly in the following years. In spite of the phenomenal growth of main-line agriculture related research, the soil science disciplines relating to the hierarchical & living soil experienced a very modest growth, at best. More importantly, their contribution to the development of agriculture in the decades to come (after 1946) would scarcely be sought. Consequently, exactly those disciplines that could have contributed to a development of agriculture with an eye to interests and possibilities of farmer and ecology, got effectively marginalized.

> Indeed, as to e.g. research into N-contributions to the plant by free-living Azotobacter , the contributions of Winogradsky c.s. were soon forgotten. To such an extent that, when research in free-living Azotobacter contributing to plant growth was resumed around 1990, these results were apparently not used at all.

8.15. Post-war agricultural research: Summary & outlook

1. The newly institutionalized agricultural research very soon rejected, effectively, all kinds of exploratory research, and approaches that respected the farmer and the local ecology.

2. The institutionalization of agricultural research was nevertheless impressive, e.g. in terms of its financial resources. Therefore, the double isolation of this research put its stamp on quite a few other research fields too, especially on disciplines that were closely related to main-line agricultural research. There explorative research was easily marginalized or, worse still, agricultural research in effect imposed its box-like research paradigm.

3. Institutionalized research got completely entangled with government and industry, after the war. For example, in its commitment to the officially endorsed, 'high-fertilizer' crop varieties, to the exclusion of farmers' varieties. Quite generally, in its discard of farmer-and-ecology centred inputs. As a result, research policies, as well as research methods, were completely subservient to the policies of government and industry.

4. Hardly any of the natural resources that are available to the local farmer & community have been developed. That means that there are richess to explore - but it had of course as its concomitant negative side:

5. immense efforts were applied to the de-localization and de-ecologization of agriculture, by swamping it with industrial ingredients designed in central laboratories.

6. Such swamping implied a definite denial of the realities of farming & ecology, and its results have been devastating (health effects of fertilizer in feed & food; multiple unsustainability of high-fertilizer crop growing; etc.).

7. Yet, because of the ‘walled-off’ research with its tunnel vision, also inquiries into these negative results have been hampered severely, with as an important result that some of the worst effects have remained largely undetected, up till very recently.

8. Uncovering such effects is the negative side of a movement to re-instate farmer & ecology, and to have society at large to regain a true perspective as to the rural & urban future.

9. This re-instatement of farmer and ecology is the only choice that is ‘down to earth’ and true to life, and therefore the only entrance to sustainable food production.

In the first paragraphs of this chapter we learned that in the earth sciences, and as a corollary in agronomics, we meet, equally, both generalisation and contingency. There was and is no justification for the introduction of the reductionist approaches that, because they were part and parcel of post-war technocracy, landed us in the present disarray. Quite to the contrary: the local level needs adaptive management by the local expert (the farmer) if we want the realisation of sustainable ‘emergent’ properties & practices. In the words of Kirschenmann (2006):

> ‘*as we are learning from ecologists and social scientists, adaptive management, especially when emergent properties reign as they do in nature, is far more reliable than control management. Control management, which lies at the heart of industrial agriculture, operates under the assumption that constancy is the rule. But, as C.S.Holling (1995) reminds us,*
> “*Assumptions that such constancy is the rule might give a comfortable sense of certainty, but it is spurious. Such assumptions produce policies and science that contribute to a pathology of rigid and unseeing institutions, increasingly vulnerable natural systems and public dependencies*”’ .

As Kirchenmann and many others demonstrate, ‘*complex, synergistic systems are proving to be much more productive than monocropping systems, while using far fewer, potentially environmentally damaging inputs*’. Such complex, synergistic systems were a ‘trade mark’ of traditional agriculture, and we denied them to our own peril. For some time now we witness their re-discovery, but to dispel the collapse of food provision we need the explicit re-installment of farmer and ecology. That re-installment will not occur unless government and industry and its expert step back from their presumed central place and allow farmer and ecology to take the centre again.

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9.

Puzzling policies

9.1. Agripower and food prices

> *'What sort of productivity might exist in the Corn Belt today if researchers had spent the last fifty years concentrating on developing varieties that would grow best with a groundcover legume that would both hold the soil in place and provide nitrogen?'* (Judith Soul & John Piper 1992 p.64)

As we saw in Ch.6, Soul & Piper's question is not an utopian but a real-life one. Research in corn growing for the small farm with its mixed farming, and focussing on the use of local natural resources, was an integral part of agricultural research at many Agricultural Experiment Stations in the US up to the war. It was only in the context of the war economy that agricultural policies effectively started to exclude the local-resource options, for up to that point the great importance of local natural, human & community resources had been duly acknowledged by many researchers.

As Lewontin a.o. have convincingly argued, research certainly could also have focussed on open-pollinated varieties instead of on hybrid corn. Stated more generally still: we saw in Ch.6 already how post-war, officially endorsed, corn breeding, made no use at all of e.g. most of the exciting discoveries in corn genetics (those of Barbara McClintock, cp. Keller 1983). It simply stuck to this one option, the high-fertilizer one.

Yet research could easily have remained open to the many possibilities offered by the corn's rich biodiversity. For they were exemplified in the farmers' and wild varieties, and effectively used in their traditional apllications (e.g. Louette, Charrier & Berthaud 1997). There is no doubt that the Rockefeller financed research, which had its start in Mexico during the war, was familiar with these uses and varieties (e.g. Colwell 1946). The fact that even 'weeds' were integrated into these local agro-ecologies could scarcely have escaped notice (cp. Ch. 2).

If their institutional paradigm had allowed them to take real notice of all of this agro-biodiversity, researchers could have linked up with the cooperative research that was still close to mixed farming and to the small farmer in their own country. Then, by taking local practices seriously, their research program would have greatly opened up, and hybrid corn would only have been one of the many possibilities actually pursued in the cooperative research endeavours. Then seed procurement would also have remained primarily an on-farm activity.

It is especially such on-farm development of the rich biodiversity resources at his disposal that would indeed have made the farmer a 'self made man'. The fact that his access to these biodiversity resources was cut off, due to the big-industry research approach of the Rockefeller Foundation, caused his dependency on big agro-industry. Still, it is important to realize that, though one-sided in character, pre-war hybrid corn breeding as to was not the real culprit of this dependency. For it was to a large extent a cooperative farmer-researcher event and did not greatly diminish the farmer's economic independence (because it did not cut the foundation of farming in the local ecology & community).

A number of political actions during and after the war cut out virtually all options but the high-fertilizer one. This was as true for Europe as it was for the US. Then the total package, as it is part and parcel of the high-fertilizer approach, dislodged farming from the local ecology & community. With it the farmer became a dependent in every respect. The following quote (Soule & Piper 1992 p.62) gives a good impression:

> *'Only 10 percent of the value added in agriculture is added on the farm. In other words, about 10 percent of the price paid for food reflects the work of the farmer. Forty percent is added in creating inputs that the farmer must purchase, and 50 percent is added between the farm gate and the table'.*

The farmer is caught between oligopolistic suppliers and buyers. They in effect determine the farmers' part of the 'value added' – and they always make it less. The phenomenon - known as the 'scissors' for a long time - has some aspects that can already be traced to e.g. farm-city relations in the later Middle Ages. Still the slump in the farmer's percentage in the 'value added' occurred in the 20th century. During the post-war period it soon seemed to attain a 'free fall'. For although prominent authors like Galbraith (1953) stressed the importance of maintaining a 'countervailing power' (benefiting the farmer instead of the big economic agents), their warnings have not been heeded.

This central problem of post-war agriculture remained hidden especially because of the grossly insufficient discussion of the main-line economy. One of its worst practices was (and is) its focus on prices only, neglecting most other aspects of the real-life economy (cp. Ch.1). Another of its bad practices is (Schumpeter a.o.) treating 'technological development' as a kind of 'natural necessity', instead of the embodiment of human, cultural and political choices.

9.2. Dangerous policies

And so we see the great majority of agricultural economists in post-war decades treat high-fertilizer, fossil energy dependent agriculture as a 'technologically inevitable development'. As a corollary they keep silent about the decisive roles of prominent economic and political authors in cutting the farmers' bonds with their local resources - resources that had always constituted the main part of the farmers' autonomy. Soule & Piper bring us back to reality in the following extensive quotation (1992 p.62/63) (in which, by the way, 'good business' refers to the current 'business-is-business' approach):

> *'What is good for farm-input manufacturers is farmers who continue to need their products. It is good business for pesticides to be addictive, but it is not good farming practice. It does not help the farmer make the budget balance, and it does not keep the farm and surrounding neighborhood a healthy place to live.*
> *It is good business for a seed company to produce seeds that do not breed true, so that they must be purchased each year. It does not help farmers to have to add seeds to their list of purchases for the year.*
> *It is good business, when chemical companies own the seed companies, to produce varieties that thrive with heavy fertilizer applications and pesticides. But farmlands and farmers would be better off with varieties that thrive in soils rich in organic matter, are competitive against weeds, and are mixtures of lines with a variety of resistances, so that pesticide use could be minimized.*
> *It is good business to keep inventing more expensive and more sophisticated machinery so that farmers have to continue upgrading to keep up with the competition.*

But farmers who can operate thriftily by repairing and altering old equipment as needed have one less source of major debt.
It is potentially very profitable for a company to develop a crop that resists herbicides so that it can be grown using herbicides and marketed as a seed-plus-herbicide package, as some biotechnology companies are now pursuing. Though this may be useful for soil-saving reduced tillage, over the long run it will not help farmers or the general public for herbicide exposure to rise, nor does it help for seed prices to rise'.

As western society at large, with vertical integration in the food sector now the rule and with only a few big retailers governing most of the citizens' food supplies, the economic merging of agro- and food-companies has already become an outright danger in a political sense. We experienced some of the immense dangers of such a system during World War II, when totalitarian powers tried to crush any resistance by exercising 'food power'. Evidently, inviting 'economic food dictatorship' has nothing to recommend it.

Besides, it is clear that we have no 'normal prices', or a 'normal market', in our oligopolistic agro- and food-economy. It is not true that 'large-scale industrial agriculture' means 'cheap food for all', simply because the 90 percent of the 'value added' is taken by this system and determines most of the consumer's food prices. In other words, whoever can find a short-cut in the system's 'food chains', will earn so much money that he can easily pay the farmer twice the price that he receives now... In short: both farmer and consumer will benefit when this oligopolistic system gets dismantled.

Only by re-establishing the farmer's professional independence, by allowing him once again to base his farming practices on the local ecological & community resources, the many dangers of the present system to farmer, society and ecology can be diverted. Semi-totalitarian food-power is a dead end, not only for the farmer, but for society at large. In times of relative tranquility it is a dead end for both social-economic and for ecological reasons. In times of crises it can (and will) feed totalitarian economic policies that only fools desire.

9.3. Matters of course

When the US *House Special Committee on Postwar Economic Policy & Planning* in 1946 published its report, the paragraph on post-war agricultural policies didn't even show a trace of the insights gained under the later New Deal. There is no mention of the diversity of farming systems, with their diverse regional needs and possibilities, in the US, in spite of the fact that the existence of this diversity was common knowledge (as it was a decade later – e.g. Parks 1958). There even is no reference to soil conservation and its urgent needs, in spite of the widely known projects of the Soil Conservation Service's (SCS), and some thorough and widely read publications.

For the urgency of soil conservation see Burges 1936, Bennett 1939. For a general as well as a regionally differentiated account of the SCS labours see contributions to *'Soil conservation and good land use'*, Soil Science 64('47)254-370.

The House Committee speaks the same language as the author of the Preface to the 1943-1946 Yearbook of Agriculture. It eliminates all doubts with its opening sentences (of the Summary):

> *'The technological revolution in agriculture has made it possible for the farm family to handle larger farms, increase output per worker greatly, and reduce costs. This trend will continue in the future. For many commodities it has increased supply more rapidly than demand in the United States, and this has resulted in a long-run downward pressure on agricultural prices. An essential need in such cases is to reduce the over-population in agricultural areas so that commercial farm families can operate adequate farm units on a profitable basis'.*
> (House Report No.2728, p.39; cp. also Jesness (ed) 1949, p.240)

This statement implies a denial of the primary importance of the local natural resources and of the community resources for farming and food provision. The *'technological revolution in agriculture'* has taken over. Yet it was only too well known that soil conservation had covered only 10% of the acreage that urgently needed it.
In other words: the House Committee accepted unconditionally that all problems would be solved due to the 'evident' revolution of technology in agriculture. Presenting its policy recommendations based on the 'certainty of technological progress', it refrained from any reference to social and ecological problems, and focussed on price & trade policies instead. In doing so, it turned the projected scale-enlargement & ongoing mechanization of agriculture, the depopulation and community breakdown in rural areas, the intensified losses of natural resources, etc., into a 'technological necessity'.

The many and diverse problems, which had been the subject of research under the later New Deal USDA, were not even mentioned anymore, and so none of them were ever solved. Without any weighing of the pros and cons, the Committee presents a 'one-option-only progress'. There is some logic in it, but only in the sense that it finishes off the mid-war annihilation of the agricultural democracy that had been under construction under the later New Deal. As everywhere – e.g. in the USSR - 'progress' proves a concept easily used by those who want to have their way and who look down on careful research at ground level, close to man and nature. In other words: *'Der Fortschritt entpuppt sich als die Flucht vor der Verantwortung'* (Chargaff 1982 S.130).

And of course, with any and all alternatives denied, there are no brakes anymore on de-ruralization and on urbanization. The decrease in the number of farms & farmers, which had led to many a thoughtful proposal during the New Deal to reach alternative policies in a farmer-, soil- and community-respecting way, was now made a hallmark of 'technological progress'...
From then on real alternatives could only exist within strong communities with a well-developed 'local economy' that was strong enough to withstand aggressive economic intruders. In contrast, rigid individualists among the American farmers, or those who were not members of a strong community, would soon discover the self-perpetuating 'truth' of the Committee's verdict.
The Committee's policies denied any scope also to the (small) farmer- and farm-centred research that was still rather strong at several of the Agricultural Experiment Stations. The government did not even maintain its own laws that would have guaranteed public breeding to have its own place besides commercial breeding. Instead, public breeding was progressively controlled by the big seedsmen. Pretty soon agricultural research in general, and agricultural economy in particular, in the States was reduced to the 'one-option-only' kind. In an acute sense it became a 'mirror image' of politically correct research in the communist bloc, whose experts were similarly convinced of the superiority of their 'free' system...

Carl Sauer 1962 on The weak ecological & social base of US agriculture
Dispersed living, the isolated family farm, in the US had its cause, first, in the fact that ‘*the act of living on the land was part of the process of gaining possession*’, then later, in the practice of the ‘*General Land Survey [that] established the rectangular pattern of land holding and subdivision*’. Characteristic of most villages in Europe is their complex ecological base. Lacking a village history, such a base for agriculture is largely absent in the US. Still, ‘*farm families were part of a larger community*’ and ‘*the country church played a leading part in social communicat-ion*’. Note that ‘*The churches also pioneered higher education, founding colleges and acad-emies across the Mid West, from Ohio to Kansas..*’ - ‘*they did so by coeducation*’. ‘*Country and town were interdependent, of the same way of life… By a tradition that may go back to the town markets of Europe, Saturday was the weekday for coming to town to transact bus-iness .. and to visit*’. It was only with World War I that things changed abruptly. Due to government-promoted intrusion of industry and finance, ‘*Farming became less a way of life and more a highly com-petitive business*’. The late New Deal brought a re-discovery of possibilities, but not yet the renewal of rural life. When then, during WW II, big economic agents seized power, there was not much of a countervailing power that could stop them ‘steamrolling’ the country.

With brakes removed, the ensuing ‘agricultural research system’ soon led to a kind of ‘technological progress’ that flew in the face of agro-ecological reality. Compare e.g. Hardin’s 1952 *‘The effect of technological changes on farm management’* (also in Halcrow (ed) 1955, Ch.6):

> *‘First, let us look at some of the changes taking place in grain production as a result of low relative prices of purchased plant food and advances in mechanization. Under certain price conditions on the more level land it may be cheaper to buy than to raise such plant food nutrients as nitrogen. Some, as Indiana’s Dr.Scarseth, now suggest that soil fertility may eventually be built profitably by cropping to continuous corn. The theoretical framework, he maintains, exists.*
> *....it does suggest experimentation with fertilization through-out a new range of heavy application. For effectiveness, new cultural and application methods may be necessary. ...*
> *Should substantial progress be made in corn and other grain production, it is conceivable that relative supplies of grain feed might increase appreciably. Traditionally Corn Belt farmers have grown rotation grasses and legumes primarily for the complementary relationship to row crops. Their feed value was recognized. But above all we raise rotation grasses and legumes for nitrogen, improved soil tilth, and erosion control – essentially complementary purposes. Should new cultural practices and fertilization techniques decrease the pressure on grasses and legumes for their complementary contribution, we may then grow grasses and legumes primarily for their feed value.*
> *The above two developments in cropping systems in themselves might materially alter our traditional concept of desirable crop rotations. Where previously grains and legumes were complementary on a given soil, they may now become competitive. Feed production patterns might change as a result – thus altering relative prices of different feedstuffs’.*

A faith in ‘technological progress’ is presented here that clashes with all the solid research results of those years also in the US. The need for legume rotations, plus an ample supply of stable manures, was well researched in the US. Several long-term agricultural experiments had proven the loss of soil structure and fertility, which was the result of discarding rotations and manure/compost. The need for traditional, conservative, agricultural practices was especially obvious because the Dust Bowl had given soil conservation research a renewed impetus and this research had established such practices as mandatory.

So when authors like Hardin start believing that 'technological progress' will build soil fertility without rotations, due to the use of large amounts of mineral N-fertilizer, they are culpable of leading agriculture into the realm of virtual reality. The consequences of their disruption of mixed farming - e.g. large scale mechanized growing, farmer displacement, feed lots, and eutrophication - only pushes society deeper into this virtual reality.

9.4. Real-life value

The real-life value of all this 'technological progress' is very questionable. That is soon evident if we, forestalling crass pronouncements about 'feeding a hungry world' for the moment, take a closer look at the real use of the greatly enhanced US corn and soybean yields.

For the yields multiplied (especially of soya, Keddie & Wandel 2001), but nearly exclusively to feed the animals in the bio-industry, which are to provide the 'luxury diet' of an ever more obese population (Rifkin 1992). When soya culture in Brazil likewise was made part of 'agricultural modernization' there, it (a) marginalized mixed cultures with beans (b) soon introduced resistant pests. In other words, it meant an impoverished diet for poor Brazilians (the vast majority of the population; cp.Arrayo 1978, Nelson 1980, Helfland 2001, Torras 2001).

As is widely known, the main product of the high-fertilizer agriculture that is linked with the bio-industry, is liquid manure, which -being produced far from the source of the feed- leaks from the agricultural nutrient cycle in enormous quantities. The 'industrial agriculture' is to blame for the eutrophication of rivers, lakes and seas, with its concommittant loss of fish a.o. high-protein human food. In addition, there is the constant threat of catastrophic poisoning of surface- and drinking water with cyanobacterial toxines.

Eutrophication and cyanobacterial toxins: The high prevalence of cyanobacterial toxins in algal blooms received some research attention in the 1980s and 1990s, but without response from policy makers. For a recent review see: Graneli & Turner (eds) 2006, *Ecology of harmful algae* (e.g. Ch.18: E.Graneli, K.Flynn, *Chemical and physical factors influence toxin content*, and Ch.26: P.M.Gilbert, J.M.Burkholder, *The complex relationships between increases in fertilization of the earth, coastal eutrophication, and proliferation of harmful algal blooms)*. Another review is Preaps & Charette 2003. Cp. also Boyd 2001. Persistence of the toxins in the environment: Jones et al. 1995. Treatment methods for the removal of toxins: Lawton & Robertson 1999, Hitzfield et al. 2000. The toxins are heat-resistant, cyclic peptides (boiling is no solution). Liberated especially when the microorganisms die off, the ensuing flush of organics will even prevent active carbon from absorbing the toxins.

The 'one-option-only' agricultural policies did great harm within agriculture too. For it decimated the nitrogen-fixing and other symbioses that are evident in e.g. many farmers' and wild varieties of corn (Reis et al. 2000, Riggs et al. 2001, Estrada et al. 2002, Dobbelaere et al. 2002). It even made soybean-growing fertilizer dependent. Then the soybean, though a legume, started to deplete soil fertility too. As Smil 1997 (p.141) reminds us of an ancient known fact:

> '*Only when soybeans can derive more than about 80% of their nitrogen need from symbiosis, or when they are planted as green manures, do they bring a major nutritional benefit for the subsequent crop*'.

The cause of this extensive disruption of agriculture is the singular fixation on the high-fertilizer option as the only 'modern' one. A fixation that maroons us on the high-fertilizer island with all our bridges to the wider agricultural reality burnt down.

The worlds of the economy in general and the agricultural economy in particular issued no warnings. The war experiences had proven the need to consider material, energy and labour factors in their own right, in terms of quality and quantity (cp. Ch.1). Monetary aspects played an important, but secondary role in the regulation of the various aspects of the substantive economy in general, and agriculture in particular. But they were neither decisive nor fundamental.
In spite of these experiences, the post-war economy increasingly focussed on costs & prices only. As to the agricultural economy, this meant a complete *neglect of the basics,* when the balances are drawn up. The care for land and animal (labour intensive), the flows of matter & energy, all the diverse aspects of natural resource stewardship, and all the efforts to maintain (and preferably strengthen) environmental and community integrity, were not taken into account. It is puzzling to see even authors like Theodore Schultz thus neglect the basics and, as a result, construct an agricultural economy wearing massive blinkers.

The results of it all are sobering. For now the general 'faith in technological progress', that made the 'flight forward' the hallmark of Modernity, prevented for decades cooperative research into the wider possibilities that 'traditional' agro-ecologies had to offer. It cut the bonds between farmer & farming and the local resources (which were usually freely accessible), and stuck to 'developing' only the high-fertilizer option. And so the **farmer and society got marooned on the high-fertilizer island in a sea of malfunctioning ecologies.** Unfortunately, the main-line economy did little to facilitate an escape: having forgotten the basics, it consistently figured out that the Modern world was the island itself...

It is of prime importance to realize that the post-war years hardly invited balanced approaches. The extreme disruptions caused by the war left man and society without perspectives. So it is at least understandable that, with 'technological progress' viewed as opening up a manageable future, the majority of the 'experts' probably felt great relief, and only a few independent minds were left. In the US from the end of the war, and in Europe after a few years of wider discussion, agricultural economists felt themselves absolved from the task of painstaking introspection, and focussed their attention on 'productivity' and prices instead.

But then, the contributions of those few independent minds were still very much to the point (e.g. Galbraith 1952, Long 1949). Only when the disregard (or outright denial) of authoritative and valuable contributions – from widely different authors like G.Myrdal and K.Polanyi - was continued in post-war decades, the (agricultural) economy got thoroughly petrified. For decades virtual progress and economic tunnel vision were to blame for the puzzling developments in agricultural policy.

Chargaff (1985 S.101) diagnosed *'das völlige Verschwinden der Fähigkeit, die Wirklichkeit zu erkennen'* as a characteristic of post-war American society. Tucker (2000) a.o. gave extensive descriptions of the ecological havoc that the US had caused at home and abroad. Evidently

there was a massive inability to ‘see’ ecological needs - and ecological possibilities. In a profound sense, most efforts were aimed at the construction of a virtual reality.
But of course, policies only define a track if they are institutionalized. And institutionalization in Europe was different from the US. So why was the US example copied in Europe?
From Niehaus’ description (1957 Kap.4) of the 4th Int.Conference of Agric.Science (in Stresa, Italy) it is clear that most European participants still accentuated farmer & farming diversity, but that the idea of the supposed superiority of the ‘progressive farmer’ over the ‘traditional peasant’ was growing at the same time. In spite of the recognition of diversity in Europe, the tunnel vision of ‘progressiveness’ was a bridge-head for the construction of virtual rural reality there as well.

9.5. Bridges to Europe, I: Mansholt

After first shaping the Dutch agricultural policies, Sicco Mansholt became more than anyone else responsible for the formation of agricultural policy in Europe (for his international role see Merriënboer 2006 197f., Jaspers 1991). Mansholt was a highly motivated participant in the Food and Agriculture Organisation of the United Nations, from its very start in ’45. His post-war years as a minister of agriculture in the Netherlands were certainly decisive in shaping his later European agricultural policies. And in those post-war years, FAO was a most important reference point, both for Mansholt and for his nephew, the director-general of food supplies S.L.Louwes (cp. Merriënboer 2006 p.125).

Louwes soon was appointed as a special advisor to FAO’s first director-general, John Boyd Orr from Scotland, who strongly wished to establish a World Food Board with ample competencies and financial means to fight famines in the wold. (His ideals are reminiscent of those of Henry Wallace c.s.). Mansholt and Louwes supported him in his idealistic endeavours (e.g. Mansholt 1947), but the UK and the US declined for both economic and political reasons (they refused to go any further than establishing an International Wheat Treaty). Their refusal was decisive for Boyd Orr’s departure in ’48. Still this phase in history shows how strongly the FAO was imbued with idealism at its start. Without doubt an important reason why Mansholt and Louwes trusted their contacts there. These were the years that agricultural policy was actually involved with ‘feeding a hungry world’, and the derailments of the US’ agriculture were not yet clearly visible. There was the need to feed a hungry world, and there was this accelerated growth of ‘agricultural productivity’ in the US. The link between the two was not provided by logics, but by a tunnel vision.

The probability of such a ‘leap of faith’ was high with Mansholt, who himself had been a rather a-typical Dutch farmer. Mansholt never had to face the requirements of a sustainable agriculture (cp. for personal aspects of Mansholt’s life in what follows e.g. Merriënboer 2006). Though socially alert, neither Mansholt’s period in Indonesia (plantation agriculture), nor his years as a farmer in the new Wieringermeer polder (the prime example of ‘rationalization’ and mechanization of agriculture in the Netherlands), had acquainted him with the quest for sustainable farming that was of decisive importance for most of the world’s farmers.
During his years as a farmer in the Wieringermeer, Mansholt remained a stranger to the fundamental roles of agro-ecologies in sustainable farming. His father and grandfather had been keeping track of the Zuiderzeeworks, of which the Wieringermeer was the first polder (reclaimed land). Yet all the time they had primarily been thinking of large scale, ‘modernized’ agriculture, and had hardly shown any affinity with more ‘traditional’ types. When Mansholt became a farmer in the new Wieringermeer, he saw himself surrounded by a

'rationalized' landscape and standing on a fresh and fertile soil. He was far closer to a typically American 'frontier mentality' than he probably ever realized.

And yet, though his experiences with agriculture were strongly biased, Mansholt must have been aware of marked differences between the various experts in their approach to rural economies and agricultural sciences. It is evident that people like Mansholt, who had worked very hard during the post-war years to provide the bare necessities of life for the people, and subsequently exerted themselves to complete the reconstruction of the war devastations, simply lacked the time and the opportunity to make a thorough study of the different opinions and approaches.
Yet Mansholt must have been aware of the works of renown authors of those years like Boeke or Timmer, and it is significant that he chose to pass them over, even though they were among the best-informed authors, with an excellent international reputation. Surely, FAO was partly to blame for this neglect. But note: it is authors like Boeke and Timmer who presented agriculture and rural economy in all its diversity.

Quite in contrast, Mansholt even neglected the rich pre-war literature on agricultural diversity in the Netherlands. From the start of his term of office, Mansholt stressed 'rationalization', 'specialization', and the like (e.g. Mansholt 1946b). He was so outspoken about these subjects, that representatives started questioning him explicitly about it (Discussion Budget of Agriculture, 1949), but they did not get much of an answer (Memorandum in Reply of Mansholt, 1949). Mansholt continually proved his affinity with the approach that would lead to the small farmer being pushed aside. From the many discussions about the small farmer in the House it is evident that Mansholt felt uneasy about the subject: he doubtless felt his social responsibility towards the small farmer. But nowhere he displays a deeper understanding of his situation and of his potential resources.

In such respects his nephew S.L.Louwes shows more empathy (cp. Krips-van der Laan 1985). His brother, H.D.Louwes, even had a leading role in uniting farmers and farm labourers from very different backgrounds (cp. Geurts 2002). Reading about H.D.Louwes' work in the post-war Foundation for Agriculture (Krajenbrink 2005; Voortman 1996), we see that in a polite way he repeatedly disagreed with Mansholt. It is quite likely that the influence of the Louwes brothers at least caused the policies of Mansholt to be more balanced.
Part of their influence consisted in stressing the importance of a relative self-sufficiency in providing food for the country, as the risks of depending too much on trade were too great (as much of an old Dutch error, as it was a British one). The urgency to grow food locally, and to create a close network of farmers working largely with on-farm means, was only too evident from the war, when S.L.Louwes had been responsible for food provision and distribution. He and others were not slow to stress the point once more during the crisis in Korea.

Yet after '53 this emphasis soon declined. That year was a turning point anyway as to the influence of the brothers Louwes. In that year S.L.Louwes died and H.D.Louwes, because of reorganizations and adverse farmer policies, saw his efforts to unite people suffer a clear set-back. Then very soon we see alternative options disappear completely from the agricultural policies. While De Vries 1948 still pictures the broader scene, there is not even a trace of alternative approaches left in Penders (ed) 1956.

9.6. Bridges to Europe, II: FAO

The years ’45 to ’53 had seen a growing influence of the US on agricultural research & policy in the Netherlands, which became even stronger when the Marshall Plan took effect. From the many field trips that were part of the Plan, more than half of the participants worked in agriculture and in the food industries. The American influence grew largely unnoticed at the time. Boeke and Timmer were among the few that were aware of this trend, but they were not involved in the deliberations. The influences of the FAO and the Marshall Plan soon merged and shaped much of the ensuing policies.

> **The Marshall Plan field trips:** The field trips to the US were all of similar design and administered by the (US) Economic Cooperation Administration. The sending country decided the composition of the teams and their manner of publication, and co-determined e.g. the duration of the trips. We read about the **industrial Teams** from the **UK**:
> ‘*the members … are drawn in equal numbers from the supervisory, the technical and the workshop levels*’ (Specialist Team 1951, cover overleaf). The Team of Metal Finishing Specialists (greatest weight on user aspects) had thirteen members and spent six weeks in the US. ‘*Visits were paid to twenty-one plants, ranging in size from small jobbing shops to plants of the largest manufacturing organisations in the country, and discussions were held with individuals or groups of American specialists on some half-dozen occasions*’ (l.c. p.1). Their report is practical and technical, and includes a chapter on ‘Safety precautions and health preservation’. (A quote: ‘*In general, there is a tendency in American industry to subordinate safety to output*’).
> The **Dutch Team on Poultry Breeding/Farming** is strikingly different (Studiegroep Landbouw 1951). Its five members are from government and big breeders. Dutch poultry in 1950 is still eminently an occupation of the small farmer’s wife, but she is not even mentioned in the report. It derives the ‘importance of poultry farming’ from the 2785 million dollars ‘value added’ after the farmers had their pay of 404 million dollars (l.c. p.27). Farmer and chicken do not figure in their own right, but only as ‘producers’; ecology and community are nowhere. The report has been fashioned by a government-directed ‘Study-group Agriculture’. Its one and only focus is poultry ‘industrialization’.

In retrospect it is clear that the bridgehead for the introduction of (US’) one-option policies in the Netherlands, was constructed during the years ’45-’53. From the Netherlands, Mansholt c.s. would spread the gospel of agricultural progressiveness all over Europe.

Hambidge (1955) has recorded for us what happened during the first ten years of the FAO. It is largely reminiscent of similar accounts of post-war governments in the Netherlands a.o. countries that were devastated during the war: people felt immensely responsible and worked very hard indeed. The feeling of urgency is the more evident if we remember that FAO was, in its formative years, closely related to the UN relief organization UNRRAD.

At its start 44 countries were member of FAO (Russia played the role of observer). The Interim Commission preparing its Constitution (from ’43 on), was truly international, with the three different committees headed by an Indian, a Brazilian and an Iraqi. However, the first FAO General Director was Sir John Orr, the later Lord Boyd-Orr. He probably assigned people from the US and Britain to the top posts for financial reasons in 1945. S.L.Louwes was appointed as a special advisor and (a bit later) A.H.Boerma as an economist.

Orr often made use of working committees with specialists from different countries, and his successor Dodd only intensified this same approach. E.g., Orr sometimes dispatched ‘near natives’ to problem countries.

> The most famous of those ‘near natives’ was the Greek-American ms. Andromache Tsongas, who worked in Greece for three years. The 1947 report of the preparatory FAO mission to Greece is very revealing. It not only describes extensive travels under oftentimes difficult circumstances, but also gives an insight into the diversity of the work done on the spot. Eventually it gave approximately ninety recommendations on a great variety of subjects. But given the very difficult political situation in Greece in those years, it keeps away from most historical, social, and even ecological analyses.
> This mission is quite formative for the ones that follow. From now on missions and reports will be dominated by ‘technical’ aspects, with the ‘technical’ character balanced only by an attention to land tenure problems, that will be a constant focus up till the 60s (several publications especially by Jacobi). Already in the 1950s the ‘technical’ approach is completely dominated by US government policy with its one-option-only of high-external-input agriculture.

And yet in spite of all this diversity and complexity, FAO was essentially organized top-down, from its primal start in Washington.

From ’43 up to its official start, the US was the only country that could offer the facilities for FAO’s preparatory work. When the FAO was started in Washington, there were no farmers from any country in the world among its participants, due to the material needs of those years. And so the question is, if the members saw their activities as preparatory for **a truly global FAO, formed bottom-up**, as befitting a sector that had always been the local co-production of the farmers and the ecology, or as preparatory for **a FAO that would disseminate its supposedly superior information from some ‘progressive’ centre?**

In spite of ‘agrarianism’ being an outspoken characteristic of e.g. Central and Southeastern Europe, and of the small farmer dominating Asian and African societies, the farmer-and-ecology based option was not the choice of the big powers of those years. Even more than the US, the USSR government had demonstrated its hostility to the small farmer. Furthermore, there was a curious unity in US and USSR politics as to their faith in the ‘industrialization’ of agriculture. That faith, by the way, could be called irrational a long time. After all, in the Russia of the first decades of the 20th century Bulgakow (in opposition to Lenin), and next Chayanov, had expounded the fundamental differences between agriculture and industry. Evidently, from the side of politics there was much that worked against the creation of a farmer-representative FAO. And of course, the great agrarian movement of Central and South-Eastern Europe was silenced politically after the war. So, who could have remedied this problematic situation?

Well, there were at least some experts in the field of rural economies in the (former) colonies who had given penetrating analyses of rural societies and agricultural economies in e.g. Asian and African countries. It is quite astonishing that people like Boeke and Furnival were not invited to participate in the FAO deliberations (not even after the war). In a similar vein there were some people, who had done anthropological and related research in the agricultural societies of the colonies for decades, who were not consulted either.

Then, of course, there was the larger group of knowledgeable people who were involved, in some way, with christian missions, who were bypassed as well. This is quite remarkable: remember that most education, by far, in the colonies was initiated by missionaries.
The famous John Mott, who before the war had presided over several big, international congresses, had an encyclopaedic knowledge of the international scene and rural poverty.
And the Dutchman Hendrik Kraemer was both a recognized authority on several subjects at the tangent plane of sociology, cultural anthropology, linguistics and comparative religion, and he was actively involved with missionary work focussing largely on the rural poor.
That missionaries had been most closely involved with the common farmer and with the rural poor was acknowledged by Hamblin in his 1955 overview (p.90f.). And yet, their involvement was evaded (see Selinger 2004 for this same issue in 'development').

And so the start of the FAO was associated with a curious negative selection. Not only were the farmers not represented, but most of those experts, who had an intimate knowledge of the rural population that FAO was supposed to represent, got excluded. Given the fact that higher education before the war was still a prerogative of only a small part of the population, even in western countries, this negative selection meant that on most issues neither the practical nor the theoretical expertise was acquired that FAO badly needed for its start.

How did the 'founding fathers' in fact start the FAO? The report *'**The work of FAO**'*, which, according to Hambidge, *'built much of the **philosophical base for the organization's work**'*, gives us important information (see Hambidge '55 p.54f.). Its philosophy reflects its Washington origins, where the brainstorming & writing were done. A quote will illustrate the well-intentioned nature ánd the limited perspective of the emerging FAO-philosophy:

> *'The most significant fact in the less advanced countries is that in the twentieth century, out of every ten people seldom less than seven are farmers... The industrial revolution and its accompanying agricultural changes have not yet reached these people.*
> *In the technically advanced countries a vast amount is known that can be adapted and applied to revolutionize production in the less developed regions.*
> *There is no reason why a country should not be able, through FAO, to have its whole agricultural system, or any segment of it, surveyed by some of the world's outstanding experts with a view to working out comprehensive plans for improvements and new developments.*
> *The time has come when international organization is urgently needed to accelerate throughout the world the advance of scientific knowledge and its application to human affairs. FAO would fulfil such a function in the great and important area represented by food and agriculture'.*

As the hard working people who wrote the report saw it, the advanced science and technology at their disposal allowed them to solve the problems of the poor farmers and the hungry nations, to a historically unprecedented degree and with record-breaking speed.

Yet, their 'advanced S & T' had no links with sustainable, 'traditional' agriculture. It knew next to nothing about European or Asian, village-based farming systems. It was oblivious in regard to e.g. the soil-building practices of European farmers and their 'anthropo-soils'. With the Mexican peasant, the US neighbour, it perceived only backwardness, where in fact were complex, sustainable farming systems, like the milpa system, for everybody to see. More generally, this 'advanced S & T' was a complete stranger to the agro-forestry systems that, for centuries, had been practiced in Europe, Africa, Asia, and parts of the Americas.

Confident with mechanization, hybrid corn, mineral fertilizer, and pesticides, it paid only cursory attention to soil science, and rarely more than lip service to the recommendations of the US Soil Service. It took no notice at all of the careful research that, in the late New Deal, had focussed on the real (mostly small) farmer in the US itself, or of research of famous US geographers like Carl Sauer. We see its limitless self-confidence exemplified in the volumes of Advances in Agronomy that appeared in the 40s and 50s. It was a Cartesian Agronomy, building certain knowledge from the 'elements' that it discerned so confidently.

Its 'expert', then, lacked the expertise of the 'traditional' farmer. A stranger to the local ecology and culture, he re-cast the world of agriculture into one resembling his laboratory and office. Still, his self-confidence that was contagious, and soon his precepts were also being applied by Europeans (Penders ed 1956 gives us an account of a training course in the kind of top-down extension work FAO tried to push). But it did not take long for the disappointments, caused by this kind of agricultural development and extension, to become manifest.

As to development economics/agricultural development, Seers' 1979 *'The birth, life and death of development economics'* leaves few doubts about the misapprehensions of this branch of economy. For general treatments see Van Geet 1989, Crush 1995, Nederveen Pieterse 2000 and Heft 2 of Helvetica 1999. For a one-time widely influential publication on 'agricultural development' see Wharton Jr. (ed) 1969, but note that also then critical analyses were being published, e.g. Stewart 1971, McGranahan 1972). For some incisive analyses of the dominant approach (of Wharton c.s.) see Lappé & Collins 1977/78 and Nelson 1980. Ikerd 2008 is a volume of assays by a professor of agricultural economics who started re-thinking his subject, after his economic advise from the 70s brought farmers straight into bankruptcy in the 80s. Valuable in making the connection to e.g. production and maintenance of natural resources are Smith & Wishnie 2000 and Berlik et al. 2002.

9.7. FAO experiences

As it was, the 'gospel of productivity', which had its origins in the US, was institutionalized, from the start of FAO, with the stated aim to approach the farmer top-down along the chain of command. Yet another stated aim of FAO for a time prevented its full implementation: FAO's concern about land tenancy conditions offered some counter weight to this 'laboratory approach'.

FAO's focus on tenancy led to an awareness of the general problem, and an involvement with local conditions. Though certainly not its immediate cause, it received much of its impetus from the 1949 field trip of the, then newly appointed, FAO general director H.E.Dodd to Asia. Dodd quickly spotted the impossible tenure terms of many farmers: he had worked with the USDA and had experienced first-hand how this agency had not been able to give small farmer, tenant or share cropper any real security during the 30s. Like others who had moved from the USDA to FAO – especially people from the Bureau of Agricultural Economics whose social a.o. research had been wiped out from '42 to '45– he yet wanted very much to restore their security within the framework of FAO. And so for quite a few years FAO had its *Land Tenure and Settlement Branch* (some late New Deal ideals revived!). Its work was still important in the 60s (see Jacobi 1968) and its attempts to improve the tenancy situation were actually approved by the UN (cp. also Dunham 1982 and refs.). And indeed, in a score of countries tenancy reform was a decisive component of the agricultural productivity rise in the ensuing years.

But of course, the energy spent on tenancy matters by the rather small FAO organisation was no longer available for the development of farmer-centered research. And there were other idealistic endeavours that required the attention of FAO's officers. Dodd tried to revive Boyd Orr's global clearinghouse idea, with his proposal for an International Commodity Clearing House (Staples 2000 p.395f.). It was well thought over: it would have made food purchase by the nations both independent of the politics of the day, and feasible from an economic point of view. Yet it was rejected outright by several western countries (now united in the Cold War), and their governments rejected Dodd for a second term as FAO general director. Food definitely had become an important theme in politics – and that would make a sober evaluation still far more difficult.

In the meantime FAO's growth was especially due to its Technical Assistance Programs. In its first few years its field work had largely been limited to programs financed by the UNRRA, the UN relief and refugee agency. But then from '48 on 'technical assistance' received increasing attention both in the US and in the UN. US president Truman, in an address delivered at the 4th FAO conference in '48, and in his inaugural address in '49, stressed the kind of technical assistance, based on centrally devised science and technology, that FAO had embraced already. Then FAO indeed acquired a large part of the UN Technical Assistance (TA) finances that was made available after the 1950 UN TA Conference (Staples 2000). In the light of its limited finances before that date, this meant a real 'head start' for FAO – but one that implied a near-complete dependence on the politically acceptable notions of science & technology. The projects were screened by and needed approval of the Technical Assistance Board TAB in which the US had a decisive voice! (Cp. UN 1953 in which FAO has to humbly report to TAB).

As Dodd was not eligible for a second term, FAO was prone to lose part of its idealism that had been so striking at the start. Indeed gradually the top-down approach, with its centralized S & T concept, acquired a more central role in its policies. Reading important FAO documents of the 60s, like *'Agriculture and industrialization'* (1967) and the *'Indicative world plan for agricultural development'* (1969), there indeed seems only one option left. In these documents the role of the farmer is referred to as strictly passive and subservient, bound by centrally devised protocols, and the ecology is not mentioned at all.

And so we hear B.R.Sen, the then director general of FAO, in his opening address to the 7th Int.Congress of Soil Science (Wisconsin 1960) speak with unreserved approval about the 'Freeedom from Hunger Campaign' that had started just then (promoted especially by the US). He brims over with enthusiasm about the *'scientific methods of agriculture'* that the West was able to offer to the many *'countries with backward agriculture'*, and announces:

> *'Governments, assisted by FAO and other international organizations, are establishing a variety of institutional services ... to provide agricultural credit, machinery, seeds, fertilizers, and insecticides'* (Sen 1960).

Quite evidently Sen adhered more strictly to the FAO working-philosophy formulated in '45 than any others before him. Nowhere does he mention the local, natural a.o. resources. In his address the farmer is obediently following centrally devised protocols. It is a remarkable stand indeed for the highest FAO officer, for he not only is opposed to (the practices of) nearly all of the people he is supposed to represent, but he squares also with the focus on local resources that is a hallmark of important Sections of this 7th Int. Congress of Soil Science (and its predecessors).

Sen's glowing speech derived not from his scientific knowledge – he was an Indian diplomat – but from devastating experiences in the years 1943-1946 when he, as the Director General of Food for all India, was helpless in the face of the Bengalen famine. It was evident that a clear break had to be made with the past.

Still the road projected squared with many, if not most, research projects of his audience. But then, people who certainly could know better projected this same road (e.g. Kellogg 1960).

As to the discordance with soil research projects,
consider that at the 1954 Congress both the number and quality of the papers in *Soil Chemistry* (Comm.II) and *Soil Biology* (Comm.III) had been favorable compared to those on *Soil fertility and plant nutrition* (Comm. IV) – and of the latter only part had been about industrial fertilizer use. Of the 34 contributions to *Soil Chemistry* none were about the application of mineral nutrients and only some five had an indirect connection with it. Of the 26 contributions to *Soil Biology* most were about the microbiology of specific soils, five were concerned with biological nitrogen fixation, and four focussed on the interactions of microbes with humics (etc). Of the 31 contributions to *Soil fertility and plant nutrition* about half dealt with inorganic fertilizers on a rather superficial level. Only five of the contributions focussed on yields, and none of these was concerned with inorganic nitrogen fertilizer.

9.8. FAO, USDA and soil science

As a matter of fact, already at the First Int.Congress of Soil Science (Washington 1927) there had been a remarkable difference between many of the American and most of the other contributions.

Comparing e.g. the contributions of Lipman and that of MacDowell with those of Europeans like Winogradsky and Rossi & Ricardo we soon learn that there is a world of difference. That these Europeans were indeed the leading researchers in their fields is evident from e.g. Winogradsky 1935 and Rossi et al. 1936.

Most American contributions to the 1927 Congress had been strongly pragmatic, ready to apply whatever promised to increase short-term gains – and leave it at that. The other contributions – including a relatively small number of American ones – were at least trying to be subject-centered and were (therefore) also more self-critical in issues of application. Evidently at the 7th Congress this difference between 'pragmatic' and 'subject-centered' research had not diminished, after more than 30 years.

But some 1927 American contributions were definitely more self critical. A.F.Woods of the Bureau of Soils, for example, stressed the erosion problem and warned (Woods 1927 p.35):

> '*This problem of controlling erosion .. is, I believe, the most vital soil problem we have and on which we are doing the least work*'.

A few years later, his words were borne out by the ecological and social devastations of the Dust Bowl, which have been described so incisively by John Steinbeck in his novel '*The Grapes of Wrath*'.

As to this US problem, Bennett in 1939 gave a detailed account, while Jacks & White 1939 became widely known for their account of the global problem. Bennett, who by 1939 was Chief of the Soil Conservation Service (within the USDA), stressed that e.g. contouring, though often complusory to prevent erosion, was practiced only rarely (cp. also Bennett 1950). One of the reasons for Whittaker 1946, following Sauer 1938, to write about *'this destructive country'* (par.1.4) and to doubt if a real change in practices had been made.

Had Sen and his officers taken more time, they could easily have spotted important differences like the ones indicated. For now, to the better informed members of the 1960 audience, who knew about the immense waste of the US soil resources, Sen's address was very partial indeed – and Kellogg's (USDA) bordered on the ridiculous. After all, his proud announcement of a rise in wheat yields, at the end of the 50s, to 1,5 t/ha (Kellogg 1960 fig.1) was not exactly convincing to e.g. the Dutch participants, for whom 3 t/ha was already a common occurrence at the end of the 20s (before the gift of large amounts of mineral nitrogen fertilizer became customary, Knibbe 2000). Van der Paauw's address at this same Congress must have brought this home to many members of the audience (Van der Paauw 1960 fig.1; he also gave a real insight into the great dependence of yields on meteorological a.o. conditions, something that Kellogg c.s. left out of consideration).
And so to the soil scientists at the 1960 Congress, Sen's and FAO's approaches were far too narrow, and they were too partial in terms of the information considered, to be convincing. More specifically, there were far more freely accessible, natural resources at hand for the (small) farmer than Sen's narrow perspective could show.
If we turn to the 1954 Congress once more, one of the freely accessible resources that is prominently referred to in contributions, is biological nitrogen fixation especially by free-living Azotobacters.
In fact with the 1927 contribution of Winogradsky to the 1st Congress this subject had already come of age. By then the subject had been studied closely for some decades, e.g. in the Netherlands by the famous microbiologist Beijerinck, whose research had become well-known among Dutch agriculturists due to his publications in the 1904 edition of the Landbouwkundig Tijdschrift (Agricultural Journal) (cp. Hissink 1917, pp.89-96, *'The transformation of atmospheric nitrogen in soil'*).

In the 50s researchers like Parker, Quispel and Stevenson review already several decades of solid research in biological nitrogen fixation. Next the remarkable research of e.g. Johanna Ruinen in the Netherlands, and the equally remarkable research of Johanna Döbereiner in Brazil, are widely acknowledged internationally. Yet this exciting research into biological nitrogen fixation receives scant attention in the FAO publications of the 50s and 60s, nor do they play an important role in policy decisions in those years. On the contrary, Döbereiner and Ruinen, with their research, soon are faced with difficult decades.
For more than two decades FAO, as well as the agricultural research and policy in the rich countries, hardly paid any attention to important soil scientific research in general, and into biological nitrogen fixation in particular. So there is something very peculiar about Sen's and MacDowell's enthusiasm (at the 1960 Congress) for industrial fertilizers – that were hardly affordable anyway by to the great majority of (poor) farmers in the world.

Döbereiner was faced with quite some obstruction, but not confronted in her own country (Brazil) with wholesale 'modernization' of agricultural research, as Ruinen, and Anna Post, were in the Netherlands. This Dutch 'modernization' meant the end of the subsidies for Post's research already in mid-60s and of those for Ruinen's research shortly after 1970. Ruinen's truly pioneering research was paid for by the International Biological Program for some years. Yet in spite of the acknowledged level of her research, and although the famous biologist Quispel (Leiden) stressed the importance of biological nitrogen fixation (Quispel 1974), agricultural science and policy turned a deaf ear and research in this important field came to a halt.

Sen’s and MacDowell’s perspective was the main-line US one. In the States, agricultural use of industrial nitrogen fertilizer had more than doubled from ’50 till ’54, and for many people this spectacular rise was proof enough. In the 1957 Yearbook of Agriculture *‘Soils’*, it is only Allison who still stresses the importance of biological nitrogen fixation and expressly warns to limit fertilizer use to *‘a supplement to the other forms of nitrogen to the extent needed’* (Allison 1957 p.94). But pragmatists like J.R.Adams forget all about nitrogen fixation, rotations and manuring, because for them the steep rise in fertilizer application is proof enough of its agricultural power (e.g. Adams 1957). What is completely missing, both in the contemporary Yearbooks and other USDA literature, and in the FAO publications, is a thorough evaluation of this fertilizer use. And yet its huge problems had by then been predicted for years already. Here are just two of them.

That losses were increasing in absolute ánd relative terms, when increasing the nitrogen fertilizer gifts, had been known a long time by then (Bondorff & Petersen 1924). As Black, Nelson & Pritchett wrote in their 1946 publication: *‘These data ... indicate that the efficiency of utilization of nitrogen fertilizer decreases with increasing rates of fertilization’* (l.c. p.396) They report a decrease in the crop’s use of fertilizer from 57% to 28%, a decrease that, coupled with the accompanying dangers of nitrate leaching, would be pointed out again and again.
Arnold 1954 carried out an exemplary research project in which NO release was measured from soils in their field state of aggregation - when fertilized with ammonium and/or nitrate – and proved the great extent of the phenomenon.
But then, USDA and FAO spokesmen evidently did not bother to consult those results in the soil scientific a.o. literature. When we trace the development of their opinions, it soon becomes clear that it was not independent research and evaluation, but simple pragmatism that was their guide. An example of the leading USDA spokesman Kellogg:

Kellogg in his 1947 book *‘The soils that support us’* still explains (p.232): *‘The two most important sources of this element [nitrogen] are barnyard manure and that fixed by bacteria’*, but then a bit later he writes that this is of course not enough *‘for the large amounts of nitrogen’* carried away with *‘our truck crops’*. He does not ask any further questions, nor does he start intensive research. The result is that, a few years later already, his recommends industrial N-fertilizer use rather exclusively (Kellogg 1957). The same can be said about his colleague Hill, who has apparently lost sight of biological nitrogen fixation and soil organic nitrogen delivery (Newman & Hill 1957). In 1964 we find Kellogg and Hill already so accustomed to the high industrial nitrogen fertilizer applications that interest in BNF or in farmyard manure has waned completely.

As indicated, we look in vain for an evaluation of the high rates of mineral nitrogen fertilizer use that were introduced at very high speed, especially in the 50s and the 60s. But one soon perceives something else: for decades there is a evident chasm between well-financed mainline agricultural research, in line with official policy, and independent soil scientific investigation, that is subject-centred and always lacks finances.

The FAO had quite an important voice with policy makers during those decades – not the least because of Orr’s and Dodd’s idealistic endeavours (see e.g. Mansholt 1947). And yet, before long, it led the way without knowing where it was going. In 1960 we saw director-general Sen display the same boundless optimism as the US House Committee in 1946. An

optimism that by then had been embodied by the reckless de-ecologization that was touted as progress (and that as such found e.g. a gullable Hardin in '52).
Only with the Oil Crisis of 1973 FAO officers hesitantly started questioning the wisdom of this policy of the previous decades. But by then the high-fertilizer option was already engrained in about all of the agricultural policies everywhere. Quite decisive was that, as a rule, the FAO staff itself worked within the parameters of the World Bank's lending policies, and these excluded any 'alternative view' (Dunham 1982 Pt.II).

That brings us to an important characteristic of these post-war agricultural policies: the great extent to which they got institutionalized, in laws & policing, in loan & trade policies, in research & extension, etc etc.

9.9. United in Modernization.

World War II had seen a far-reaching regulation of agriculture. In the Netherlands this was, in important respects, a continuation of Depression policies, with bureaucracy and all that goes with it. And so, after the war, that is after about one-and-a-half decade, an extensive bureaucracy was solidly in place. Then because of the difficult post-war years, it obviously had some important tasks in the years of reconstruction.
In the Netherlands there was another and remarkable line of bureaucracy. The Dutch government had committed itself in a very special way to extensive public research and development, in connection with the Zuiderzeeworks and its reclamation of new polders. From the very start most of the experts involved were determined to turn farming in the new polders - reclaimed from the sea since the 1930s - into a model of its kind (e.g. Comm.Bestud. Uitg.Zuiderzeegronden 1930). In a curious way even independent research came to be subservient to that goal.

Important research was done in the reclaimed polders (Zuur 1934, Schreven 1954, Prummel 1954, Veldman 1954), and the new soil was prepared well by the (mostly temporary) state farms for the arrival of the new farmer (Mesu 1954). It was mostly high quality professional labour - and yet hardly anybody thought of linking up with 'traditional' agriculture. The professionals were in a new way 'part of the bureaucracy', yet they were proud of their professional status (van Dissel 1991, Geurts 2002). And so, curiously enough, the 'professionalization' strengthened the growth of centralized bureaucracy in agricultural policy.

After the new polders had been reclaimed from the sea, the landscape and ecology of course had to grow 'from scratch'. In the first years of the polders some truly biological methods were used, to arrive at agriculturally valuable soils. Yet also the first large-scale experiments in landscape construction were then made (as conceived in that way, Com.Bestud.Uitg. Zuiderzeegronden 1930). In a reversal of earlier reconstruction policies (Comm.Wegbepl.1939), it was not an existing landscape and ecology that was 'followed', in the preparation of soil & vegetation, road construction, etc. Instead, some centrally conceived development plan was imposed and landscape and soil were supposed to conform (v.d. Wall & v.d. Bom 1954, Lambert 1971, van Dissel 1991, v.d. Wal 1997).
Within the framework of this 'modern landscape', farming methods were also conceived centrally. What was 'modern' was decided by some central services, and the farmer who applied for the new farms was screened extensively (as was his wife) as to his mastery of the 'modern' methods and approaches. So in this country, in which even the many antropo-soils

reflect centuries of careful local productivity enhancement thanks to farmers' expertise, policy development aspired to be 'modern' from the start, which means, applying a centrally devised approach (see Groenman 1953, Venstra 1955, Constandse 1960; also Vermooten & Vink 1955, Rasch 1955).

The Dutch were proud of their new polders, and indeed earned admiration everywhere from the start (Verschave 1939, Haefs 1940). Unfortunately, this induced policy makers to focus on all of these novel developments, and apply it as a standard measure to the rest of Dutch agriculture. It is puzzling to see how post-war views of the government and of the business world are confined to the promotion of 'modern agriculture', as conceived in the new polders (e.g. Willems et al. 1963).
Within a decade after the war, policy makers in the Netherlands were sure what 'modern agriculture' was, and they were determined to transform all of agriculture after its model. As to agricultural policy they had no questions left... And so the two most widely distributed books, commemorating 10 years of liberation, do not even waste a word on agriculture, in spite of the crucial contribution of the farmers during and after the war (Damsté & Cocheret 1955, van 't Veer & Schrofer 1955).

For decades the IJsselmeerpolders, and next the Deltaworks, attracted the proud attention of the Dutch – but unfortunately they led to decades characterized by a lack of awareness of the loss of landscape and ecology (van der Windt 1995). Somehow these proud public works had become the focus of a general 'endorsement of modernization', both within government bureaucracy and with the public at large. The greatly strengthened bureaucracy, which after the war was closely intertwined, for decades, with officially endorsed (and financed) agricultural research and extension, and with R & D focussing on large-scale public works, was united in its goal of all-out 'modernization'. This mighty bureaucracy derived much of its strength from the fact that the broad public hailed the success of these projects as unadulterated 'progress'. And so, for decades the elimination-by-policy of landscape and agro-ecology was accepted as 'a matter of course'.

9.10. World War II as a turning point

After the war, the Netherlands had a government, for years, in which Labour played a prominent part. Besides following a different political course, the Netherlands differed from the USA in practically every other cultural respect during the first post-war years. And yet the Netherlands soon experienced a similar drive towards 'modernization', a drive closely linked to the de facto extension of government bureaucracy with an array of professional 'services'. This extension was closely connected with experiences during the Depression and the war, as well as with the eye catchers, the IJsselmeerpolder- and the Delta-works. These two cooperated, in a historically contingent, yet powerful way, to make the Netherlands prone to 'modernization' the American way. The story of World War Two's importance for post-war policies will ask our attention in Ch.10. For now we will only describe the radical change in *perception* of agriculture that we find after the war, compared to pre-war years.

Up to the war the Netherlands had been proud of its diversity of agricultural regions. The diverse traditions and practices were proudly recorded in a host of publications (as in Minderhoud 1935, de Vries z.j.(1937?), Heimeijer 1941 and Sneller (red) 1943). They were not only published in popular and scientific reports, but also in government accounts (e.g. Dir.de l'Agric.1937). There is no doubt that government and population both considered the social and ecological diversity of agriculture a very valuable asset, in spite of the numerous problems of those pre-war years. So in spite of the modernization tendencies that were also present then – see the previous paragraph – these tendencies dominated neither popular, nor political, nor (general) scientific opinion.

In the work that, as to agriculture, set the tone immediately after the war, things have changed. Maltha & Oorthuys 1946, a work that only could be published in that year of scarcity owing to extensive government support, agriculture in the new polders is the radiant example for all the rest. In the book, Maltha chose for an evolutionary approach and no more for a historic-geographic one. The great diversity of historic and local practices and implements is re-cast in a linear-evolutionary mould, in which an imagined progress towards modern agriculture (as devised by centralized science & technology) supplants all of the manifest diversity. From the 'history' of the plough in the book we soon learn that we are transferred to a virtual reality (see Casanova 1990 for some real history). It is important to see that its linear re-definition of agricultural history had ***not*** dominated pre-war government publications.

Also in pre-war years norms & standards had evidently been important in connection with trade in agricultural products. Even then there had been the constant temptation to sell products abroad claiming a 'scientifically guaranteed quality'. And yet, diversity was seen as an asset, not as a 'deviation from modernity', and most norms & standards had only an advisory character. So in all probability it was only during World War II that norms and standards, as to agricultural implements, practices & products, became rigidified in inviolable rules, applied with all the force of government behind them. So indeed, there seems to be a decisive political flavour to all of this 'agricultural modernity'.
After the war the enthusiasm for a rich agricultural diversity melted away within a few years, and a uniform picture of 'modern agriculture' took its place. This happened not just because some historically contingent factors had been at work. For it was war-time policies in England and the Netherlands, and in Germany during the whole of the Nazi regime, that erased much of the diversity of the farmer's resources.

Then in post-war years it was a definite choice of Mansholt c.s. to consistently neglect the role of both the small farmer and the agricultural diversity. The many times Mansholt was questioned as to the fate of the small farmer – most of the farmers in the Netherlands were "small farmers" in those years - he hardly ever gave a substantial answer. But his policy decisions led to an always decreasing role for the small farmer.
Equally definite was also the choice of Mansholt and his officials to indeterminately prolong Nazi Ordinances after the war, e.g. such concerning breeding and crop variety use. This in spite of the general repeal of such laws in '45 already. This puzzling choice had far reaching consequences. The most harmful among them were the enormous loss of agricultural biodiversity, and the fact that the farmer lost most of his free assets.

Both these remarkable choices will be reviewed in the next chapter. For now we can conclude that the choice for the "industrial option" limited the agricultural options of the farmer and society. This development, which became typical of post-war policy and opinion, was not just the result of narrow-mindedness, which, due to its human character, is repeated time and time again in history. Instead, the decisive factors were of a far more political and institutional kind, many of them of an international character.

Prominent among these is the close post-war cooperation between the US and Western Germany in conservation of landraces and wild varieties. Waldee, the leader of the US Point Four Programme (that was introduced in Truman's inaugural address in'49, cp. White 1996 p.205 f.), '*personally took an interest in the conservation work of the KWI*' (Pistorius & van Wijk 1999 p.90, referring to Flitner 1995). But surely, it was hardly a secret that e.g. breeders of the Kaiser Wilhelm Institut f. Pflanzenzüchtung cooperating with SS departments had robbed the USSR of many of its extensive collections, so this 'interest' was not just scientific in character, but also an interest in the booty. The joint USDA/KWI expedition, in 1952/53, for the collection of wheat samples in Iran, likely used the information gathered by Vavilov c.s. before the war and now in possession of the KWI.
When then in 1953 the Western German government adopted the Law on the Protection of Varieties and Seeds of Cultivated Plants, offering the breeders the exclusive right to produce and market the seed of their varieties (Pistorius & van Wijk 1999 p.80), this was the 1934 Nazi Ordinance resurrected, complete with its transfer of exclusive breeding power to the big breeders, including the relevant KWI institutes. After that this concentration of seed & plant power followed its grim course: the KWI expedition in 1959 to the Andes was co-financed by the Rockefeller Foundation, the Deutsche Forschungsgemeinschaft, IGFarben, La Roche, and Leverkusen. Big economic powers were determined to rule the world, by way of plant and food.

In regard to the US, this 'will to power' had matured already in wartime and post-war years, in the Rockefeller dealings with Mexico. These dealings amounted to a conscious assault of the community-centred breeding that by then had been developed in that country. Because it is a forgotten piece of history that is quite important to our subject, we turn to it in the last few sections of this chapter.

9.11. Rockefeller re-entering Mexico

As to the roots of the post-war 'industrialization' of agriculture, the decisive importance of the Rockefeller and Ford Foundations' involvement in post-war agricultural research is generally acknowledged. Both as to Europe and the world at large, the Rockefeller research projects were also influential because these were the years of reconstruction and scarcity: there were no finances and so these were the only eye-catching research projects. It was therefore not difficult for these Foundations themselves to be convinced of their leading research role…

Reading Harrar & Wortman 1969, we see that the Foundations trumpeted that they were showing the way to global agricultural progress. Yet their report has no scientific value: there is no consideration of alternatives, no evaluation even of their own research, no indication of the many centuries of peasant expertise with e.g. maize and wheat farming, no account of soil microbiological and ecological factors, etc. Harrar & Wortman are intent to sell their product, not to have it evaluated. Research evidently has been shaped by the big industry perspective. Remember that this perspective, with its commitment to large-scale technology, is the same in capitalism and in communism (or mainline socialism). In the specific setting of those post-war decades of faith in the central role of big-industry-research in achieving 'progress', this perspective was considered a value-neutral one. In spite of its consistent attempts to shift initiative and decision from the 'shop floor' to a distant centre, it was promoted as opening up the 'realm of plenty' for everybody everywhere.

How much the Rockefeller Foundation's spokesmen Harrar and Wortman were children of their time is evident from the title of the volume to which they contributed, '*Overcoming world hunger*'. Nobody in the conference, from which this volume originated, had tried to remind those men of the plain fact that food, the world over, always had been produced by (mostly small) farmers working with local resources. This proves the fact that all of those present at the conference were convinced they were rendering the world a service by supplanting local diversity with a centrally devised 'factory system'. Their effort to 'balance the world on a needle' (of industrial fertilizer) certainly required tight discipline: industrialists, experts and government officials had to work as one man on the huge task. In other words, the institutions founded to tackle this task applied strict discipline, of a peculiarly political kind.

If we think of the singular devotion, in those same decades, of so many able experts, officials and industrialists to land people on the moon, we see some close parallels. For indeed, there was an enormous task that required and received the singular dedication of all people partaking in it. The rude awakening from their dream in effect occurred only with the Challenger disaster. By then the dream proved to have given birth to a self-propagating institutional system that, 'for the good of the cause', had been ready to marginalize professional integrity a.o. qualities that are of paramount importance to both technology and society (Vaughan 1996, Petroski 2006). Yet in the 1960s the task ahead was generally conceived as a big challenge, and so, for quite some time other voices simply were not heard.

As to agricultural policy and research, even up to this day reports like that of Harrar and Wortman are repeated over and over again, in spite of all the evidence against it. Listen to what they say about the Mexican roots of their High Yielding Varieties research (Harrar & Wortman 1969 p.11 f.):

> '*To the leadership of the country it seemed clear that Mexico would need some outside help in solving its agricultural problems. Accordingly, the Rockefeller Foundation was invited to cooperate with Mexican governmental leaders and scientific personnel in a campaign to close the food gap, with particular reference to the country's two basic food cereals – corn and wheat*'.

Maybe Harrar and Wortman had managed, by then, to start believing in this piece of fake history. But it is quite sure that Mexican peasants were of another opinion. Yet, they were not represented at the 1969 conference, or similar conferences where mainline experts and policy makers met each other.

As to the Mexican food situation, corn and beans were the staple food for most of the populace, so any 'closing the food gap' had to start there. Yet, the Rockefeller approach initiated decades of a worsening diet for the common citizen, both in Mexico and elsewhere, because it focussed on grains only, with the growing of beans etc. as protein source for the populace neglected to this day (e.g. Lappé & Collins 1977/78 p.175 f.). The Rockefeller victories, especially that of wheat growing in Sonora, soon invited ecological catastrophe. There was no wisdom in growing a vulnerable mono-variety crop on a man-made island of fertility in a desert environment…

Related problems befell HYV wheat growing in the Punjab. There the social consequences proved enormous too. With the Rockefeller Mexican breeding program at the roots of all 'progressive' post-war breeding the world over, we had better look somewhat more closely at the period of history involved. The rejection of the local rural community, the *ejido*, was at the heart of it.

9.12. Breeding policies

The Rockefeller Foundation gained its position in Mexico only in the chilling war year 1942/43. Before that, Mexico, under president Cardenas from 1934-1940, had seen sweeping agricultural and land reforms. Plus the expropriation of (Rockefeller's) Standard Oil, after this had refused, time and again, to give in to any of the government's conditions.
An essential part of the agricultural reforms was a scientific program of corn improvement starting from farmers varieties and farmers' methods (Yapa 1996 p.81). A quote (Echeverria & Robles 2001 p.155):

> '*It was under his [Cárdenas'] Administration that agricultural genetics research was introduced with a particular approach, the so-called 'agrarian socialism'. Cárdenas thought the common land or* <u>*ejido*</u> *was the basic unit of economic development, so it had to be economically, institutionally, and scientifically encouraged with federal funds. However, subsequent governments did not follow his social policy. They thought private companies had to be protected and strengthened … The economical and institutional support that governments gave to the programs started by Cárdenas was less than that provided to* ***the Programa Agricola Mexicano*** *…recently established (1944) as a cooperative program between the Mexican government and the Rockefeller Foundation*'.

Then Borlaug c.s. soon sidelined the work on rain-fed corn and other crops that were of decisive importance to the Mexican peasant. This was work done by Mexican scientists in the Instituto de Investigationes Agricolas (Inst. of Agricultural Research, IAR). Long time leader of this research was Edmundo Taboada Ramirez, a highly qualified geneticist who wrote the first Mexican textbook of genetics (1938).
Note that his work linked up with that of those Agricultural Experiment Stations (AES) in the US that focussed at cooperative breeding, with the regional farmer, of hybrids adapted to their predominantly organic agriculture. That line of research was disabled by the war Administration, and came to its end when the post-war government, in defiance of US law, no longer upheld the cooperative role of the AES, but shifted 'breeding power' to the big, private breeders instead.

Yet, had it not been for the political shifts in his own country, Ramirez could have managed without such external contacts. Listen to the account of Escheverria & Robles (2001 p.156/7):

> '*the main contributions of the IAR to Mexican agriculture … especially in the 50s, were the so-called 'stabilized varieties' of the local; varieties of maize, hybridized by Taboada himself. The method used by Taboada was based first on getting pure lines with few agronomical defects and good combinational fitness. Second, by means of several crosses and the subsequent assessment of the hybrids obtained, the most efficient were selected and, finally, all possible arrangements between these hybrids were made, producing a genetically balanced population*'.

That means that Ramirez' work was more solid than that of US private breeders. Methods of producing mixtures like those he used have many times been recommended by others also, but most breeders refrained from following the advice, especially because they wanted to maintain secrecy and rejected involvement of the farmer.

Ramirez was specifically working to enhance the relative autonomy of the peasant, as to his local resources. It was this enhancement of relative autonomy that was not welcome to the Rockefeller Foundation, or to its contacts in the post-Cárdenas Mexican government (l.c.):

'*With stabilized varieties of maize, Tabaoda was trying to solve one of the main problems the poorest farmers had to deal with: the acquisition of seeds at the beginning of every sowing time. In this [post-war] period, the Agencia de Investigationes Especiales [Special Research Agency SRA, the Rockefeller Foundation initiated agency] had gotten the farmers used to cultivating hybrid maize because of its greater productivity … However, farmers were constrained to buy seeds every year because if they sowed the sound generation of the hybrids (that is, the produced grain), they segregated and had a very poor productivity … Taboada succeeded in his research, but the stabilized maize he got was not distributed to the farmers as he wished … Taboada's work was shaded, and blocked to a certain extent, by the creation in 1944 of a new institution, the SRA …*'.

When the SRA in 1960 swallowed up the IAR, the work of Taboada c.s. was finally moved out of sight. The result was a deadlock for the poor farmer (l.c. p.160):

'*The view of the post-Cárdenist governments on Mexican agriculture was one formed by private companies managed by well-funded owners; however, it was not a realistic picture of the Mexican farmers situation because most of them were poor and not capable of getting bank credits or making any investment in hybridized seeds and genetic products. In this way, the government policies deepened the standing differences of the rural classes in Mexico; the situation was even worse because the small landholders and natives were unable to take advantage of the 'green revolution' benefits. On the contrary, they were gradually left out of the national economy since they were considered a non-productive sector'.*

Escheverria and Roberts add (l.c.): '*The most important contribution to solve a national problem seems to be that of Tabaoda's stabilized maize*'. Yet, the Rockefeller Foundation insisted on centralized, non-cooperative breeding. With that, it increased marginalization and impoverishment for the peasant, in Mexico and elsewhere on the globe, for half a century. Yet, its ventures were celebrated as the one-and-only way to solve the world food problem.

Still, it has become abundantly clear that at their inception resistance to the Rockefeller policies was strong, both within and without the Foundation. Several of the experts consulted advised against the Mexican policy (Marglin 1996), with Carl Sauer the most prescient of them. Sauer did extensive field research, also on crop origins and biodiversity, both in Mexico and in the Andes (Gade 1999). His letters to a.o. Willits of the Rockfeller Foundation (while on a field trip in South America under a R.F. grant, 1942) have become more widely known thanks to West's publication of research in the Rockefeller Archives (West (ed) 1982). **They leave no doubt at all about the mistaken character of the Rockefeller venture**.

For anthologies of Sauer's publications see Lighly (ed) 1974 and Sauer 1981/1990. For a wider collection see Denevan & Mathewson (eds) 2009.

A note on CYMMIT and IRRI

Those two institutions, that up to a point are products of the Rockefeller/Ford era, have become famous for their breeding research, the CIMMYT (Mexico) in wheat and maize, the IRRI (Philippines) in rice. There is no doubt about the high level that researchers in those institutions seek to maintain. Yet, from the start they focussed on fertilizer-responsive, (semi)dwarf varieties, and selection for 'organic' farming systems was not part of their goals. Although their capital is especially in the huge collections of landraces (12.500 maize landraces held at CIMMYT, Edmeades et al. 1989), the cooperative, farm-based, in-situ maintenance of those varieties is not their policy.

That means, of course, that their advanced research is completely dependent on the accumulated crop variety capital of peasants the world over, and on the organic systems in which they are 'at home', yet, their own research focus and practice militates against maintenance and extension of this capital. We meet that e.g. when they apply mineral fertilizer where in fact organic practices, that also stimulate plant-microorganism associations, are the only logical choice.
Clearly, CIMMYT and IRRI are products of an era that pledged to re-design agriculture the industrial way: their breeding research is still ruled by the 'industrial fertilizer paradigm'. And so the CIMMYT research still reflects that ominous choice at its start: not to study peasant experiences, of centuries, with mixed grain-bean systems, but instead build 'industrial' agriculture from its conceived 'elements', that is, from genetic material, mineral fertilizer, pesticides, etc. Researchers also within those institutes have expressed their doubts about the reductionism that is implicit in this dominant research strategy: it is of little help in re-discovering the expertise of the peasant of old, because that pertains to a systems level that does not simply follow from the 'elements'.

9.13. Breeding violence

When Rockefeller Foundation breeder Borlaugh received the Nobel Prize for his work on wheat in Sonora, the complete neglect of farmers' breeding, local initiatives, and enhancement of local resources, that was the 'negative essence' of his approach, was not even mentioned. But then, is it really a love of mankind that moved the Rockefeller/Ford efforts, when such a denial of life at shop-floor level is a core characteristic? We turn again to Mexico for an answer.

From 1910 to 1920 approximately two million Mexican peasants died in their struggle against a very bloody kind of capitalism that had too many connections with US 'robber barons' of the age (Wolf 1969; Hart 1987). The equivalents of the big industrialists like Tille in Germany and of American robber barons, the *Científicos* around dictator Díaz in the late 19th century preached & practiced ruthless social Darwinism (l.c. p.14), aiming at the obliteration of the Indian element in Mexican society and at 'white' control. Perry gives an overview of their successes (1996 p.66 f.):

> '*Diaz opened the door to foreign investment, which not only served the interests of a few special constituencies but generated a relationship of dependency that had profound consequences for the mass of the population. Indigenous peoples, with minimal power, bore the heaviest burden of these developments*'.

During the Díaz regime

> '*foreign capital helped to rebuild an infrastructure to make the extraction of Mexican resources easier … From 1876 to 1910, railroad tracks grew from a total of 666 kilometers … to 19.280 kilometers. Most of these ran north to the United States as a means of extracting resources'.*

The Rockefeller family was among the primary recipients of the huge profits. As to the peasants,

> '*by the end of the Díaz regime in 1910, 99 percent of rural family heads were landless'.. 'Food shortage became a national crisis, since hundreds of thousand of indios and other campesinos had been driven from their ranchos and ejidos. During [the Díaz regime] haciendas and survey companies acquired at least thirty-eight million hectares of land. Haciendados were producing coffee, cotton, and henequen fiber, rather than corn and beans, and the need for imported food, most of which was beyond the means of the poorest and hungriest, exacerbated the economic tailspin.*

> *The price of corn rose, while domestic production of corn, chillies, and beans declined*'.

During the second decade of the 20th century, Mexico was rocked by a brutal civil war, brutal especially, because the ruling classes were still pervaded by the spirit of social Darwinist 'science' with its genocidal intent. Indeed, the two million dead prove their blind pursuit of racist (biological determinist) doctrines. Yet, even after the civil war it took one-and-a-half decade more before reform really got a chance: only Cárdenas' policies from 1935 on did initiate massive agricultural reforms.

So it was hardly a sign of sensitivity of the Rockefeller Foundation to help Camacho, the next Mexican president, in reverting these policies and displacing the peasant again. For indeed, it did just that, by authorizing a form of breeding that essentially consisted in cutting off the peasant's access to his breeding resources. Centralization and standardization, in addition to the imposition of the breeders' law, were among the institutional factors employed in this cutting off access. Soon those peasants that had not stuck to their landraces were changed into powerless dependents.
As indicated, the Rockefeller Foundation was warned in time by the capable geographer Carl Sauer. He stressed that agricultural and nutritional practices of the Mexican peasantry were quite sound, but that they needed support and strengthening (cp. also Jennings 1998, as based on research in the Rockefeller archives). **Sauer recommended the cooperative, Mexican agricultural research initiated under Cárdenas**. The Rockefeller Foundation did not follow Sauer's advice, and, instead of promoting agricultural development, supplanted farmer-centred agriculture with a big-power-centred version. The parallels with the policies, of those years, in the US itself are only too evident.

The Mexican and the US example make it conceivable that the 'industrialization' and 'globalization' of agriculture was primarily achieved because a (politically) powerful centre managed to cut the small farmer off from his resources. A kind of 'scientific progress' was attained that was essentially negative, relying as it did on a concept of the world of farming that had been 'emptied of farmer and ecology'. Displacement by law, of the farmer, then followed in its wake.

The US is at the base of global 'agricultural modernization', in post-war decades. In a general way, because financial and industrial concentration had made a massive re-start during the war, and big economic powers now were in a central position. More specifically, because those powers effected the farmer's loss of local resources, and the change-over to their industrial resources as a substitute. If not for this seizure of power, the American farmer could have trod his own path, and it is even likely that US agricultural research would have re-discovered the diversity of farming systems, with their local resources (note that Carl Sauer had shown the way already).
Yet, there is this seizure of power, and already during the war it reached Mexico. In regard to breeding, it empowered an approach that not only neglected the farmer and his local resources, but also cut off the farmer from those resources. As a result, HYV breeding (Borlaug c.s.) was completely 'out of balance', and its constructs were conceived in disjunction from farmer and local ecology. Even where research was not under the direction of government or industry, as in CIMMYT and IRRI, this greatly unbalanced approach left researchers without the means that would allow them the change-over to farmer- and ecology-centred breeding.

Still, those American power concentrations in themselves were not able to bridge the distance to agriculture in Europe or other continents. That bridges had to be constructed largely from the other side – and they were. After the war, High Modernity became the unifying credo of governments, all over the globe. Centralization of research and planning would surely allow the reconstruction of nature and society!
Yet, before the war Europe displayed a great diversity of agricultural regions and systems, with a vitality that did not derive from e.g. industrial supplies. As a matter of fact, in many countries, also in Europe, agrarianism was prominent, and it was not even likely that bridges to any American power concentration would be constructed.

Then World War II came, and history took a course that was unexpected. The least so, maybe, in England, that had done so much to distort its rural regions and agriculture (see e.g. Mingay 1990). Its Oxford-based agricultural economists displayed a near-complete lack of knowledge of ‘traditional’ agriculture; when these experts received near-absolute power during the war, ‘traditional’ farmers faced a hard time.

Yet, circumstances elsewhere in Europe differed strongly from those in the UK, and we are left with the gap between the pre-war diversity, that often was a cause of pride to governments, and post-war ‘modernization’, that decided this diversity was fit for the dustbin. A primary example of this sudden change is the Netherlands, that after the war would soon become a bridge-head connecting the US and Europe. Nobody did more for the introduction of ‘industrial’ agriculture, after its US example, in Europe at large, with the formation of the Common Agricultural Policy (CAP), than the Dutch post-war minister of agriculture, Mansholt, with his staff.

> The major expression of Mansholt’s policies is the ‘Mansholt Report’, COM(68)**1000**. It is **completely oblivious in regard to ecology, ruralities, and local resources**, but so are the studies surrounding it, e.g. van Riemsdijk 1960, Kriellaars 1965, LEI 1965, van Lierde 1967, ‘t Hooft-Welvaars et al. 1970, Maris 1971. Only after emphatic denials the human and ecological problems received gradual recognition (e.g. Schaap 1983). But in post-war years, the bureaucracy displayed a complete lack of awareness of agro-ecologies, ruralities, natural resources (e.g. Platenburg 1948; the same was true of Wansink 1956, who represents ‘modern’ farming). This ‘ecological vacuum’ tells us much about the origins of ‘industrial’ agriculture in Europe, and of the Common Agricultural Policy.

Mansholt shaped the agricultural research, as well as legal instruments (e.g. Re-allotment Law and Breeders Law), that empowered him to effect the change-over to ‘industrial’ agriculture. Those means were only very partially in existence before the war, and became prominent only during the war. The story of those sudden changes is the subject of the next chapter.

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10.

The never-ending war

and the small farmer

Why focus at the war?

With post-war changes in farming a true avalanche, it stands to reason to look at least for some roots of it all in the war. Yet research into lasting influences of World War II in the post-war decades was, quite generally, a non-item up till now. This although it is completely clear that e.g. the Dutch Government of State in the 19th century cannot be studied but from its roots in the Napoleontic years (Pfeill 1998, Preface). Those years of what was, in plain fact, the first world war, saw severe interference at village level also, e.g. those around taxes on the milling of food grains. This institution shifted not only the tax burden disproportionally to the poor, but it also drove a compulsory monetarization of what in important aspects had been an exchange-economy, and forced the miller to give up any other economic activities (l.c. p.451 f.). In other words, the increasing total-war character of those Napoleontic years translated into totalitarian interference with the life of the common people and their institut-ions, with results that were to shape life for decades after the war (and even up till the present).
Now there is no doubt about the total-war character of World War II, so we simply cannot refrain from looking closely at its severe interferences with rural life and the agricultural economy. The more so because WW II is the sign everywhere for policies that, in effect, will drive peasant and small farmer from the land. Note that the powers of those war years, the party bosses in the USSR, the industry and finance bosses in the US, and the Nazi leaders in Germany, in a way were united in their contempt of the peasant. How did they interfere in the life of peasant and small farmer especially, and in what ways did that interference shade over into post-war years?
To look for lasting influences means to search for institutional and legal factors first of all. But even the broad subject of economic collaboration resurfaced only very recently. Yet, looking into the matter, it soon becomes apparent that not a few wartime measures had a lasting influence in post-war decades. That was true even in the US, e.g. with big enterprise being financed lavishly for war production, while small enterprise even saw the materials refused that it needed for its continuance.
Redress of war-time injustices has been very partial, at best, so much is known also from other examples. Is it conceivable that this victimized specific strata of society, like the small farmer and his communities, e.g. under the pretence of the primacy of reconstruction? If so, did such refusal of redress find its way in law?
At the start of the present part of research I knew of the Breeders Decree, with its exclusion of farmer and farmers' varieties, under occupation rule in '41/'42. But how was it that an occupation decree could be prolonged in post-war years, next even to become the very core of post-war law?
So at the start of this part of research I had plenty of questions, yet I did not know where they would lead me. All I knew was that careful research would be worthwhile.

10.1. Introduction and method

The Netherlands in 1950 was still a country of small farmers: about a third of them was farming 1-5 ha, and another third 5-10 ha. But then, within a few years, most of them would disappear – wíth their communities and practices, and with their agro-ecologies and rural landscapes.
Landscapes do not just evaporate. In some countries depopulation of the countryside initiated its deterioration. But not so in the Netherlands: there landscapes got forcibly erased with the large-scale land consolidation projects of the 50s-70s. After these projects had been steamrolling the country, the great variety of agricultural regions in the Netherlands was largely a thing of the past, as was the accompanying variety of local agro-ecologies.

In these same decades, together with the disintegration of rural communities, local modes of production and distribution disappeared as well. Not only that, but crop diversity disappeared too, as did the great number of farmer's varieties for each crop. And all of this precipitous loss of socio-economic and bio-diversity was government-directed.
Within a few decades the small farm saw its local resources erased and, as a result, its existence denied. Only recently we discovered that this all-out loss of diversity had led us into a dead end. After half a century of all-out modernization, primary production had lost its base in socio-economics and in biodiversity.

Yet, it all started with this disproportionate interference with the small farm & farmer, as exemplified by the land consolidation projects. If we trace the roots of this massive bureaucratic-technocratic interference, we find some of it in the Depression years, but most of it, by far, during the war and in post-war years. It was the war that brought a totalitarian economy directed from the centre. Next, the post-war years saw - especially as to agriculture – its continuation in the directed economy of those years.
There is a correlation between our all-out agricultural modernization policies and those extremely government-controlled war- and postwar-years. Yet, it is a correlation that till now received scant attention. If one wants to research the causes of the disappearance of the small farmers and local ecologies, one cannot just pursue specific issues, but one will also have to research the framework around these issues. My research required some ground-breaking and many time-consuming efforts, but because of the sheer abundance of non-researched subjects, I had to limit myself to exemplary investigations.
Still that sufficed to give me an insight into the connections between the (post)war regime and the consecutive all-out 'modernization' of agriculture. With it the question arose if maybe the dead end indicated for agriculture and food provision, was a self-inflicted fate. If so, then the question is reversible: will it really be so difficult to restore the socio-economics and biodiversity at the heart of agriculture, that we need for sustainability? In short, **why not re-instate the peasant/small farmer?**

But of course, that suggests a 'small & slow' society. Untill recently, that was completely 'unthinkable'. But then, with the Recession, our economic 'certainties' evaporated, and now we are in for some thorough going re-considerations. We start with a rather fundamental one.

Le mobile du savant

For anybody who has read some of Michael Faraday's reports it is evident that scientific research publications need not be dry. Yet because only a few scientists are good writers and

many editors tolerate abstruse language, most scientific publications are not exactly a literary delight. Faraday, Chargaff and some others are exceptions in this respect.
It is broadly acknowledged that a writer, or a poet, can accomplish things that a researcher cannot. Dostojewski or Tolkien, to mention two widely different authors, open up a world that research is hardly able to enter, a world that is emphatically not 'just fantasy', as even most scientists are ready to admit. So, we had better take a good look at such writers when we are confronted with either superb beauty or fathomless evil.

Surely **beauty** is important, for scientists and science no less than for the rest of humanity and its endeavours (read Pascal or Soloviev). A quote from Simone Weil (1949 p.329):

> '*L'esprit de vérité peut résider dans la science à la condition que le mobile du savant soit l'amour de l'objet qui est la matière de son étude... La vraie définition de la science, c'est qu'elle est l'étude de la beauté du monde*'.

As to fathomless **evil**, a 'scientific' approach is hopelessly inadequate (e.g. statistical reports about Auschwitz inmates). Tolkien did better when writing about Saruman - and by placing the humble Sam and Frodo at the heart of his story. As to the latter Simone Weil helps us understand why (l.c. p.287):

> '*Le seul châtiment capable de punir Hitler et de détourner de son exemple les petits garcons assoifés de grandeur des siècles à venir, c'est une transformation si totale du sens de la grandeur qu'il en soit exclu*'.

And she continues (I now follow the 1952/78 English translation, p.217):

> '*It is chimerical...to imagine that one can exclude Hitler from the title to greatness without a total transformation, among the men of today, of the idea and significance of greatness. And in order to be able to contribute towards such a transformation, one must have accomplished it in oneself.....This is far from being an easy matter, for a social pressure as heavy and enveloping as the surrounding atmosphere stands opposed thereto*'.

I gather from this, that the scientific study of World War II and its aftermath is only then really possible when we are prepared to strip it of **dominant notions of 'greatness'**. What constituted the 'power of the weak' that made many of them withstand the Nazis, and why did so many of the 'great' end up in collaborating with them? These are undeniably guiding questions (if only in making us aware of our own shortcomings in accomplishing the transformation indicated).
Now if '*le mobile du savant soit l'amour de l'objet qui est la matière de son étude*', this definitely refers to methodology (see Scarry 2000 for a sequel). But the pretence of neutrality is a fraud, especially in this context of a horrible war.

> Dintenfass (2000 p.20) points to the need '*to accept the prominence of the Holocaust in historiographical controversy … as irrefutable evidence of the centrality of questions of good and evil to the historical enterprise and to begin to consider the study of the past as a project of the should and the ought as well as the did and the was*'. Note that Dintenfass c.s. is at one with great historians of a former generation like Huizinga.

This is also true for the pretence of superiority (refer again to our shortcomings in the transformation needed). But focussing, time and again, on 'little people' can be a great help (as we learn from Tolkien). The more so because the Netherlands was a country of predominantly 'little people' (see Gellhorn 1944 for a typical example). Most of them, by far, were convinced that convictions were part of life as well as of science, and rejected pretensions of 'value neutrality'.

Showing one's colours

A characteristic of Dutch society in the years around World War II was its compartmentalisation ('pillarization' is the Dutch expression). In post-war analyses the concept became rather empty of meaning, often used only to indicate a presumably rather inflexible past in which Dutch society was still on its way from tradition to modernity.
Yet, this compartmentalisation was part of a rather colourful society, and we need to understand why common people experienced it as a valuable aspect of life. Without some such understanding our analysis will tell us little about Dutch society in wartime. Instead, it will merely reflect the a-historic character of post-war High-Modernism.
When investigating wartime society in the Netherlands, we cannot do without the concept and its background notions, because it expresses something of the conceptual and emotional 'home' of the people in those years. It says something about the base from which they tried to evaluate their stance about the Nazis and, because of its communal character, also the base from which action was generated. Through the years, several historians shared this view, and yet, the concept in public discourse acquired largely negative connotations.

It is essential to notice that also critics of compartmentalisation around the war– prominent figures like Banning, Kohnstamm, Kraemer, Buskes, and Van Eijck among them – shared some decisive background notions with its defendants. Note that even the creation of Dutch economic models at its origins was part of this compartmentalised society (van den Bogaard 1999). Van Cleeff, who stood at its cradle,

> '*accepted the different ideologies and tried to combine them in such a way that they remained visible. .. Van Cleef tried to integrate the pillars while Tinbergen tried to exclude them*' (l.c. p.342).

In Van Cleef's own words (l.c. p.343):

> '*Underlying this [system of equations] is ultimately man as the cause of movement – or, more precisely, the economic subject – or groups of people... Wouldn't it be useful for once to describe in a survey a specifically "literary" image of the people, or groups of people, on whom the model-builders based their assumptions? It would be useful to know if the mainly qualitative image, strictly following the model, of the economic subjects that would result from it seem to be realistic. If so, then the probability that we do good work increases. If not, then this gives cause to correct the model*'.

Dalmulder's 1937 study '*On econometrics*' (no.19 in the series of the Netherlands Economic Institute) is completely situated *within* this frame delineated by Van Cleef: it is econometrics of a high level, and at the same time tries hard to be philosophically explicit using a Thomist analysis!
After the war, Van Cleeff wanted people to direct the economy on the basis of their convictions, even though he was critical of the rigidities of compartmentalisation in pre-war Dutch society (in giving priority to conviction he was at one with the critics mentioned already). It was Tinbergen who wanted an 'objective' economic model that excluded 'subjectivities', with a bureaucracy that in post-war years expected just that from him. In the end that left us with intricate models that were not transparent at all as to the assumptions from which they started or the extremely narrow conditions under which they could be validly applied.

Bureaucracy apart, right after the war there were many more people who adhered to such conviction-based policy determination. We see it e.g. defended by the Christian trade union CNV (Stapelkamp 1945), in no uncertain terms. This union, that was composed of labourers from widely different churches, did not reject cooperation with other unions, yet had its own, positive goals. Notably – and this is of central importantce to our subject – like others it endorsed the Dooyeweerd 1936 analysis of place and task of a union (when a few years later Ruppert (CNV) clashes with Dooyeweerd it is on a completely other subject). Dooyeweerd rejected the ‘Ordnungs’ approach that was then current in Germany, and that in fact had induced cooperation with the Nazi regime. Rejecting this approach before the war already, the Christian unions were prepared to resist the Nazi demand of ‘unification’. Hardly strange that after the war they were not willing to as yet welcome a ‘unified’ labour union.

In fact, it is the rise of ‘expert-based policies’ with its ‘neutrality’ that is in need of an explanation, not the ongoing presence of ‘conviction-based policies’.

> At a more theoretical level, Polak in 1947 still presents a valuable overview and discussion of the issues of value and ‘neutrality’ in social science (focussing at the German discourse before the Nazi era – names like Weber, Scheler, Mannheim, Grünbaum - cp. Grünwald 1934 for a good pre-war discussion). His thesis is still linked with e.g. Dooyeweerd 1938 and Sassen 1941, philosophical publications that illustrate that there is ample scope for value orientations in science. Huizinga 1946 is a widely known example of a leading academic who, from the experience of World War II, stresses the need for value orientations, more still than he did before the war (in e.g. Huizinga 1935).
> Yet, within a few years we see profound changes, and social scientists like Hofstee become convinced that Modernisation with the help of ‘neutral’ science is the true road to Progress. There is an ideology in the air that, for a time, even brings critical minds like Dippel to ambivalent statements (e.g. Dippel 1951; by then Polak has turned into an outspoken proponent of this ideology). For some time other authors, like Oldenburg (1965), evidently are not themselves proponents of ‘value-neutral’ social science, yet, they turn increasingly to American social science that, like a Torjan horse, brings it into their publications. In the end ‘modernized’ social science fails to link up with the ongoing reflection about the theme of values/’neutrality’, as this has born fruit by then in e.g. Rosenstock-Huessy’s ‘Soziologie’ (1956). Just one quote (l.c. S.54): ‘*Beteiligung und Mitleidenschaft des Soziologen, sein leidender Eintritt in die Pathologie des Falles als Teil des Falles, ist der entscheidende Schritt zur Vergegenwärtigung dessen, was fehlt. Erst hinter diesem Mitleben her eröffnen sich auch Erkenntnisse*’.

‘Expert-based policies’ that pretend to start from neutral knowledge leave no room for diversity. Indeed, it seems likely that there is some close relationship between the loss of biotic diversity under High Modernism, and its loss of human & communal diversity. Note that High Modernity only recognizes a black-and-white world (‘tradition’ vs ‘modernity’, with a ‘modern society’ inhabited by people who are driven by its ‘rational-economic’ motives). It has no concepts for the colourful world inhabited by ordinary people. Yet, it is an understanding of those ordinary people that we need in our investigation of the influences of war.

War and occupation can very well have been the years that gave a previously unknown impetus to the post-war eradication of human and ecological diversity, an eradication that has no parallel in history. Very soon after the war, government-initiated economic growth, with its devastation of local communities & ecologies, gains momentum everywhere. But then its motor, the choice for all-out modernization, must have been in place already, that is, it must have been institutionalized and legalized to a considerable extent.
It goes without saying that totalitarian systems have always been enemies of human diversity, and we experienced the worst example in history. Still we are not used to considering the connections between the war era and the post-war decades of growth at the expense of diversity. Yet, we will see that e.g. land consolidation and the introduction of breeders’ law in the Netherlands illustrate the existence of such connections.

'Showing one's colours' was a characteristic of large segments of Dutch society with which they entered the occupation years. It was a characteristic that, as such, had the approval of many of its leading intellectuals. In our sociological investigation of wartime influences in the post-war era we need this concept of 'colours', both to understand the behavior of the common (wo)man in wartime, and to probe behind others' pretense of 'neutral expertise'.

10.2. An unthinkable war

At the outset the complete contrast between the Second World War's devastations and the general lack of research into its lasting influence on post-war institutions & policies should be considered. Most studies pass over the war as an unhappy incident that just held up the normal course of things, or as the start of a new era thanks to the America-inspired 'modernization'. Now such a stance is hardly rational, as is readily apparent.

The Second World War on the European continent was a totalitarian one also as to its death toll (Judt 2005 p.17/8):

> *'It is estimated that about thirty-six and a half milion Europeans died between 1939 and 1945 from war-related causes (equivalent to the total population of France at the outbreak of war) – a number that does not include deaths from natural causes in those years, nor any estimate of the numbers of children not conceived or born then or later because of the war'*

This death toll was highest, by far, in Eastern and Central Europe, with Russia leading numerically - Judt gives more than 10 million dead - and Poland proportionally - one inhabitant in five a victim of the war. Without any doubt, Judt's estimates are conservative: another professional historian gives e.g. 20 million casualties in the Soviet Union (Fox 2004 p.423).
So there is no doubt that the bigger part of the German army was fighting its ruthless war at the Eastern front for years at a stretch. Yet, western European stories of the war more or less shrug off the horrors experienced in Eastern and Central European countries. With the Iron Curtain thus soon closed as much by the West as by the East, one wonders if *both* sides have not been busy re-writing history.

There is no denying that the totalitarian character of World War II wás indescribable. Just enumerating some of its 'facts' does not do justice to the people suffering its horrors and is of limited help in fathoming its evil. And it hardly informs us about the attitude of the majority that saw its uneasy expectations shattered: the expectation that some compromise with this evil ideology would be functional in preventing things becoming worse.

There is much in this war that can only be described by using apocalyptic language, not the language of 'facts'. There was an extreme *Ver-nicht-ung* at the heart of this totalitarian regime and its war: its annihilation of people was an attempt to crush the essence of humanity. In a profound sense it is and was impossible 'to come at terms' with this apocalyptic past.
But then, the way the 'free world' tried to settle the account was not exactly laudable or profound. Even the Nürnberger trials lost much of their integrity when both West and East were proud to have 'caught the criminals' and stopped short of self-scrutiny. By then everywhere in Europe history had largely been covered-up by myth-making. Just some examples.

In the Netherlands by then, high officials of the (state) railroads NS who had been directly responsible for transport of the Jews to the concentration camps had been exempted form prosecution (Romijn 1989 p.115; on the subject see e.g. de Jong 1975 p.38-43). But then, it was becoming only too evident that their behaviour had not been an exception: the good intentions of many a high official had chiefly effected a more direct entrance into the Nazi hell - for the Jews first of all.
For sure, these railroad officials were not alone. Take secretary-general Spitzen of Transport. Different from many colleagues he had been trýing at least to operate as autonomously with his Department as was possible in the war situation. Yet, as to the fate of the Jews, he was co-responsible: he was the highest Dutch official in the over-all field. Years later he apparently suffered from loss of memory when the Parliamentary Enquiry touched on the matter (Enq.Comm.1947-55 p.84). Quite likely the subject was too painful: his ‘ethics for ordinary times’ had failed miserably here, when confronted with Nazism. What had happened was too frightful to remember.

As it was, in all of Europe the post-war Hungarian trial and execution of three high officials of the Ministry of the Interior who had been largely responsible for the deportation of Hungarian Jews remained a solitary exception. Even though in most countries high officials had similarly collaborated in exterminating the Jews, after the war Reconstruction only too soon implied a cover-up nearly everywhere. In plain fact only in a few countries, like Denmark and Finland, high officials had protected their Jews.

10.3. The impotence of neutralism

But then, remember high officials in most countries *before* the war had found some way – they thought – to get along with Hitler’s Germany. They had mentally separated Nazism from e.g. Germany’s economic transactions, as the cordial relations of high officials everywhere with Germany’s economic elite exemplify. It was decidedly not just a matter of formal relations with e.g. minister of economics Schacht (Hirschfeld in the Netherlands was a prime example – see Hirschfeld 1998). In addition there are the close relations that big businessmen like Henry Ford had with both their counterparts in Germany and with many high officials. The clear warnings as to the all-encompassing character of Nazism, issued by many a well-informed person in the West, as well as by many international organisations, were brushed aside. Even though there could hardly be any doubt about the truth of the information: in the Netherlands the analyses by top level academics (Huizinga and Kohnstamm among them) as well as by leading socialists and theologians, had already clearly indicated the futility of compromise.
As it was, Hitler’s staff found many (if not most) high officials in Europe quite ready to ‘cooperate’ starting from the assumption of comparative neutrality of their field of expertise (e.g. economics, transport). Strongly inclined to consider as non-experts all those participants in their field who spoke about values and presuppositions, they were hardly prepared for the confrontation with this totalitarian system.
They could have known, from the many warnings issued before the war, that such preparation was urgently needed. But instead they derived comfort from the thought that many of their German colleagues were reasonable men, if only one was able to penetrate the layer of Nazi propaganda. But the judgments of their German colleagues were based on ‘*einem durch die öffentliche Meinung geknebelten Erkennen von Gut und Böse*’ (Bonhoeffer 1932, as cited by Heer 2001 S.1091 n.105): the German public opinion for decades had been dominated by ever growing social-darwinist concepts. And not only that (for that was true also for many other

countries) but since ’33 Germany had been rúled by a horrific synthesis of such concepts and practices.

Then with the success of Nazi war in Western Europe in 1940 Bonhoeffer was forced to conclude (l.c. S.1089):

> *‘Grundsätzliche Änderung im Volk ist erfolgt, geschichliches Ja gesprochen zum Nationalsozialismus, Meinungsäusserung weiter Kreise, liberale demokratische Welt zu Grunde gegangen, für Partei sichtbarer Erfolg’.*

The ‘reasonableness’ of liberal democracy, though a dominant part of pre-war officialdom in Europe, was of no avail anymore. Pretending it was, quite general among high officials and industrialists, led to actions based on assumptions that were powerless in withstanding the totalitarian evil. Much of it had become apparent already in pre-war Nazi Germany. Kobrak & Schneider indicate (2004 p.151):

> *‘One of the paradoxes of the Third Reich is the degree to which, in many areas, it preserved the trappings of normality in a perverted form to manipulate key figures and erode resistance to its ultimate ends’.*

But then, the ‘man of conscience’ had always been far more dependent on his not-too-unreasonable environment than he had been willing to admit. Without it he made a poor show:

> *‘Die unzähligen ehrbaren und verführerischen Verkleidungen, in denen das Böse sich ihm nähert, machen sein Gewissen ängstlich und unsicher, bis er sich schliesslich damit begnügt, statt eines guten ein salviertes Gewissen zu haben, bis er also sein eigenes Gewissen belügt, um nicht zu verzweifeln’.* (Bonhoeffer, l.c. S.1091)
> *‘Es herrschte eine Dämonie, die wir nicht begriffen. Unsere ethischen Maßstäbe, mit denen wir aufgewachsen waren, reichten an sie nicht heran. Wir blickten in Ab-gründe und gerieten, bewußt oder unbewußt, in sie hinein’* (von Weizsäcker 1996 S.11).

Eventually, Hitler found too many people in high office totally unprepared, sooner or later willing to belie their own conscience. In the Netherlands for example most High Court judges, whose rulings were outright unconstitutional even at decisive moments and by that were a big help to the Nazis (Belinfante 1978, Romijn 1989, Michielse 2004). To exempt such judges and officials from prosecution after the war flew squarely in the face of the rulings of the QueenWilhelmina’s London ‘44/’45 cabinet and her clear advice to the post-war cabinets (Fasseur 1995). And yet such exemptions became the rule – as it did everywhere in Europe. But then, not only high officials, but big parts of the population also did not dare to take an honest look at their recent past. When myths were constructed with which to repress the deeply disturbing memories, many, if not most people welcomed them.

Still in post-war years a lot of material was prepared and collected that could have helped in the start of a thorough evaluation of the broad and deep influence of the war years. There wére serious efforts in the rehabilitation of people who had suffered injustice under the Nazis. In the end, the Council of Rehabilitation in the Netherlands, in its Departments, had produced more than 200.000 files (de Jong 1988 p.657f.). That they were part of positive justice is clear from the fact that Gerbrandy and Cleveringa dominated the Council’s activities: two exceptional figures whose uncompromising attitude during the war was widely known.

Yet also those men could only administer justice where things were somehow clear-cut. But what to do when the products of the Nazi administration remained in force after the war (or were soon put in force again) – as was the case with many war-time regulations on Food & Agriculture? As we shall see, official efforts to ‘clean up’ law from Nazi remnants failed in important respects. And surely, with high officials exempted from prosecution and their subordinates no less eager to forget about a dubious past, an investigation of their common war-time administration as to its products was not to be expected.

Indeed, up to the present day the files collected by the Council of Rehabilitation remain unresearched, just as the contents of many other war-related archives. And so for decades the myths have prevailed and the long-lasting influence of the war on post-war society has remained a practically un-researched subject.

10.4. Myth making

It stands to reason that every European country had its own specific post-war track. As to the Netherlands, it was somewhat exceptional because soon after the liberation some 100.000 'collaborators' had been put into camps for investigation. Though some of the camps treated their inmates harshly (Belinfante 1978), the overall positive result was that in the Netherlands only some 100 executions took place due to 'retributive justice' outside the legal circuit. In e.g. France this number was much higher, about 10.000 (Belinfante 1978; Judt 2002, 2005).

In fact, the Resistance in the Netherlands showed a rather exemplary self-restraint. But then, there had been some clear agreements with the London cabinet about the purge of high police officers and judges, of the secretaries-general, and of leading industrialists (e.g. Sandberg 1950, Romijn 1989). These agreements made the leadership of the Resistance (united in the *Grote Advies Commissie der Illegaliteit* GAC) determined to restrain the blood-thirsty elements within the movement. Yet with the first anniversary of the liberation an embittered GAC had to remind the government of its complete negation of the agreements.
Due to the large number of camp inmates and the limited means at hand, prosecution progressed slowly. Then after the less serious cases had been dealt with and the prosecutors had collected the information they needed for the trial of the higher officials and industry bosses, most of their work was deliberately stopped by the government. Why?

The historian Tony Judt in his recent (2002) '*The past is another country: myth and memory on post-war Europe*' gave a succinct explanation of the growth of such puzzling post-war behavior of governments everywhere in Europe. An extended quotation (p.163/4):

> *'...the uncomfortably confusing recollection of things done by us to others during the war (i.e. under German auspices) got conveniently lost. It was in these circumstances that the 'Resistance' myth emerged. ... Thus to be innocent a nation had to have resisted, and to have done so in its overwhelming majority, a claim that was perforce made and pedagogically enforced all over Europe, from Italy to Poland, from the Netherlands to Romania.*
> *Where the historical record cried out against this distortion - ..., in the Netherlands where grossly exaggerated accounts of heroic farmers rescuing downed British airmen became part of the post-war national mythology – national attention was consciously diverted, from the very first post-war months, to examples and stories which were repeated and magnified ad nauseam, in novels, popular histories, radio, newspapers and cinema.*
> *It is understandable that former collaborators, or even those who simply sat out, should have been happy to see the wartime tale thus retold to their advantage. But why did the genuine resisters, who in most cases were also those on power in the*

immediate post-war years, agree to retouch the past thus? The answer is twofold. In the first case, it was necessary somehow to restore a minimal level of cohesion to civil society and to re-establish the authority and legitimacy of the state in countries where authority, trust, public decency and the very premises of civil behaviour had been torn down by totalitarian government and total war. Thus [European governments] all found it necessary to tell their citizens that their sufferings had been the work of the Germans and their handful of traitorous collaborators, that they had suffered and struggled heroically and that their present duty, the war now over and the guilty suitably punished, was to address themselves to post-war tasks, place their faith in constitutional regimes ***and put the war behind them****. Seeing little option but to concur, the domestic resistance movements abandoned their plans for radical domestic renewal and went along with the priority accorded to the search for stability even if (as in the Italian case) it entailed ... the continuity of the Fascist state apparatus into the post-war area'.*

Judt's explanation applies not only to the Netherlands, but to other European countries as well. War crimes trials notwithstanding (for which see Bloxham 2001), myth-assisted cover-ups are manifest everywhere in post-war Europe. In the East no less than in the West (on Poland see Polonsky (ed) 1990, Steinlauf 1997, on communist Eastern Europe in general Fox 2004, Shafir 2004).

Take **France** as an example.
There myth construction prevailed too, although war-time and post-war conditions there differed quite strongly from those in the Netherlands.

> Burrin 1995 gives an extended overview, while Gildea 1996 gives a shorter account encompassing e.g. De Gaulle's part in myth-making 'to build the nation', the collaborators' return to high positions, the part of the Resistance in an 'official' historiography that lasted into the 60s, and its crumbling in later decades. The troubled path of historiography and justice, as sketched by Gildea, is evidence enough that the effort to pass over unsettled war accounts was a dubious affair and did not settle anything.
> Just one example. The same Papon who as a secretary-general of the prefecture of Bordeaux had been responsible for the deportation of 1700 Jews, in 1961 as a prefect of police was responsible for the death of 200 Algerians demonstrating against the Algerian war...
> See Fishman et al. (eds) 2000 for recent accounts, e.g. Hoffmann 2000. For the technocratic link between war and post-war years see Azéma 2000 p.16.

Several aspects of this troubled path have recently been described.
But it was Robert Paxton himself already (who initiated the 'revolution' in French war historiography in the 60s and 70s) who pointed at a decisive aspect of the **continuity between war-time France and that of post-war years: that of the increase in power of political technocrats**.

Next consider the case of **Austria** (some relevant studies: Bergmann et al. (Hg.) 1995).
Austria regained its full independence by the 1955 State Treaty. At that occassion it (Judt 2002 p. 168)

'extracted from the Allies an agreement to relieve it of any responsibility for its years under Nazi rule, and thereby relieved its citizens in their turn of any last remaining need to remember those years or the enthusiasm with which <u>*all*</u> *sides ... had greeted the idea, if not the reality, of Anschluss*'.

The only result: a continuation of the denials and cover-ups that had become rampant already after the war. But now it became official policy…

> At the foundation meeting of the '*Österreichische Zeitgeschichts-historiographie*' in '60 the minister of education decided, in concord with the historians present, that a partial *'heilsames Vergessen'* was the way to go:*'Es wäre unverantwortlich, die unentschiedenen Schlachten der eigenen Jugend vor der heutigen Jugend nochmals auszutragen'* (cp. Botz 2006 S.1070).
> So, when later historians uncovered the unsavory past e.g. in connection with Bundeskanzler Waldheim, they were actually considered 'staatsfeindlich' by ÖVP-related officials and politicians (Botz 2006). So it is evident that this policy of 're-writing' not only fails to solve anything, but it also allows grave injustices to go unpunished ánd creates new instances of it. Simon Wiesenthal relates the example of Murer, 'butcher' of Vilna: the indictment by the prosecutor is perfectly clear, yet Murer escapes justice thanks to an Austrian jury united in the denials indicated. The father of a murdered son, who is present as a witness, has to experience this grave injustice (Wiesenthal 1980, ch.*'The Knife'*).

Myth-assisted cover-ups everywhere, there is no doubt about it. But then, we had experienced a totalitarian system of a perplexing kind, and it had gravely changed our personal as well as our public existence. Due to our refusal to take a critical look at our compromises (both during and after the war), any ongoing influences of this totalitarianism were accepted without reservations. More specifically, as the traces of the Nazi-system on the institutional level were not diagnosed, they could not be erased either.

All over Europe we were soon able to forget our own part in the system's horrors and were happy to attribute all of them to 'the Germans'. Then within a few years only the 'real Nazis' were held accountable. Even the industrialists, like those of IGFarben, co-responsible for much of the atrocities, saw prosecution against them shelved in '48 (Borkin 1978), the same year that the UN War Crimes Commission got disbanded (Judt 2005 p.54). Given the fact that

> *'researchers have not found a single [German] company that did not employ at least forced labourers'* (Kobrak & Schneider 2004)

and the fact that deadly labor conditions for the concentration camp inmates, used by big German firms, were the rule, such exemption from prosecution amounts to a refusal of post-war authorities everywhere to do justice to their own dead, at least posthumously.

It is hardly a miracle that protest in many countries was emphatic and loud. In spite of that, the official accounts of the war turned to a 'mythology of resistance' and any further investigation into our own collaboration was discouraged or suppressed.
In countries like France, the UK and the Netherlands myth making was the stronger because very soon also their colonial past was 'in need of' public forgetfulness. With post-war Dutch military cruelties in Indonesia only indicted by a small minority (e.g. Buskes and Verkuyl), this public failure only intensified the desire for 'myths' of the Dutch. As recently as 2002 Raben concluded:

> '*Das holländische Bewusstsein schliesst ein mythisches Narrativ von gemeinsamem Leiden und gemeinsamem Widerstand während des Krieges sowie von einer kollektiven Anstrengung für den Wiederaufbau nach dem Krieg ein. Die Geschichte der Überlebenden, egal ob Juden oder andere Zurückkehrende aus den nationalsozialistischen Konzentrationslagern oder Opfer der verschiedenen Kriege in Südostasien, wurden nicht gehört*' (Raben 2002 S.94)

Only in Germany the memory of the war atrocities has been a core part of its public consciousness since the 60s. Of all other countries, I am only aware of the Slovac parliament and government, one year after the 1989 revolution (Fox 2004 p.429) *'issuing a proclamation condemning the deportation of Slovak Jews as a crime committed by the state, and asking for forgiveness from surviving Jewish citizens'.*

In other words, for half a century there were no countries – except for Germany – really willing to critically investigate their own part in the atrocities of World War II. Hardly surprising that research into e.g. the prolonged effects of the war and occupation on law & institutions was not considered.

10.5. Bureaucracy and the 'possibility' of the Nazi regime

In a way research into the relationship between occupation and bureaucracy would have been straightforward. After all, Max Weber's analyses of bureaucracy were central to his opus and had become widely known before the war already (e.g. Weber 19..). More than that: Weber had thoroughly analysed also the threats of bureaucracy. I will use Mommsen's excellent summary (Mommsen 1989 Ch.7 p.114):

'*First, the more fully developed bureaucratic systems are, the more their operations become strictly impersonal: bureaucracy's specific nature emerges the more perfectly the more it is 'dehumanized', the more completely it succeeds in eliminating from official business love, hatred and, above all, purely personal, irrational and emotional elements which escape calculation.*

Second, all bureaucratic institutions tend increasingly to subject the personal conduct of all the individuals within their reach to formal-rational regulation of their own making, if only in the interests of a gradual perfection of their administrative performance.

Third, bureaucratization inevitably tends to create a new, priviliged 'class' of bureaucratic office-holders, which is separated from the mass of the population by upbringing, specialized education and training, as well as security of employment and guaranteed regular income. Djilas's 'new class' is indeed foreshadowed in Weber's ideal-typical analysis of modern bureaucracy. Worse, this 'class' is prepared to serve virtually any master, whatever his origins and whatever his objectives'

Bureaucracy is at the heart of 'the pure type of legal domination' that (l.c. p.115/6)

'*is based upon a legal system which operates according to formally enacted, codified laws and regulations which are purpose-oriented rather than based on moral norms of some kind. Its operations are therefore calculable and predictable throughout. It does not allow for any significant individual initiative, lest this upset the system even to the slightest degree. Ideally its legitimacy rests exclusively on the assumption that all its laws are legal to the extent that they have been enacted in a formally correct way; there are no substantive standards of justice or legitimacy*'.

It goes without saying that all this is perfectly applicable to bureaucracy under the Nazis. For now a quote Moore (1997 p.195) on the Dutch civil service:

'*Overall, it appears that all sections of the Dutch bureaucracy adopted an accommodating attitude to the Germans. This was not a result of massive changes in the personnel....Far more telling were the traditional attitudes of the Dutch civil service, who were wedded to the principles of administrative and public order above all considerations. These values transcended any assessment of whose interests a continued adherence to this philosophy served*'.

Moore then (p.195 f.) relates the story of the Dutch system of population registration that was perfected *after* the country had surrendered. Lenz, the head of the State Inspectorate concerned, '*was so successful that his new cards were deemed by the Germans as better and* *more secure than their own Kennkarte*'. That was very useful to them for their persecution of the Jews and in rounding up members of the resistance.

> '*If nothing else, here was a case where the traditional Dutch civil service ethos of obedience and order had shown itself capable of implication in the most heinous crimes when all moral and legal controls were removed*'.

That is true as far as it goes. For the question remains, hów these 'controls' were so completely removed. And more than that, why not any of these controls had been 'internalized' by Lenz and his fellow bureaucrats to guide their actions.

As to the latter question, we will repeatedly have occasion to consider it. As to the first question, Moore points to the fact that Lenz'

> '*aim and motivation was bureaucratic perfection, apparently without concern for the practical effects of his work.... However, Lenz cannot be seen merely as a cypher, happy to please by carrying out the orders of others. The arrival of the Germans gave him the chance to carry out his dream of complete population registration without being hampered by the restraints of democratic government*'.

And indeed it has definitely been established that this attitude was common among Dutch high officials:

> '*Even interventions that clearly could not reckon on a parliamentary majority before the onset of the hostilities, got pressed without mercy*' (Buyst & Lefebvre 2004 p.194)
> '*One clearly gathers that it was felt like a liberation [within the Departments] that parliamentary control had fallen off, so that now one could quickly and efficiently realize one's wishes, even...when befóre the occupation the same wishes had been rejected emphatically by the Houses of Representatives*' (Gerbenzon & Algra 1979 p.355).

The civil servants displayed a dangerous tendency here of considering themselves the 'neutral experts' better able than anybody else to make the 'perfect protocols'. Weber in passing had already indicated this connection between 'expert' and 'bureaucracy':

> '*...dass die Entwicklung zur rationalen 'Sachlichkeit', zum 'Berufs'- und 'Fachmenschentum' mit allen ihren weitverzweigten Wirkungen durch die Bürokratisierung aller Herrschaft sehr stark gefördert wird*' (Weber 19.. p.735)

In the Netherlands the incorporation of important engineering experts, and other experts connected with the Zuiderzee works, in government Departments, had in a specific way strengthened this connection. Also the great extension of the civil services, as connected with the regulations originating in the effort to combat the worst consequences of the Depression, initiated a strong increase of the number of experts within the government. The great increase in the involvement of government with agriculture was closely connected with those influxes of experts.

Lenz' contribution to 'bureaucratic perfection' was regarded as implying him in the most heinous crimes, but what about many other such contributions? The question is easily posed, but not that easily answered. Even in Germany the vast majority of such problems have not been researched (Stolleis 1984), let alone in countries in which the 'myths' were maintained till very recently. Clearly we'll have to do some original research, into the question of post-war continuation of war-time regulations first of all. We start with looking at the origins in Germany itself.

10.6. Power science

In a way for the Germans the war started already in 1933. Hitler's notorious concentration camps were then set up – and their 'effectiveness' soon became only too well-known. After Hitler's first broad acceptation by the Germans (both worker and capitalist) this enthusiasm soon had reason to subside. And it did, if we consider e.g. the near-complete lack of enthusiasm at the Führer's visit of Berlin on his 1936 birthday (20th of april). The incident infuriated Hitler (Kohlbrugge 2002 p.19).

This does not mean that the continuous experience of Nazi terror, esp. such to weak parties in society, did not have a big impact on the bystanders. Especially from the start of the war in '39 it is probably right to speak of widespread dehumanization as an existential phenomenon in those not actively fighting the Nazi evil. Remember that this evil by then had been institutionalized in all spheres of life.
Ruthless destruction is only too well-known from the 'great' conquerors of history. Also harsh oppression and terror are hardly new – just think of the 'great' dictators in the past and present. But it was Hitler's Reich that 'succeeded' in *institutionalizing* destruction and terror in a way that even to the Azteks, whose state system required regular sacrifices of humans, would have been quite incomprehensible. Now for the first time in history the state not only had been transformed into a ruthless war machine or a system of brute oppression, but into a monster of outright, demonic proportions, into

> *'that vast machine of administrative mass murder in whose service not only thousands of persons, nor even scores of thousands of selected murderers, but a whole people could be and was employed'* (Hannah Arendt – cp. Barnouw 1990 p.144; reviews of relevant literature: van Hattem 2003 and Gregor 2006).

Zygmunt Bauman's 1989 *'Modernity and the Holocaust'* (for a summary see Laban Hinton 2002) expounded what others had described already: that *technocracy* and *bureaucracy* were at the heart of the Nazi system. In some way these disciplines were essential to it. From his awareness of those historic 'qualities' of modern bureaucracy and technocracy Jacques Ellus for half a century kept on working at his critique of the 'technological system'. Hopper's summary of the subject:

> *'scientists and engineers carved out a uniquely important place for themselves in Nazi Germany by being the 'technocratic experts or assistants' who actively or passively made the implementation of some of the most irrational policies of the Third Reich not only possible but more efficient. Technocracy, in short, provided for rational implementation of the often irrational policies of the 'polycratic' cartel of overlapping, competing, and contradictory power blocs that constituted the Nazi government'* (Hopper's summary - Hopper 1996 p.176 - of Renneberg & Walker's argument in their 1994 '*Science, technology and National Socialism*' - Renneberg & Walker (eds) 1994, Introd.)

It is important to remember that the Nazis with their choice for 'technocracy' were constructing a truly modern research & extension structure – not the least for agriculture**.**
Such a 'modern' character of the Nazi system after the war got denied by suggesting a 'Sonderweg' for German pre-war modernization (see Hirschfeld 1997 and Steinmetz 1997 for discussions). But gradually this Sonderweg-thesis proved untenable - and we are obliged to consider the dark sides of 'modernization' too.
'Modern' evidently is hardly a neutral concept, and we better take a close look at the changes that are rooted in wartime. Note also that the connections of the German scientists, making

their career in the *modernized* academic & research structure, with the regime were often extremely close! An example: to be able to be the first in ‘obtaining’ the seed collections of e.g. Russian institutions an unknown number of them kept close relations with the SS in occupied territory. (Evidently Mengele was just an extreme example from within a framework of harsh science...)

> The active participation of those ‘experts’ had long been prepared, especially by their elaboration of **social darwinistic** theories in a lot of disciplines, both under the Kaiserreich (with its brute colonial policies and Great War) and under the Weimar republic. Cp. Dessauer 1943/46; Gasman 1971; Koch 1973; Weiss 1987; Quitzow 1988; Peukert 1993; Weissmann 1995, Evans 1997; van Galen Last 2000; Föllmer 2001; Madley 2005.

As is apparent, it was a specific brand of research that could grow. ‘Pure’ academic research, e.g. climate or oceanographic research, not connected with the war effort, came to a standstill in Nazi Germany (Fischer et al. 2000 S.519/20). Even the world-renown series *‘Das Tierreich’* was discontinued in 1941 (in a victorious Germany! l.c. S.476). As Raphaël (2001 p.23) rightly indicates

> *‘Die Diktatur setzte jedoch besondere Rahmenbedingungen und liess allein technokratisch-autoritäre Formen der Sozialexpertise zu’.*

As it was, the Nazi’s only were interested in science that was motivated by ‘Herrschen’. The members of the ‘Notgemeinschaft’ of top science, when linking up with Hitler’s totalitarian aims, certainly made a conscious choice for just this type of science. A conscious choice because the 20s had seen a thorough discussion of research aims & attitudes. Some specific examples will follow.

1925 saw the publication of Grünbaum’s *‘Herrschen und Lieben als Grundmotive der philosophischen Weltanschauung’*. Max Scheler - who wrote the preface for Grünbaum’s book – in his ‘*Die Wissensformen und die Gesellschaft*’ (1926) stressed the importance of ‘*Weltanschauungslehre*’ as a central academic subject, adding (S.520):

> *‘Dilthey, H.Gomperz, Max Weber, Ernst Troeltsch, Jaspers, Grünbaum, Radbruch haben je in ihrer Weise diese früher zu sehr in die blosse Geschichte aufgegangene Disziplin für die Gegenwart sozusagen neubegründet’.*

In a discussion with Grünbaum, Kohnstamm in ‘26 added that the researcher can be driven by e.g. ‘Lieben’ instead of ‘Herrschen’ and that this will affect both his methods and his observations. Next Helmut Groos 1927 - *‘Der Deutsche Idealismus und das Christentum. Versuch einer vergleichenden Phänomenologie’* - extended this discussion still further (publ. Reinhardt, München; Kohnstamm 1929 p.104 refers to this work).

In fact this discussion was part of a broad and international one on the character and limits of social science and science in general, in which eminent authors like Buber, Scheler, Berdjajew, Rosenstock-Huessy, and Mannheim were participating (e.g. Berdjajew 1925, Scheler 1926, Grünwald 1933). The participation of e.g. Plessner, Dessauer and Kohnstamm shows that this discussion was not limited to the social sciences (Plessner 1928, Dessauer & Meissinger 1931). Because Kohnstamm (1926/1929/1931) partook in the discussions at the highest level, quite a few people in the Netherlands with an academic education received pertinent information. That means that a shift to centralization and top-down policies especially in the Netherlands has to be interpreted not just as an unfortunate or badly informed choice, but as a conscious choice for ‘power science’.

The singular choice for 'power science' in 1933, by leading German scientists, had its methodological ramifications. The choice for 'Herrschen' was a choice for 'harsh science' as well: as to the methods of research, as to the ways it dealt with its subjects, and as to its general dealings with 'others'. It implied the emphatic denial of the need (a) for respectful dialogue and (b) for participation of people with practical experience. In other words, it denied the need for a science humble enough to recognize its essential incompleteness and limits...
There is no doubt that the researchers choosing the technocratic-authoritarian approach did so in an era in which alternatives were known. Yet they recommended their specific choice fervently at the start of the Nazi regime. It was their choice that got institutionalized – and we had better consider if their choice was also incorporated in Nazi agricultural policy, with its post-war ramifications.

> It should not be forgotten that '**harsh science**' was not typically German – e.g. Kühl 1994, Hansen & King 2001, Jeanmonod 2003. Refer to the approaches of the Mexican *Científicos*, and note that also the Brazilian population suffered from such approaches by its 'elite'. Note especially that social darwinism from the later 19th century on was *bon ton* among the *educated classes* in the US and in many European countries. The subject is well researched: Dessauer 1946, Conrad-Martius 1955, Gasman 1971, Koch 1973, Allen 1976, MacKenzie 1976, Kühl 1993, Evans 1997, van Galen Last 2000, Föllmer 2001, Sapp 2003, Kenny 2004. So if we take a closer look at the German scene, we should keep in mind that in e.g. England and esp. in the US and its satellites similar notions were popular indeed.

10.7. The scientific road to annihilation

In Germany social darwinism ruled the minds of the 'educated classes' (see esp. White 2002), with a grip from which in the 30s only a small minority was able to extricate itself - as a rule only *after* its consequences in the Nazi regime had become manifest. We see it firmly in place among e.g. industrialists, decades earlier already, on the occasion of the 1900 Krupp prize competition bearing the title

> '*What can we learn from the principles of the theory of descent with respect to the internal development and legislation of states?*'.

Reading some of the prize-winners – e.g. Ammon, Schallmayer – is a strong antidote against the 'neutrality of science' thesis (Weiss 1987). A quotation from O.Ammon's '*Die Gesellschaftsordnung und ihre natürlichen Grundlagen*' (Jena 1896, S.154/156):

> *'In seiner Gesamtwirkung ist der Krieg eine Wohltat für die Menschheit, da er das einzige Mittel bietet, um die Kräfte von Nation zu Nation zu messen und der tüchtichsten den Sieg zu verleihen. Der Krieg ist die höchste und majestätischte Form des Daseinskampfes und kann nicht entbehrt, daher auch nicht abgeschafft werden. ... Kurze Kriegen wirken entschieden als reinigende Gewitter auf die Bevölkerung, indem sie vorzugsweise die verweichlichsten und nicht mit genügend Lebenskraft ausgestatteten Individuen beseitigt, zugleich aber der Bevölkerung einen neuen frischen Antrieb gewähren, der sich in grösserer Gesundheit der Geborenen, in der Abartung der Erwachsenen und in einem bedeutenden Aufschwung des öffentlichen Geistes zu erkennen gibt. Durch den Krieg wird schliesslich wieder eine klare politische Lage hergestellt, welche den derzeitigen Machtsverhältnisse entspricht und folglich Aussicht auf Dauer gewährt'.*

Leading industrialists like Alexander Tille were agressive social darwinists, Tille even likening London's Eastend with its starving crowds to a *Nationalheilstatt* allowing 'natural' forces to do their work (Sweeney 1998 p.58).

In England many of the leading intellectuals - Thomas Huxley among them – sided up with Tille's social darwinism. They had a.o. William Booth and (later) G.K.Chesterton as their opponents - people able to rouse broad and active opposition to social-darwinistic policies (cp. Chesterton's '*struggle for life and survival of the nastiest*').

With their ample financial means Tille c.s. exerted great influence in, for example, the institutionalization of (part of) the social sciences in Germany (Sweeney 1998; an institutional-ization meant to sideline Brentano a.o., Grimmer-Solem 2003). But their influence on 'scientific' approaches in industry was hardly less. Their so-called scientific approach to labor in fact started with a total **denial** of the qualitative aspects of work and focused exclusively at machine-serving power and 'movement'. Their '*Arbeitswissenschaft*' was a Europe-wide phenomenon, with its relation with US-Taylorism only too evident; but note that both started from **complete oblivion as to the potential of human movement** (Pruijt 1996, Lewis 2001, Kanigel 2007). So much is clear from studies that díd start from experience of human move-ment in its own right (esp. the work of Laban c.s., cp. Laban & Lawrence 1947; on Laban see Hodgson & Preston-Dunlop 1990; for another rewarding approach see Buytendijk 1964).

Only by taking into account the social-darwinistic frame of mind of Germany's educated classes, their enthusiastic initiation of & participation in World War I becomes understandable. An enthusiasm that was decidedly not shared by e.g. most farmers, as Eugen Gürster told us (Gürster 1949 S.529):

> *'Nie werde ich ein kleines Erlebnis aus den Augusttagen des Jahres 1914 vergessen. Ich stand als ganz junger Mensch in der Feldherrnhalle in München und sah in das in wildem Enthusiasmus tobende Publikum des Mittagkonzerts hinunter; Angehörige der gebildeten Stände, vor allem Studenten, waren zahlreich vertreten. Eben wahren frisch eroberte Kanone von den ersten lothringischen Schlachten unter heftigem Jubel eingebracht worden. Nicht das sich ein Gefühl gemeinsamer Bedrohung, der Abwehrbereitschaft in allen Gesprächen hervortat war das Eigentümliche, Bestürzende, sondern der immer überall ausgesprochene Gedanke, dass man erst jetzt auf den wahren eigentlichen Sinn des Lebens gestossen und das alles vorher Gelebte zufällig, spannungslos und wertlos gewesen sei. Neben mir aber, über der Brandung auf der Odeonsplatz, standen drei Bauern; aus ihrem Munde sprach schon in diesen ersten Augusttagen die Stimme der Sorge um das Kommende, der Angst vor den unter dem Jubel von Dichtern und Denkern entfesselten Kräften der Ver-nichtung. Ich habe die von diesen Bauern ungelenkt vorgebrachten Worte tiefer Befürchtung lange mit mir herumgetragen...'.*

Indeed when we call World War I 'the chemists' war' we refer to the whole-hearted partic-ipation of industrialists, chemists and technicians in it, each playing his role, convinced as they were that they were serving the nation and, thanks to the 'survival of the fittest', human-kind. So by 1932 youths in schools had been taught a similar mind-set a long time. As F.Steinecke's '*Methodik des biologischen Unterrichts*' then stressed (see Quitzow 1988 S.38):

> *'Das Bewusstsein, das der Kampf ums Dasein nicht nur das Einzelindividuum, sondern auch die Völker betrifft, weckt im Schüler den Willen zur Behauptung seines Volkes im politischen, wirtschaftlichen und kulturellen Wettbewirb mit anderen Völker. Es macht den Schüler zugleich gefeit gegen schwächlichen Pazifismus und Völkerverbrüderung'.*

By then the 'educated classes' had introduced their race-hygienic calculus a long time already (Maasen & Weingart 2000 S.56):

'In the racehygienic discourse, coupled with Weismannism and shortly after the turn of the century with a crude Mendelism, the notion of the 'fit' and the 'unfit' in the Kampf ums Dasein was translated by 1913 into a normative economic calculation of the 'costs of the inferior to the state' (Kaup 1913) and by 1920 into the supposedly humane suggestion to 'destroy life unworth living' (Binding & Hoche 1920)'.

There is no doubt that, in this rise of the 'bionome Geschichtsbild' (Dessauer 1946), the denial of the human was there for everybody to see: *'einem Zweifeln, ja Verzweifeln am Humanen'*. Indeed, in 1933 the Nazis found a well-prepared German academic community for their purposes. The

Bekenntnis der Professoren an den deutschen Universitäten und Hochschulen zu Adolf Hitler und dem nationalsozialistischen Staat (Dresden 1933)

leaves no doubt about that. Still the very sizeable community of German refugee scholars (esp. in the US) after 1933 reminds us of the fact that this 'Bekenntnis' was a voluntary choice, not an involuntary one. The German academics of the 'Bekenntnis' had international contacts with e.g. English or Dutch scholars who emphatically rejected social-darwinism, so they were well aware of the resistance to their 'harsh science'. The resistance that the Leiden historian Huizinga put up, for example, became widely known (see Otterspeer 1984 on the incident-Von Leer in 1933).

Germany under the Weimar Republic not only had an academic community with strong social-darwinistic leanings, but this ideology had been increasingly institutionalized already. More often than not with the help of 'neutral' science and technology. Many decades of close connections between government, academics and engineers made such institutionalization even rather straight-forward. It was this fact especially that facilitated the Nazi grip on universities and research.

10.8. Nazi rule of agriculture

In the past Nazi influence on farmer and agriculture was often associated with ranting Blut-und-Boden rhetorics (like those of Darré c.s.). The re-discovery of '*Raumwirtschaft*' and its Nazi roots - the infamous '*Generalplan Ost*' was part-and-parcel of it - proved that the 'Nazi connections' were not that superficial: they got anchored in academics, bureaucracies & institutions that continued into post-war decades. So also as to any lasting Nazi influence on agriculture we better take a fresh look.

As to the close link between research and policy in Nazi Germany the *Notgemeinschaft der deutschen Wissenschaft* (the 1920 union of most universities, academies, technical universities, as well as of the Kaiser Wilhelm Gesellschaft) right at the start of the Hitler regime left no doubt. Consider the following recommendations of two of its influential members, Eugen Fischer and Erwin Bauer, at its 'Wissenschaftliche Kundgebung' in Königsberg in may 1933 (Mertens 2003).
Eugen Fischer, from the *'Kaiser Wilhelm Institut für Anthropologie, menschliche Erblehre und Eugenik'*, was proud to point to the large part that his institute played in the sterilization law that was soon to be published. And he concluded that, for the *'Erhaltung guter, gesunder deutscher Familien'*, the *'erblich Kranken und rassenmässig in unser Volk nicht Passenden müssen ausgemerzt werden'*.
Erwin Bauer, director of the *'Kaiser Wilhelm Institut für Züchtungforschung'*, spoke about *'Die volkswirtschaftliche Auswirkung der Pflanzenzüchtung'*. He pictured his centralized,

institutional breeding as empowering the Reich to strive for self-sufficiency in food & feed. Small wonder that a little later, in 1934, the Nazis forbade any use of (or trade in) all but the centrally bred and officially endorsed varieties...
Right from the start the Nazis had the full cooperation of the Kaiser Wilhelm Gesellschaft (now Max Planck Society), of the Deutsche Forschungs Gemeinschaft, and of the Reichs-kuratorium für Wirtschaftlichkeit (Schmidt 1999; Mertens 2003; Shearer 1997), to mention three of the most important institutes in the field of science and technology. So there is no doubt that the efforts of the leading experts of those years were co-determining Nazi institutionalization (Jahr 2005), including its centralized rule of agriculture.

The introduction of the *'landwirtschaftliche Erzeugungsschlacht'* in '34 (Petzina 1968 S.91f.) marked the end of the first phase of centralization. By then vertical integration of production, processing and distribution – pro product – had shifted all power to the Reich:

> *'Über die bis ins kleinste Dorf reichende Organisation des Reichsnährstandes liess sich die Erzeugung erfassen und von oben her die Verteilung bis zum letzten Verbraucher lenken'.* (Boelcke 1992 S.200)

December '33 had already seen the introduction of marketing boards for eggs, milk and butter, cutting through the farmers' networks. Now consumers paid more – and farmers received far less for their products (Noakes & Pridham (eds) 1984). A continuously increasing bureaucratic control, with always harsher penalties meeted out, soon had to guarantee the farmers' 'cooperation'.
The production plan was decided upon centrally and soon it was strictly imposed upon each locality. Importantly, self-processing and self-marketing, though common before the Nazi regime, was strictly forbidden. It all amounted to a violent disruption of rural and small-town socio-economics in e.g. Bavaria, where the rather independent position of the farmer's wife, which was largely based on her independent management of her resources (poultry & eggs among them), was systematically undermined (Osmond 2003 p.90).

Within a few years everything was strictly regulated, and the ensuing system from March '37 on (when the *'Verordung zur Sicherung der Landbewirtschaftung'* was issued, Petzina 1968 S.92) - prescribed not only rationing but also (Stephenson 1997 p.348)

> *'surveys of soil types and crops grown, with quotas detailing the delivery requirements of each commodity from each farm to the relevant state depot'.*

Research in which farmer and researcher cooperated locally was thus replaced by research and design in a distant centre, applying 'scientific' classifications that did not recognize the role of the farmer and that excluded social and ecological aspects.
Because more than a third of the farmers of working age got conscripted (Haushofer 1963 S.268 f.), very often the farmer's wife or the aged at the farm were 'sentenced' to hard labour that now amounted to *the carrying-out of orders*, which dented the morale of the peasants. Subsequently, a dense network of controllers was instituted to guarantee obedience to the regulations.

The decisive role of technocratic bureaucracies is the more clear if we consider that the system the Nazis imposed was one of *modern agricultural research & extension*: protocols were devised in central institutions and then passed down to the local farmer who was obliged to use them. Agricultural research and extension in the service of control from the centre (Stephenson 1997 p.349):

> *'The overriding preoccupation was with the maintenance and promotion of control by the ... National Socialist leadership, but the effect was modernizing, with greater*

> *concern about urban opinion, greater state control, and the relegation of agriculture in practice to the status of handmaid to the industrial, modern state'.*

Together with centrally released varieties, the use of mineral fertilizer was precribed (Streb & Pyta 2005). A modern picture indeed, effective only, because a strong bureaucracy secured the imposition of central directives.

But of course, with the conscription of the male peasants, the young, the elderly and especially the women had to do the heavy men's work, and also make far longer days than before (Haushofer 1963 S.268 f.; Kershaw 2002 pp. 55f., 282f.). There is an endless number of Nazi observations specifically pertaining to this point, e.g. the Reichs Führer of Schwaben in '38 already stressing that, as a result of the shortage of labour,

> '*farmers and their wives were becoming overwrought in a way he had never before witnessed*'.

If post war Germany experienced famine, one reason is this decade of slave-labor that exhausted the farm population, and a second one is the loss of most of their on-farm resources as a result of the 'modern approach'.

> The common reference to a 'lack of fertilizer' in late-war and post-war years as a prime cause of the problems overlooks the simple fact that the farmer could do with few mineral fertilizer, or none at all, befóre the Nazi 'modernization'. But with the own resources cut (their use mostly forbidden) traditional practices were out of reach. So, this 'lack of fertilizer' excuse should be interpreted as another **masculine distortion of history.**..

In this connection we need also to realize the harsh treatment of the small farmers (e.g. in Württemberg, Stephenson 1997), those on less fertile soils in the hilly or mountainous regions among them. The Nazis were on better-speaking terms with the big farmers of e.g. Prussia, for whom the Nazi measures were positive, at least for a while (e.g. Puhle 1975 S.98 f.), and especially with agri-business that received massive profits (Kershaw 2002 p.54, p.294).

Now what aspects of their complex of agricultural policies did the Nazis manage to transfer to occupied territory? As to France and the Netherlands: did not the strongly bureaucratic/ technocratic character of their central governments make them receptive in a special way to important aspects of the Nazi regulations?

10.9. War and the chances of technocracy

Officialdom in e.g. France and the Netherlands already had a strong technocratic slant before the war (for France see e.g. Ellul 1948 and Zeldin 1977 Ch.21). In the Netherlands, for example, the Zuiderzee works had induced a substantial increase in the number of government 'experts' overseeing the Wieringermeer reclamation & colonization. There were good reasons for the government overseeing those efforts – but it also led to a great extension of fake neutrality of experts within it. These experts elevated themselves to a level of governance 'above discussion', from where, so they flattered themselves, the country could objectively be guided for its good.

As it was, in countries like France and the Netherlands, the technocrats were more and more convinced that they, especially, were enlightened. Hardly a miracle that, when the war cut time-consuming traditions like parliamentary discussions, these experts felt free to organize things their way.

One of the major interventions of the Nazis was the shut-down of small, e.g. artisan-like, businesses. Bettelheim (1946 p.113/114) underlined that it was part of the general German war-economic approach. And indeed we can observe this phenomenon in the Netherlands, where the (government) interventions were likely to become permanent, due to a lack of post-war redress (De Jong 1999 p.279 f.; Klemann 2002 p.234, 240/241; Sluyterman 2005 p.119 f.; also Klemann 2006). Nearly 90% of enterprises in the leather & shoes and in the soap industry closed down and some 80% of enterprises closed down in paint industry (De Jong l.c.; Klemann 2002 Table 16.6). So, it is easy to see that the Nazi transformation of the local economy amounted to a rupture. The many enterprises closed during the winter of ’41-’42 *never reopened again* (Klemann l.c. p.240). A rupture that post-war reviews concealed because they skipped the figures indicating the fate of the small businesses in their statistics (as in CBS 1947).

In the Netherlands two related, bureaucratic decisions were taken, that prolonged the wartime ruptures, effectuated by the Nazi approach to the economy, into post-war years.
The *first* was the re-instatement of much of the Nazi-initiated Woltersom structuring of the Dutch economy, very soon after the end of the war, in spite of its blatant ‘Führer-structure’. This Woltersom structure, which had been the Nazi choice, was preferred to the structure advocated by the Dutch industrialists under the leadership of Fentener van Vlissingen. (For the war-time rejection of the Woltersom committee by prominent figures in Dutch society see Algra 1970 p.105/6; for the organization itself see Barnouw & Stellinga 1978).
The *second*, related point was the prolongation of the war-time *Rijksbureaus* bureaucracy, that had served the Nazi subjection of the national economy in order to extract resources for the war. The minister of economic affairs, Van den Brink, ordered the Rijksbureaus in ’48 not to give any more information – except for such information that had been signed by the persons investigated! - to the criminal investigation department charged with economic collaboration affairs (Meihuizen 2003 p.472).

When after the war many representatives in the Dutch House of Commons emphasized the need to promote small industry in rural communities (van Velthoven 1947 p.309, a.o.), they faced a minister who, with most of his colleagues and officials, quite likely interpreted the war concentration measures as progress. The arguments of these representatives were considered old-fashioned: as to scale-enlargement the Minister of Agriculture Mansholt interpreted war-time ‘modernization’ as progress, even if it formally stemmed from Nazi policies.
For sure, it is puzzling to see a strong figure like Sicco Mansholt accepting unquestioningly so many policy decisions from his ‘expert’ staff in the Department. But note that as to his background he felt one of them: the Mansholt’s were the foremost Dutch breeders of grains etc. Sicco fully endorsed the wartime regulations that prescribed the exclusive use of breeders’ varieties (from an official list) and that forbade the use of farmers’ varieties. Note also that Mansholt had a higher professional education, but not an academic education, so he knew enough to understand what his ‘experts’ were talking about, but too few to analyse & evaluate it himself. And then, just like the other main-liners of the labour party, Mansholt from the start was convinced of an industrial, large-scale approach to agriculture.

So what were the consequences for the small farmer?
A representative in the Dutch post-war parliament had documented the prolongation of such a Nazi directive, which had victimized a number of farmers near The Hague. In his interpellation of 13-2-47 van Velthoven (see van Velthoven 1947) questioned Mansholt about the closing down of smaller dairy farms in the region of The Hague in ’41. Small farms had been closed down in spite of the regional Dairy Society emphasizing that they often delivered

better quality e.g. milk and butter than the big farmers. Using the pretext of ‘standardization’ to maintain a semblance of law, the Director-General for Food Distribution Louwes (the best by then among the highest officials in The Hague) ordered the permanent closure of these small enterprises including the confiscation of their churns etc.

This measure was quite a typical part of Nazi agricultural policies in general. The already tight, controlling network was tightened once more in ’41. As Kershaw (2002 p.292) explains about Germany:

> *‘Peasant morale took a sharp turn for the worse in 1941 as a result of the new stringent attempts on the part of the Reich Food Estate to channel and control the few remaining loopholes in the marketing of farm produce, especially dairy products.. Only the threat of heavy sanctions against uncooperative peasants forestalled opposition... Dairy-farmers were particularly up in arms at the confiscation in spring 1941 of their butter churns and centrifugal machines, carried out in order to restrict production and sale of butter to recognized dairies and to stop peasants consuming their own products or supplying the black market’.*

It was this system of totalitarian agriculture that the Nazis endeavoured to introduce also in occupied territories. In countries where they found a cooperating central bureaucracy, as in the Netherlands, they often succeeded. Where they did not find such a bureaucracy, as in e.g. Belgium, farmer resistance had more of a chance. Belgium had an administration that was hardly at pains to adapt to German rigor, and anyway had a covert sympathy to farmers putting up resistance, like those in the Ardennes.

Quite in contrast with the Belgian attitude, the Dutch ‘regime’ of the secretaries-general was subduing any and all ‘resistance’. It even stimulated the introduction of ‘economic courts’ – after the German model - to punish any deviation from the rigid system. (Enq.Comm.1947, Conclusion; Gerbenzon & Algra 1979 p.358 f.; Hirschfeld 1988, and 1998 S.208)
It was absolutely out of the question that these policies were forced upon them. To mention another example: the Danish authorities knew how to protect their small enterprises (see e.g. Hansen 2002). This difference between Denmark and the Netherlands is in line with the more pronounced technocratic practices of Dutch officialdom in those years.

The Dutch civil servants evidently thought they were still ‘neutral’ in their dealings with the Germans, whereas in fact they had been collaborating with them for years. The lack of a post-war legal/administrative purge only strengthened this pretense of ‘neutrality’ within the administration. And so we see Mansholt refusing any & all reconsideration of the war-time closures (Mansholt 1947c p.319/320), suggesting they were urged by technical (quality control) motives only.
As to other kinds of agriculture-related closures (of some 200 cooperative diaries for example, de Jong 1976 p.143) we can only guess at the post-war continuity. But seeing the consistent centralization efforts of the post-war governments, also as to the continuation of local dairies war-time measures are likely to have been decisive.

Mansholt certainly felt not free to start a debate. After all, Louwes was the least suspect among the war-time high officials (cp. esp. van Kamp 2005). An investigation would quite probably have created a precedent causing an avalanche of similar ones. ‘Impracticable’, he probably decided, and in a way he was right.

10.10. ‘Raumwirtschaft’ and C.Staf

As to Mansholt’s own wartime conduct, we are sure that collaboration was no part of it.
As to his confident S.L.Louwes, things are less clear-cut, but Louwes evidently had no affinity with Nazi doctrine and not much with technocracy. It is important to realise, that after the war, Louwes was hardly involved in shaping national agricultural policy, because of his full-time involvement with FAO.
As a technocrat his counterpart was C.Staf, and it was he who after the war held a very strong, central position in the Department of Agriculture (see later). So much so, that much of the agricultural policy probably originated from him and not from Mansholt, an important reason to take a closer look at his actions in wartime and in post-war years.

As to Louwes, it is true at least, that he tried to avoid Nazi control as much as possible in his Department of Food Provision. The reorganization of that department had already taken place at the end of the 30s, in connection with the extensive regulation of agriculture because of the depression, and because of pre-war preparations for the food situation in wartime. Especially his right hand, the jurist Dinie Hoetink, kept focussing on the ultimate goal of the self-regulation of agriculture, bottom-up in corporate fashion. She had no sympathy for any *Führer-Prinzip* at all. When she in ’44 was transported by the Nazis to a concentration camp, and died there in ’45, the most able and independent jurist of Louwe’s Department was lost to Dutch agriculture and society.
But apart from her partial objections, technocratic high officials found not much of a counter-balance during the war. Because an evaluation of war-time measures was held up semi-indefinitely after the war, war-time technocracy could shade imperceptibly into post-war technocracy. The technocrats’ stance of ‘neutrality’ during wartime had made them oblivious to consequences of the Nazi regime.

Central to the Nazi technocratic approach to agriculture was the German ‘Raumwirtschaft’. Because of its international links with environmental planning (esp. spatial planning) in a lot of countries, it was easily conceived as largely ‘neutral’. More especially, those officials outside Germany who had a strong technocratic slant, were inclined to see only ‘technology’ here and forget about its totalitarian nature.

It is important to keep in mind that as far as agriculture is concerned top-level experts were involved from the very start of the Nazi regime (Haushofer 1963; Puhle 1975; Streb & Pyta 2005; also Petzina 1968). Highly esteemed by the Nazis was the *Raumwirtschaft* related kind of agricultural science: it was the agricultural scientist Konrad Meyer who held a prominent place in the hierarchy and who with his staff designed *‘Generalplan Ost’* for Himmler’s *‘Reichskommissariat für die Festigung deutschen Volkstums’* (Gies 1979; Raphaël 2001; Jahr & Schaarschmidt (Hb) 2005; also Overy 2002). This specimen of ‘environmental planning’ was an essential ánd destructive part of Nazi agricultural policies.

Post-war denials notwithstanding, it certainly had its influence in countries like the Netherlands. There the Secretaries-General were only too willing to have C.Staf, then director of the Nederlandsche Heide-Maatschappij, head a ‘*Commission for the dispatch of agriculturists to Eastern Europe*’ (the Culano - de Jong 1975 p.449f., also Barnouw 2004). That this mission raised little enthusiasm, did not stem from their presentation, but was a consequence of a deep-seated distrust of the Nazis among the majority of the Dutch farmers. ‘Experts’ like Staf and the high officials involved were able to discern ‘objective possibilities’ where many a layman sensed big injustice.

Staf epitomises the war-time high official who is not himself a Nazi – or even sympathises with any of Nazism's racial doctrines – yet feels at home with its concepts and practices of centralization and scale-enlargement, and is apparently oblivious to the close connections of these practices with Nazi destructive policies. Staf was director of the *Nederlandsche Heidemaatschappij* from april '40; he became its director-president in september '41. In what is an episode in need of research, Heidemij's Commissioners and Board put their interests and rulings in the hands of Staf (see for this a.o. information van Maaswinkel 1948). In that way a take-over by the Dutch national-socialists was prevented, yet at the same time the *Führer structure* that the Nazis wanted to impose everywhere was implemented. Staf then personally decided what formerly would have been deliberated with the members of the Board. (The ususal procedure would have provided ample opportunity also for the common members of the Heidemij to have their say). Staf made decisions of great importance for both the future of the Heidemij and the Dutch rural regions.

Staf's cooperation with the Nazis was close (see example given). Though certain projects were being scrapped by the Germans, the Heidemaatschappij received big orders too, e.g. in connection with the transformation of grass in cropland and with the expansion of the potato culture (for both cp. CBS 1947). After the war that would be defended as part of the effort to feed the population - though in fact the crop yields from the former grass lands were on the whole disappointing - but it conformed to German policies anyway. How much the Nazis saw in Staf the strong man able to develop new land for crop growing is proved by their aim to put him in charge of all the new polders (cp. Van Dissel 1991 p.115 f.). By then Smeding, before the war already the 'enlightened despot' of the Wieringermeer, had been appointed bailiff of the Wieringermeer by the Nazis – once more a function that fitted into the Nazi *Führer* doctrine. Evidently Smeding and Staf arrived at a compromise, for in what is one more episode in need of research, they made a shuffle, Smeding was to become bailiff of the new Noordoostpolder, soon after the appointment of Staf to a.o. '*Sonderbeauftragte für das Körperschaft Wieringermeer*' with complete *Führer* competences (completely out of step with Dutch constitutional law).

> What is not in doubt is that Staf's position, in which he combined the 'dicatorship' of the Heidemij with that of the Wieringermeer etc., next to become the strong man of the Department of Agriculture, was a direct result of his taking advantage of his positioning, as a technocrat, in the Nazi *Führer* structure. Note that also under Mansholt he remained in office as the 'President Directeur' of the Heidemij till 1-5-'47 (van Maaswinkel 1948 p.221). As the 'strong man' of the Department of Agriculture he carried the Führer structure over into 'peace' time.

Evidently, it was an ominous decision of post-war government not to subject the war-time Administration to close scrutiny, for now a technocracy empowered by Nazi appointments and Ordinances was allowed to implement its designs without effective Parliamentary control and without any right of appeal for the (small) farmer.

It is well to remember that Nazis in their own country cooperated closely with large-scale agriculture, while the medium or small farmer just got a raw deal (Puhle 1975 S.98). Part of their emphasis was on 'rationalization' including land consolidation projects, and so it is practically unthinkable that the Re-allotment Decree of July '41, changing the Re-allotment Law of 1938, was not, at least partly, their doing. In that connection, cooperation with this large-scale, agriculture-related, organisation in the Netherlands, the Heidemij, squared quite nicely with the Nazi agricultural policies. Because its rural planning easily fitted into the Nazi overall *Raumwirtschaft*, Staf could promote the extension of Heidemij's R & D in these fields, and after the war, from his central position in the Department of Agriculture, he could make it an integral part of the Heidemij (van Leusen p.130/131) *in a peculiar combination with* the Department and its 'Cultuur Technische Dienst'. If we try to follow Staf's steps in those years we come to the sequence:

1. First, there is the drive towards centralization of R & D aiming at large-scale, rural transformation, as acquired during Nazi occupation (centralized *Raumforschung* was held in high regard).
2. Next, its technocratic implementation in occupation law (the Land Consolidation Decree of '41 and its sequels), that is not hindered by parliamentary control.
3. Staf's appointment by the Nazis to '*Sonderbeauftragte*' is his final entrance in the highest echelons of the Department of Agriculture: in the first post-war cabinet we meet him as Director-General of Agriculture and the right hand of minister Mansholt (van Dis 1946 p.365)
4. This position he combines with that of chief of the Directory of Soil Use & Agricultural Recovery.

From this position Staf decided about the land consolidation of Walcheren, the start of the large-scale eradication of landscape and ecology in the Netherlands. Because it started with reclamation of the inundated island, Staf could and did afford to skip the regular procedures and decided for a radical re-structuring, as sealed in the Land Consolidation Law Walcheren of 1947. There were some in-depth discussions in the 'Snelcommissie Walcheren', and it obtained valuable information from biologists, yet, in spite of that, hedgerows, forest patches, and other ancient marks of the landscape were removed, straight canals and roads were constructed, and most small farms disappeared (van Zanden 1993). Only thanks to the action by Representatives in Parliament, at least part of the small farmers concerned were given a substitute farm in the new Noordoostpolder (see e.g. Ruijter 10-01-46).

By the continuous prolongation of the '41 Land Consolidation Decree, Staf and his officials carried the wartime regulations into post-war years. We meet this prolongation in the 'Law Occupation Measures IV' (Handelingen 1950-1951 II, Bijlagen, stuk **1954**), then next in its sequel the 'Law Occupation Measures IV 1953' (Handelingen 1952-1953 II, Bijlagen, stuk **2855**). By then Staf's policies had been fully institutionalized, and the Land Consolidation Law of 1954 was only their official confirmation. From then on the eradication of landscape and ecology in the Netherlands knew no bounds. First, because government kept the design of land consolidation in the own hand and decided about its implementation. Second, because it could 'lawfully' impose its designs against the opinion of the majority of the interested parties. Third, because from the start it separated its designs for agriculture from those for the landscape, it consistently worked to dismantle any and all real agro-ecology. (For a description envying (!) the powers of the Dutch government, see Ammer 1969).

After the war Staf was, moreover, Chief of the (post-war) Dienst Kleine Boerenbedrijven. As such he focussed on the 'rationalization' of small horticulture (Krajenbrink 2005 p.76). As to small farmers and their communities in general, Staf c.s. for some years pushed back the 'Boeren-werken', the small-scale, local works by farmers with their laboureres, upgrading e.g. drainage of their local fields, and extended the large-scale works of the Dienst Uitvoering Werken as administered by Heidemij and Grontmij. With unemployed rural laborers in the DUW, instead of employed at the Boerenwerken, public finances got relocated from rural communities and small farmers to the Heidemij and the Grontmij. (Soon these two could afford to buy the big machines with which they subsequently eradicated the Dutch landscape). But more important is that Staf c.s. could impose stringent regulations on all of the Land Consolidation Works, Boerenwerken included. Such regulations were all focussing at the 'rationalization' of farming, and their connection with the local ecology and community was zero.

> **Parliament disabled by technocracy**: as to the many questions and discussions in Parliament on those matters that yet never received a positive respons by Mansholt and Staf see a.o.: Mansholt Mem.v.Antw. 11-11-48 par.4; Handelingen II 1948/49, sessions 16-11 and 17-11-4; Rip 8-03-50 p.429; Mansholt 8-03-50 p.437; Hoogland 1953 p.3033/34.

Conclusion:
the transfer of the changes, made under the Nazi occupation, into peace time may very well not only have been substantial, but decisive even as to the course of post-war farming and agricultural policy. That could happen because technocracy disabled post-war parliamentary democracy. Technocracy's experts, who at best had pity on the small farmers, but knew nothing of their practices and resources, helped to build the bridge between occupation and post-war decades.

10.11. Technocracy and the Führer concept

Staf in person exemplifies that, for decisive years at a stretch, in the field of agriculture in the Netherlands the *Führer concept* was implemented and bottom-up information was frustrated (e.g. farmer- or citizen-participation in policy development). In the war years and the years after the war the Department and its institutes were extended and centralized, solidly top-down, with a greatly extended control apparatus to enforce the new rules. Quite likely we followed the German example in the Netherlands, because high officials were quite happy with what they considered to be an 'expert-centred' approach.

> **Parliament objecting to the use of Occupation Decrees**: after the war Representatives in the Houses of Parliament repeatedly signal that things are amiss. E.g. Verhey 1949 stresses, that the way the Central Milk Control Service and its finances are organized originate in war. Van den Heuvel 5-04-49 draws the attention to the sudden use of an occupation decree of '42/'43. In the Voorl.Verslag 1951 representatives object to the Minister wanting to use a occupation decree from 1943.

But consider the expert's viewpoint. As an essential part of Nazi policies, agricultural research of a centralized kind in often newly founded institutes experienced a real boom in Germany from '37 on. For those experts who were used to think of a 'neutral' S & T, it was easy to discard 'gross Nazism' as an aberration, while yet cooperating with the Germans in the 'technical' sphere. Remember that a well-informed person like Staf was aware of Germany's 'high regard' of institutional R & D:

> '*Die Jahre 1933-1945 waren in gewisser Hinsicht goldene Zeiten für staatlich finanzierte anwendungsorientierte Forschung, die sich in zahlreichen Neugründungen von Forschungsstellen niedergeschlagen hat*' (Raphael 2001 S.14).

As to agricultural research Heim (2005 S.12) informs us:

> '*Allein in den Jahren 1938 bis 1943 wurden fünf neue Agrarforschungsinstitute gegründet oder in die KWG übernommen*'.

Yet, how did Staf c.s. manage to come to terms with the *Ostforschung* as something ‘neutral’? After all, there could be no doubt about its importance for the exploitation of ‘*Grossraum Europa*’ (Heim 2005). Still Staf with his ‘Culano’ was willing to be part of it...

As to the ‘neutrality’ of R & D, Staf was well aware, that the universities and Hochschule had been obliged to extend e.g. their agricultural program with ‘weltanschaulich-politische Vorlesungen’. This extension had a price: *‘Als Ausgleich zum Ausbau des politischen Lehrangebots wurde das natürwissenschaftliche Studienangebot gestraft’* (Becker 2005 S.84). With Nazi policies in the Netherlands having similar aims as to e.g. Wageningen, what in the world was Staf thinking?

In Germany under the Nazis there was a shift in research from the universities to the central research institutes. Part of the finances required by these institutes was taken from the money assigned to the universities. In the same vein there was a growth of the (centrally directed) *Landwirtschaftliche Versuchsstationen* from ’33 till ’45 (id. S.86/87). But then, those same shifts we see during ánd after the war in the Netherlands. For though post-war years saw the effort by O.de Vries to return a measure of independence to agricultural research, by bringing it under the TNO umbrella (the Organisation for Applied Scientific Research), after his death in ’48 the Director General of Agriculture, Staf, brought all agricultural research under close supervision of the Department of Agriculture (Maat 2003 par.6) – that is, under his own supervision. Evidently the war-years had crippled his scientific conscience...
Though my analyses as to origins differ from that of Maat’s (2001, 2003), I agree with a number of his conclusions as to the dubious nature of post-war agricultural research (incl. breeding) in the Netherlands (quotations from Maat 2001 pp.214, 215, 216):

> ‘*Agricultural research was from the start an obvious candidate to be incorporated in the TNO organisation. However, by various moves, the ministerial Directorate of Agriculture managed to keep the research in its own hands*’.
> ‘*The intensity of the interaction between agricultural science and government favours organisational mutuality. Moreover, various examples displayed in the previous chapters make clear that the authority of science was not always based on superior and exclusive knowledge but needed the authority of the government in order to establish and maintain a central position in agricultural practice. Until present day the final responsibility is in the hands of the Ministry of Agriculture*’.
> ‘*A feature of Dutch agricultural science is that, seemingly, it feels exposed unless double-wrapped in a bureaucratic as well as scientific blanket of hierarchy. Perhaps herein lie some of its difficulties in coming to terms with the much broader range of issues and societal concerns that are today queuing up behind the word “agriculture”*’.

‘Bureaucratic science’ is bound to have decisively political roots, and that is what we are looking for here. Now note that the Nazis pronounced the verdict of ‘inefficiency’ with regard to the former university research. One of the main reasons for this verdict was, that the German professors had a substantial degree of autonomy and with that stood in the way of the drive towards centralization.
Apparently the post-war shift in the Netherlands had a similar background: professorial autonomy stood at cross-purposes with the aim of centralization. Indeed **in post-war years there were no financial problems for the new agricultural research institutes, but the Agricultural University itself had a hard time for years** (Van der Zanden, Handelingen II 17-11-48 p.312, and Mansholt Mem.v.Antw., Handelingen II, 1000, XI p.23).

As it was, during the war the supposedly 'neutral stance', demonstrated by many a high official, judge, big industrialist or expert, made them an easy prey for the Nazis, ultimately leading to their full cooperation.

10.12. The Führer concept embodied in breeders' law

What implementation of Nazi rules meant, will be illustrated with the

'*Decree of the Secretary-General of the Department of Agriculture and Fisheries for the Execution of the Breeders Resolution 1941 (By-law of the Council for the Breeders Law)'* (Nederlandsche Staatscourant of Wednesday June 24th, No.120, p.2-4, as signed by H.M.Hirschfeld).

This decree was in all its main points based on the Führer-principle:

- the President of the Council indicated, was also president of all its departments (Art.17 Pt.1), and
- all the members of the Council, as well as its officials, were sworn in by him (Art.11 Pt.1; Art.23 Pt.4)
- in each of the departments, in each separate case, the president (or the vice president if the president had decided so) indicated two other members (of the Council) to make the final decision (Art.19).

Note that according to Art.3 Pt.1 for each department of the Council the *Boerenleider* of the Nederlandschen Landstand (Dutch national-socialist equivalent of the Nazi *Bauern Führer*) nominated two members.

As to the role of the Secretary-General, this was also in accordance with the Führer-principle:

- the Secretary-General suspended or dismissed as he saw fit (Art.8-10).
- he appointed both the President of the Council and the Vice-Presidents of its Departments plus their members (Art.3 Pt.1; Art.17 Pt.2 & 3).

When after the war the 'Boerenleider' was removed from this Decree (suggesting serious purification of this part of occupation law) its rigid structure still remained, preventing any evaluation of the legal structure devised by the Nazis, of its complete negation of the farmer's historical role in breeding, and of its complete denial of resources vital for crop breeding.

Now the Kweekersbesluit (Breeders Decree) 1941 had limited the trade in, ánd the use of, crop varieties to those recognized by the Council and the State Committee for the Formation of the Rassenlijst (cp. Bieleman 2000 p.187/188). It prohibited the trade & use of e.g. farmer varieties. Only varieties delivered by the commercial and the public breeders, varieties conforming to the criteria of their 'trade', found a place in this Rassenlijst.

That List up till then had only been a list regulating the trade in commercial breeders varieties: it had not been meant to exclude either the use of other (kinds of) varieties or their propagation & selection. After all, farmers had always selected their own varieties. The number of public and of commercial breeders was small, with most of them active for some decades at most. Furthermore, those breeders were greatly dependend on the farmer varieties as their breeding resource.

If we ignore some extravagant pretentions of some breeders, it is safe to say that the limitations, dictated by the **1941 Kweekersbesluit**, had nothing to do with a real-life approach to breeding, or with respect for the historical rights ánd broad breeding qualities of the farmer. Quite to the contrary, it was **part-and-parcel of the Nazi top-down direction of agriculture**, consisting a.o. in a very restrictive Culture Plan for crops.

Already the Plan for 1941 prohibited the culture of spelt, that yet had always been a dear crop to small farmers on e.g. sandy soils. Being a small farmers' resource, it was hardly of any interest to breeders, who were sure it would not be profitable. And so spelt, *including its farmer-based improvement*, was largely unknown to the German experts and their Dutch colleagues. All they knew was that it was an 'unimproved' crop outside their 'expert circuit'. Sufficient reason to forbid its culture...

Enlightened officials expressed that it was '*harmful to their own interests*' that small farmers as a rule, and with most crops by far, still used their own farmers' varieties (Platenburg 1942 p.127). Their technocratic stance is evident from the following quotation:

> '*Extension of this [agricultural] advice also towards the unsupported small farmers would therefore be really much desirable, especially when obeying the advices would be made obligatory*. *For this is not a matter only of the interest of individual farmers, but also of the interest of food supply for all the nation*' (id. p.129)

> So we see that a high official (and a friendly one, at that) had changed over in a flash from guarding welfare and freedom of the small farmer, within the confines of the law and of the rules of parliamentary democracy, to 'enlightened' prescription of centrally devised measures 'for the good of the cause'.

Platenburg is aware that small farming is centred around the self-sufficiency of the household (l.c. p.99). Yet it does not lead our 'expert' to respect small farming in its own right: hardly any attention is paid to the small farmer's rural community, and as to the farmer's local resources, these are not even mentioned.
To the contrary, we learn that already in the late Depression years the small farmer received primarily fertilizer as support, and besides that seed etc. from the official breeders' circuit (l.c. p.61). There is no awareness, even, of a need to strengthen the position of the small farmer, by assisting him in building his local resources. If we consider what we know from other countries about e.g. the gifts of mineral nitrogen fertilizer in those years, the government's 'fertilizer support' is

(a) certain to have disturbed local biological nitrogen fixation, with
(b) hardly any positive results for the crop yield.

So much is apparent from the failure encountered with the introduction and extension of seed corn culture at the small farm. A failure that was mentioned, but never evaluated (l.c. p.67).
Platenburg is a rather kind-hearted official, as is evident from his efforts to promote honest tenancy conditions etc. (the main subject of his book). Such an awareness of social conditions, with only a very limited awareness of the embeddedness of the small farm in the rural community, and no notion at all of its ecological base and resources, is a characteristic of post-war policy (as promoted by Mansholt a.o.).

What we are faced with here is **technocracy, unchecked by either democratic laws or local self-determination, and part of a totalitarian system.** These officials were blind to what was, in plain fact, their punctual implementation of totalitarian rule. That blindness is hardly an excuse for the fact that **the Kweekersbesluit was a totalitarian decree, period.** As it was not cancelled after the war, this war never stopped for the small farmer. As a result, the Kweekersbesluit also held the citizen under (this specific part of) Nazi rule.

That the situation was and is foremost a bureaucratic one is admitted by Maat (2003 p.255):

> '*The Rassenlijst [the now obligatory varieties list] brought the leading role in plant breeding to the Wageningen Institute [for Plant Breeding] that she could not have attained from scientific research alone'*.

> *'The prominent position in the breeders' world...was based on a system of control that did not so much spring from scientific research as well as from administrative principles*'.

Significant as to its connection with the small farmer is, that this bureaucratic 'research' circuit at the start of the fifties threw all of the farmers' varieties of spelt into the dustbin (Zeven 1996). In doing so, it sealed nearly a decennium of (extra-parliamentary) prohibition of farmers' varieties...

The application of the Breeders Resolution and Decree was guarded closely, both during the war and after it. It received its legal stature especially with the *Decree Soil Production 1942 Cross Fertilization* (Nederlandsche Staatscourant, Monday August 24th 1942, No.163; signed again by Hirschfeld). That Decree was connected with other ones detailing the prescription of 'preferential crops' for each specific agricultural region recognized by law (as introduced at Nazi indication, again). It was specifically meant to prevent any cross-fertilization of crop varieties.
This Decree referred to the *Bodemproductiewet 1939* and the *Bodemproductiebesluit 1939*, yet there is nothing in these pre-war laws (meant for war emergencies) that justifies its content. The 'use' made of these laws is an expression of arbritrariness, nothing else. What we have here is just a convenient way of 'legalizing' a decree ordered by the Germans and a way of copying the laws they had imposed on their own farmers from 1934 on.

As indicated before, right at the start of the Nazi regime institutional breeders in Germany had offered their 'superior breeding power' to the Nazis. It was easy to convince the Nazis of the 'superior varieties (races)' they had in store and of the 'need' to keep their propagation 'pure'. All they had to do was playing down the great importance of Genome x Environment interactions for e.g. crop yields. A quote from Erich Thieme's 1936 article '*Die biologische Seite des zweiten Vierjahresplanes*', the part '*Erträgssteigerung mittels der Pflanzen- und Tierzüchtung*' (S.57 f.):

> *'Erbgut und Umwelt bestimmen ein Wesen, wobei dem Erbgut die bei weitem grössere Bedeutung zukommt. Pflanzen und Tiere mit minderwertigem Erbgut werden auch minderwertige Nachkommen haben.*
> *Im Saatgut schlummert gewissermassen die zukünftige Ernte. Daher kommt es beim Pflanzenanbau zunächst auf sauberes (gereinigtes) und erblich hochwertiges Saatgut an. ....*
> *Sehr wichtig ist die richtige Sortenwahl. Nur ausgewähltes Saatgut ertragreicher Sorten darf verwandt werden'.*

It was this collection of half-truths that got implemented first in German law under the Nazis, and subsequently in Dutch law under the Secretaries-General. Already in 1934 the Nazis issued their Ordinance on Seed Material, that put seed production and trade under strict state control. In its own words, the Ordinance was issued to '*protect German farmers …[from] … inferior, contaminated, hereditary-diseased seed materials*' (Pistorius & van Wijk 1999 p. 62, quoting from the Ordinance; cp. Flitner 1995). So **the Kweekersbesluit was simply a Nazi Ordinance adapted to the Dutch situation,** in 1941. And yet, after the war, by insisting that these were expert measures for the sake of agriculture and food provision, this war Decree was not cancelled. In the end most of it became part of permanent law.

The route that was followed to establish these laws had many twists and turns and was one of the most 'remarkable achievements' of post-war government bureaucracy. A quotation from minister of agriculture Mansholt in his Note to the House of 9-11-48 will suffice:

‘*Meanwhile the very advisable liquidation of occupation law is speeded up by means of new Orders in Council and Ministerial Decrees...*’ (Mansholt 1948c p.3)

No doubt Mansholt wished to speed up the revision of this body of occupation law that had been dragged on for years already. Yet the extra-parliamentary route, as constructed by the same experts who had implemented the Nazi rules, that he planned to reach that goal, indicates something of an artless attitude that is quite astonishing for a strong-willed politician.

Evidently Mansholt thought of this body of occupation law as consisting chiefly of expert regulations (as such the subject of revision by experts in his ministerial staff) that unfortunately had got tainted by Nazi input, but could nevertheless be salvaged. And of course, the role of the Bauernführer could be excised immediately, as well as the swearing-in of Council members by the President.

Still these glaring Nazi traits of the occupation law ought to have been a warning that a close re-evaluation was absolutely necessary, especially as to the ‘doctrine’ embodied in the laws. As a matter of fact, a thorough parliamentary revision was indicated. It is quite probable that this was the opinion of the extra-parliamentary Committee for Occupational Law at its start soon after the war. Yet this opinion would not prevail.

For Mansholt, the concept of ‘neutral expert knowledge’ evidently was at the heart of his notion of agricultural policy. This then he must have looked upon as knowledge of such ‘purity’, that in essence it could not be tainted by Nazi doctrine, and was not in need of any parliamentary discussion and evaluation either. It was a grave error indeed! Yet, at the same time it seemed so self-evident that it was not questioned by Mansholt, and only incidentally by the parliamentarians he was addressing in his Note. Indeed, after the war representatives in Parliament evidently did not feel at ease with occupation law. Yet, confronted time and again with the suggestion from the administration that much of it, if not most, was ‘expert matter’ of a neutral kind, we see that they were prevented from pursuing the matter.

> **Administration versus Parliamant**: these were strange years indeed, if we consider how nearly unanimous Parliamentary decisions could be held up by the Administration. The following example is an interesting one. Parliament had accepted Groen’s proposal to compensate (especially) small farmers for the fact that the fixed prices they received in ‘46/’47 were much too low. Though the official historiography of the age accepts that the vote got duly executed in the end, it is clear from the interpellations in Parliament that it was not. It would be very worthwhile to check if similar instances could be found in other departments too, in those years.

On the whole the ‘expert strategy’ prevailed and any free discussion was denied. More specifically no space was granted to

(1) extra-governmental academic/legal probing and discussion of war-time measures
(2) participation of farmers in the re-evaluation of occupation measures: they had not even a say in the question of re-establishment of the rights and practices that had been denied to them under Nazi-rule
(3) parliamentary investigation and evaluation.
(The ’48-‘53 Parliamantary Inquiry into wartime Dutch government in London fortunately unearthed some pregnant information on the wartime regime of the Secretaries-General too).

It is evident that Mansholt’s remark, implying that it was all a matter for government experts only, refers to a body of opinion that prevailed both in his own Department and in the Administration as a whole.

After the war ‘expert knowledge’, with its presumed neutrality, was central to the ill-considered ‘modernization’ of the Netherlands. As e.g. science historian Alberts pointed out, trust in the rational plans & calculations of the experts, relieved the parity principle politics that was an essential part of Dutch compartmentalised society (cp. quote in Authors Coll.2000, p.53/54). But then, being open and serious about one’s ‘identity’ and viewpoints was part of that society, while the pretence of neutrality was at the heart of its sequel, *expertocracy*.

It should be kept in mind that questioning the value of ‘neutrality’ is not the same as longing for the return of some pre-war compartmentalised society. After all, it was the socialist spokesman Kurt Schumacher who, after surviving ten years of Nazi concentration camps, stressed the need for such questioning! For isn’t the chief effect of this so-called ‘neutrality’ that, due to the exclusion of evaluation and discussion, it brings in, like a Trojan horse, the values and concepts that are anyhow contained in ‘expert knowledge’ - even then when these are of a totalitarian character?

10.13. Carry-over of war? Some objections

The common view is that the body of Depression law as to food and agriculture in the Netherlands was used to shape the extensive food distribution system during the war (with its foundation in the regulation of agricultural production). And there is strong evidence that part of this system was indeed shaped in accordance with Dutch constitutional law.

S.L.Louwes before the war received a central position in the complex government organisation for the regulation and support of agriculture that was shaped during the Depression (Krips-van der Laan 1985). In that position he showed that he did not oppose strong central-government regulation, in a situation of urgency like that of the Depression, but only on the condition that in due time some form of self-regulation of agriculture would be realized. With others he worked constructively on a concept of such common self-regulation, the first comprehensive proposals for the corporate organisation of agriculture.
At that time already he could count on the very capable jurist Dinie Hoetink as his right hand in all matters of law and regulation (van Kamp 2005). When Louwes was called to head the government organisation for food supply in wartime, in 1937, she was the main source of legislation needed for this task. Then, when the Netherlands were occupied and the Germans tried to push their own laws, she cleverly devised regulations that were still true to Dutch constitutional law. In the end the Germans got so fed up with her that they sent her to a concentration camp, where she died in ‘45.

But, of course, the Germans required a far wider framework of regulations than one just based on Louwes’ and Hoetink’s concepts of food supply and distribution. A few examples have already been given, and we saw that e.g. Hirschfeld and Stap were quite prepared to cooperate with the Germans also when this was in conflict with Dutch constitutional law.
The all-out effort of the Nazis to promote centralisation, and their drive to establish uniformity of the Dutch economy and social life, met with a lot of resistance on the part of the ‘little people’, but with remarkably little resistance from high officials like Staf and Hirschfeld. Even Louwes sometimes felt that he had to comply with the German demands, even where they were unconstitutional (like the closure of small dairy farms).

Occupation law in the field of food supply and agriculture evidently was a mix. Part of Louwes’ plan for the self-regulation of agriculture was, outside the Administration, worked out during the war (Geurts 2002), and led to the Foundation for Agriculture that became the consultation agency for the Minister of Agriculture. *But that was hardly part of occupation law, or part of post-war law building on it.* An important contributing factor for the establishment of the Foundation as a consulting agency was the fact that Mansholt saw its president, his nephew H.D.Louwes, brother to S.L.Louwes whom we met already, as the man who had accomplished the close cooperation of farmers and farm laborers. Louwes had accomplished this remarkable fact during war time, when consultations were held out of sight of the Administration, usually under the cloak of 'Church and Farmers' at its office in The Hague. As to this specimen of 'socialism', main-line socialist Mansholt sympathized with the Foundation.

> The war-time deliberations indicated, that were taking place outside the government circuit, were still part-and-parcel of Dutch compartmentalised society. Within its framework there was a quite general opposition to the pretence of neutrality, and a readiness to show one's 'colours' instead. Note that even discussions focussing on the doubtful aspects of compartmentalisation derived their convictions from this same framework. Authors like Rutgers, de Sopper or de Zwaan, who were explicit about their rejection of the status quo of Dutch society, did so in the 'principled language' of the time. Not out of convenience, but from conviction.
> In the same way after the war chief spokesmen of the 'Doorbraak' also operated within this framework, its 'principled language' being an eminent part of their discourse. So much so that, once the big majority was content with the expansion of the growth economy, their voices were silenced by the Labour Party top in the 60s. (Cp. e.g. Buskes 1971 p.64).

Yet, as to the bottom-up **corporate structure** that was its aim, it is evident that the government administration effected the delay of any serious implementation. A **partial implementation** only followed in 1953 (Landbouwschap), but **that had especially the regional aspects of the structure deleted, regional self-determination among them**. With the initiation of the *'Socio-Economic Council'*, also in 1953, basic concepts of a corporate, participatory, economic order got rejected implicitly (including concepts deriving from guild socialism). As a result **the corporate structure of the Landbouwschap became defunct** within a few years. It especially proved powerless to protect the small farmer.

It is important to realize that, especially as to the farmers' representation at government level in the Netherlands, descriptions more often than not suggest a continuity between the corporate ideals of the early years and the 'Green Frontier' power of later years. However, this 'Green Frontier' was eager to have an expert- and growth-centred agriculture have its way. Quite to the contrary, the original corporate ideas started from regional diversity and incorporated the small farmer.
As yet we have no reason to think that the often-suggested continuity of (this) farmers' representation in post-war government policies stands up to historic scrutiny. Remember that about 2/3 of the farmers were small farmers, and that government consistently denied their place in a 'modern society'. Clearly it is even cynical to suggest the representation of those farmers at policy level!
In contradistinction, the often-romanticized 'golden years of the Green Frontier' were part-and-parcel of the strictly uniform post-war agricultural policies, with no obvious relation to the typical Dutch diversity as known from pre-war years (and from the war time). These were 'golden years', after all, only for the ever-diminishing number of those farmers that applauded government policy because they profited from it. In plain fact most farmers by far did not applaud: they lost their farm.

This striking phenomenon was yet not perceived as such, because, **at government level and that of its policy-related institutes, ‘non-modern’ groups of farmers & farming systems simply did not exist, for decades. In the view of the government they had ceased to exist because they were not ‘modern’**. Some of them survived, but strictly on their own: officially they were not represented at all. Only recently have they become ‘visible’ again, because of publications of sociologists and independent scientists, who cooperated with them in their fight against government regulations jeopardising their existence. In regard to the *institutional* framework as a whole, because it had been adapted perfectly to the government’s ‘expert’ policies, there was no place for farmers deviating from the policy-acknowledged picture. Because of the extreme, one-sidedness of policy-directed research, all of those ‘deviating’ farmers got categorized as traditionalists, doomed to disappear.

The Netherlands, like other countries in Western Europe, had a relative freedom of speech. But as the example given of the ‘Doorbraak’ spokesmen indicates, that freedom was dominated to such an extent by the language of ‘progress’, that politicians thought it preferable to silence the voices of people who were not ready to accept this ‘progress’ at face value. **For decades ‘Modernity’ was the public religion in the Netherlands – as it was in the US**. After the war it took about a decade for **the battle between ‘modernity’ and ‘diversity’** to be decided. Significantly, this was a battle withín the bosom of the Dutch people, not just a battle forced on it by the government. One can in a way even say that ‘modernization’ was the Achilles heel of the pre-war Dutch society. Remember that the people representing its ‘pillars’ had succeeded, by then, in the emancipation of their own groups, legally and institutionally. Considering that this included even the founding of science departments in their own universities, this was quite an attainment. Yet it also meant that these ‘pillars’ always were receptive to ‘progress’: there was a distinctly ‘modern’ slant to the whole of this Dutch, compartmentalised system.

And so we see that the lure of e.g. American technology was strong even when people were decidedly critical of the American way of life. Yet as long as evaluation was still quite common, the balance would not easily tip to ‘modernity’, as is proved by the fact that the ‘consumers’ society’ was frowned upon. **Only when evaluation became the prerogative of some central experts (preferably government appointees) ‘modernity’ would be victorious over ‘diversity’. Not because of modernity’s merits, but simply because diversity depends for its representation in discourse on the active participation of the parties embodying it**. Such participation, where ‘experts’ play an important role, depends on the willingness and ability of those experts to stick to a facilitating role first of all. The expert must allow all these parties to speak up first, and then facilitate them in building their own practices. But that certainly was not typical of the post-war government expert system...

As it was, in the Netherlands (as elsewhere) the civil service had grown tremendously during Depression & War. This greatly enlarged administration had been very active indeed: its regulations had covered the country with paper. Now the post-war evaluation of that body of law proved to be a very slow process, and the prolongation of non-evaluated regulations a very common matter. In the end the removal of these regulations from the body of law failed miserably. In a way we ended up with a prolongation of the war situation in terms of the law, as closely connected with strong technocratic tendencies within central government.

10.14. Expropriating the weapons of the weak

Previously we looked at the example of the war-time closure of some small farms near the Hague – and Mansholt simply refusing a redress. Now such refusal was not unjust, in his own sight. For he was a ‘social technocrat’: someone with a social side ready to consider the individualized social needs of the small farmer, ánd in the same person a technocratic side unable to see any solutions for agriculture but ‘rationalization’ & scale enlargement. Prolonging small farming in his sight amounted to prolonging misery. Something in which he found many a big farmer at his side!

Mansholt showed his technocratic conviction e.g. when busy transfering the Dairy School from the little Frisian town of Bolsward to its capital Leeuwarden (Mansholt 1946 IIB p.9, IIc p.359, 365). He evidently could not envisage that something was wrong with this his measure for which he had reserved the money already in the Budget: in Leeuwarden all big factories and services were concentrated already! After much discussion in the House (e.g. Krol 1946, van der Zanden 1946) the representatives yet prevented the transfer by striating the post in the budget (Tilanus a.o. 1946).
Yet the problem was a general one. Representatives in the House time and again requested the foundation and extension of e.g. technical schools adapted to the rural community and especially supporting the foundation of small-scale industries there. Yet these people never received any concrete answers (e.g. Verhey 1948).

It was outside the High-Modernist frame of mind of Mansholt and his officials to consider the small farmer’s agriculture and community in its own right. Not even the question if maybe the havoc wrought by depression and war to the small farmer had disempowered him and ruined his access to his local resources was conceivable. All Mansholt c.s. ‘knew’ was that their farms & farming were ‘evidently’ not sufficiently ‘rational’ and ‘productive’. Without much hope – time and again it got expressed like that - Mansholt and his officials set to the task of ‘improving’ the sector. Even though recently, after half a century, war-time agricultural production received renewed attention (Klemann, Knibbe, Trienekes, a.o.), the ‘rediscovery’ of the small farmer did not yet take place.

To give an example: **the small farmer had always depended on local networks, to the extent that he could exist only within them**. So when the government’s crude measures in the Depression started disrupting these networks (instead of strengthening them), and especially when they became ‘illegal’ with the Nazi introduction of extreme control measures also in the Netherlands, the small farmer had no choice but trying to escape from the nets. Nevertheless, up till the present (e.g. Klemann 2000) this existential condition has not been used as an explanatory category. Yet, it is decisive as to interpretations of the ‘black market’, of the (in)justice of taxation policies, etc.

Wherever the Nazis found local Administration conforming to their orders - as the Dutch central Administration did already - they rigorously succeeded in cutting the farmers’ self-processing as well as their private distribution networks. The scattered news about the prosecution of non-cooperators indicates that soon harsh sentences got meeted out (e.g. Keesings Hist.Archief 1941 & 1942: 517, 4681; 551, 4956; 565, 5073; 576, 5159; 579, 5184; 584, 5224; 588, 5258; 598, 5334/35). As to the central Administration: even S.L.Louwes in a press conference 19-10-1943 rigorously attacked any self-distribution even to own neighbors or relatives (id 1943: 645, 5713/14). There is no indication that the many cases of harsh treatment of the small farmer ever were re-analysed considering the **illegality** of the approach of the

Nazis and the collaborating Administration – let alone that these injustices got redressed after the war.
Only where the local Administration knew how to take recourse to 'administrative retardation' and the like, at least part of the injustices aimed at the small farmer could be fended of (see Boot z.j. for examples of non-cooperative Administration). And indeed, wherever the small farmer had some administrative space left to breath, he very soon changed over from e.g. poultry (where he received no feeds anymore) to horticulture as a main source of income.

> See Klemann 2000 and CBS 1947 for some statistics, Boot z.j. p.93 for the problem, and van Dis 1946 p.354 for an express indication of the decisive importance of nót conforming to the myriad of government regulations during the war.

Yet time and again he got strung to new orders forbidding the escape routes, especially such orders that criminalized self-distribution, or self-processing. Or, as we saw already, orders that interdicted free use of e.g. crop varieties other than centrally issued ones.
In the Netherlands these and other war-time regulations not only were not evaluated after the war, but they even got prolonged for years, by that preventing both any real evaluation ánd a revival of local, small farmer-centred, processing & distribution. Early on such prolongation of war-time regulations was assailed in the House (Voorl.Verslag 1946 p.23), yet to no avail. In his answer Mansholt just refered to some measures that had been phased out, but he did not mention any of the decisively important measures that had been prolonged, let alone that he gave a reasoned defense of their prolongation (Mansholt 1946 IIb p.9). Before long this post-war prolongation imperceptibly crossed over into permanent regulations.

But at the heart of it, it seems that evaluation of war-time (and post-war) experiences did not accord with the post-war technocratic conviction. That the

> '*covert expropriation of the weapons of the weak*',

that always was part of centralizing efforts of the state, had been greatly intensified by the Nazi regime during war-time, could therefore remain non-admitted (or even not noticed). The same was true of the fact that it entailed

> '*a catastrophic loss of the situated wisdom in which real life flourishes*'

a loss that yet would become apparent only after

> '*the destruction of the local habitats, knowledges, and cultivation practices that used to dapple the landscape before the planners set to work*' (Caplan 2001).

As it was, the stronger the government administration's technocratic slant, the less it was either able or willing to evaluate 'modernization' policies - including those of the Nazis. Only more recently the doubts about the whole of High Modernism's state projects have increased and led to the questioning of their value for the ever-local real life of man and creation. Scott's 1997 '*Seeing like a state'* is a most eminent example of the ensuing analyses.

Yet, in-depth criticism was voiced from the first post-war years on. Jacques Ellul was its spokesman for more than half a century (Ellul 1948 f.). In his criticism the experience with France, where post-war technocracy had a main origin in the Vichy regime (Kuisel 1973), sounds through. Ellul's *desacralization* approach to technology is worth exemplifying:

> *'It seems to me that the only means to mastery over Technique is by way of "de-sacralization" and "de-ideologization". This means that all men must be shown that Technique is nothing more than a complex of material objects, procedures, and combinations, which have as their sole result a modicum of comfort, hygiene, and*

> *ease; and that it possesses nothing worthy of the trouble of devoting one's whole life to it, or of commanding an excessive respect, or of reposing in it one's success and honour, or of massacring one's fellow men.* ***Men must be convinced that technical progress is not humanity's supreme adventure, but a commonplace fabrication of certain objects which scarcely merit enthusiastic delirium even when they happen to be Sputniks****. As long as man worships Technique, there is as good as no chance at all that he will ever succeed in mastering it'* (Ellul 1963 p.26, emphasis J.V.).

Indeed, because certain accomplishments were broadly presented ánd conceived as definite progress – remember that e.g. sustainability was not yet an issue – the continuity between post war and occupation technical policies was considered a non-issue. It was consoling to think of experts, within the government bureaucracies during the occupation, to have stubbornly followed the path of progress in spite of Nazi pressures. It made the construction of the myth of a broad 'resistance' also much easier.

Yet it is only in neglecting some clear voices from the real-life Resistance that 'experts' could become part of the myth. I do think here of Kurt Schumacher, the German socialist who survived more than 10 years of the Dachau concentration camp. He rejected political a.o. 'neutrality' of government officials also because

(1) this made exactly those powers staff post-war government whose previous politics had brought the demonic war, while
(2) at the same time effectively 'neutralizing' those officials with more critical opinions (esp. by prohibiting them to be politically active).

Schumacher saw the results of this 'neutrality' before his eyes:

> *'In Politik, Wirtschaft und Verwaltung herrschen wieder die gleichen Kräfte, die uns zu den heutigen Zustände geführt haben'* (cp. Moraw 1973 S.77).

We should keep this decidedly political aspect – in the post-war context - of the 'neutrality of science/expertise' thesis in mind.

Altogether the 'myth' had the chance to rule the minds for decades. It is only now, when half a century after the war the archives have been opened, that some pressing questions have become 'legal' at least. But after the war it got soon agreed that to clear away the remnants of the war, all that was needed was to clear away the rubble. By building new factories, roads and houses we could enter the new era – unburdened. Soon even the few victims returning from the concentration camps were supposed to submit to the 'myth'...
The post-war installation of the 'myth' meant that henceforth as to the war only subjects internal to the war (e.g. concentration camps) would be researchable within the officially endorsed paradigm. As from one accord we decided that society as a whole had not been soiled by the occupation. That implied, of course, that anything and anybody taking part in reconstruction was deemed to build with solid materials. When these stemmed partly from the war, that was because they had been carefully prepared and kept pure for the purpose...

Importantly the 'myth' implied unconditional optimism in the building of a new society. Of necessity this unconditional optimism also shaped much of the post-war research agenda. A new world could be constructed 'from the rubble' - *vide* the front cover picture of the Dutch 1955 liberation memorial volume! – including the construction of a fully new agriculture not in need of any connections with e.g. farmers' practices of old.
The High Modernist spectacles anyway allowed only a few traces of former practices to penetrate to 'expert' and politician. As a result, it was hardly conceivable that there would be such thing as injustice done to the small farmer: all the government experts had done was building solid bridges to post-war society...

10.15. Post-war bureaucracy

At the start of this chapter we touched on Weber's demonstration that indeed bureaucracies are perfectly 'able' to strangle diversity. Now if we look at the post-war years, is anything like it discernable?

Post-war expert bureaucracies emphasized their own role in **rational decision making** as including (Allum 1995 p.387):

(1) listing of all alternative strategies
(2) determination of all those alternatives' consequences

Their pretensions that they were thus able to offer a **complete picture** – from behind their desks – to decision makers was important as to e.g. post-war institutionalization of planning. We should be aware of the fact that this was 'power science' even when it did not intend to be so (e.g. with a man like Tinbergen).

> It is no incident that some of the politicians who were most conversant with the limits of S&T were also the least impressed - e.g. prime minister Schermerhorn in the first post-war Dutch cabinet, see Griffiths 1991 p.129/130.

Broader still, the post World War II boom in research in Europe and elsewhere wás indeed aiming at 'Herrschen', not the least because it everywhere followed the pragmatic course that was propagated by the US. As to methods and institutions, it is quite well possible that technocratic-authoritarian science deriving from wartime received its continuation and extension in post war decades. (In this connection we looked already at the great changes wrought by wartime agricultural policies in the US).

As to institutions, in e.g. the Netherlands some five new agricultural research institutes had their start under Nazi occupation (Maat 2003 p.258; I do not count the 1940 Agricultural Economic Institute with these, because it had its roots in pre-war years already). This round of institutionalization reminds a bit to much of Meyer's *'Forschungsdienst'* to be just incidental. Remember it was in the Netherlands, just like in Germany, accompanied by all-out efforts at central regulation of agricultural production. Then in post-war years it was indeed this track of centralized, specialized research that was at the heart of the Mansholt's agricultural policies.

> We find the wartime origins/extension of centralized, bureaucracy-directed, research also in other branches of the economy. Cp. Collette 1990 for the origins of four Institutes of the building industry, and Faludi & van der Valk 1994 for decisive developments in public planning, e.g. '*its hierarchical nature [that] is at odds with local autonomy*' (p.75). There evidently is more than a remnant of the **Führerprinzip** here.

We saw already that there was an effort – by O.de Vries – to have institutional agricultural research (re)gain some freedom, but after his death in '48 the little he had accomplished in this respect was reverted again and all agricultural research brought under the Department of Agriculture (Maat 2001 Ch.4, Ch.8). From then on modernization as directed from the centre could progress unhampered: where research and policy were at one, policies could be of a remarkably prescriptive kind indeed. Within the rigid hierarchical bureaucracy of the Department of Agriculture the jurist Joustra for a long time controlled conformance to dictated policies (Bekke, de Vries & Neelen 1994 ch.3; van der Kroon 1994 p.177 f.).

This rigid bureaucracy guaranteed that any constructs decided at the top would be implemented - institutionalized, standardized, etc – also then when their relation with farm, farmer & farming was uncertain at best.

Such unreserved implementation of constructs was the other side of the near-complete disregard of history in research and policy. As to wartime history just one example: there was no investigation into the spectacular fall in rye yields later in the war (for which see CBS 1947). No effort was made to look at consequences of the extremely strict, central directives that, with full cooperation of (the experts in) the central Administration, were imposed in those years. Only recently (and in another connection) the precipitous fall in quantities of manure resulting from the early war-time fall in numbers of pigs and poultry, especially, was signalized in its relation to crop yields (Knibbe 1998 p.85). Untill then a presumed ‘lack of mineral fertilizer’ was made the culprit of disappointing yields in war-time – a safe indictment because it did not so much as suggest even the role of the expert or the impact of occupation measures...

The best one can say is that there was no effort to find the historic and local facts. Nor was there an effort to link up with local practices. Instead immediately after the war we see a feverish activity in which the Department of Agriculture tried to educate especially its field personnel in the expert knowledge its expert officials had in store (Landbouwcursus 1946/47). Evidently from the conviction that agriculture needed a full start under ‘expert guidance’. Looking at e.g. Dijkstra’s contribution to this Course on dairy farming we see no exploration of farmers’ dairing at all, just an exposition of ‘modern’ factory dairing (Dijkstra 1946/47). This though the pre-war government-induced exposition had emphasized the highly developed character of much of on-farm dairing, including its high level of hygiene (Dir.de l’Agric. 1937 p.175f.).
Related to that view of ‘central expertise’ there was, one surmises, the aim to have the centrally devised directions implemented by the farmers *via* the Department’s field personnel. That strict top-down model would soon give trouble with some of the ‘old guard’ of agricultural advisers, who were used to operate rather more independently.

As it was, a top-down concept of ‘expert’ government direction of agriculture became the core of post-war policy that yet never got purged from its war-time origins and meanings. Quite symptomatically the framework of the ‘*Bodemproductiewet 1939*’, wíth many of its war-time measures (considered ‘neutral’ but evidently never evaluated), for long years into peace time was used for directive purposes. In ’46 Mansholt, being questioned by the Standing Committee (Voorl. Verslag 1946 p.23), did defend this chiefly in the light of distribution needs (Mansholt 1946 IIc, p.363). Yet an express objection of Parliament was that the rules were a straightjacket that neither had been formed, nor was it applied, in any consultation at all with the farmers. Mansholt didn’t even touch on this aspect.

> As it was, some yéars later, in ’49, representative van den Heuvel still had to castigate the minister for his continuous use of such war-time ‘law’ (van den Heuvel 1949 p.1371) – that in this case even allowed him to abrogate something like absolute power (id. p.1370)!

With the waning of post-war shortages the ‘productivity gospel’ that was at the heart of the Marshall Plan and its Technical Assistance came in the centre. Now more than ever it was the large-scale, ‘rationalized’ (factory-like), enterprise that was the ultimate standard. Soon policies aiming at progress along this one dimension made other options disappear from sight. In fact there were several strands here mutually strengthening each other, until, from the beginning of the 50s on, the cord was thus strong that it could be used to pull the one dimensional policies.

One of these strands was considered in a previous paragraph already: that of re-allotment etc. The Standing Committee on Agriculture of the House (Tweede Kamer) in ’46 warned that more often than not it had only limited results, and inquired after the intent of scale-enlargement of re-allotment works that the minister had announced (Voorl.Verslag 1946 p.7). Then Mansholt in his Memorie van Antwoord limited himself to a short defence without entering into the question of scale-enlargement (Mansholt 1946 IIb p.31). Next representative van Dis (1946 p.354) called re-allotment ‘a disaster’ for many of the small farmers. Now Mansholt gave an extensive answer in which he did all his best to make the connection between re-allotment and the small farmer (Mansholt 1946 IIc p.355 f.). Yet also here he started from a ‘rationalization’ as devised in some centre: there was no effort at all to collect an input from the small farmer.

Indeed, as to the myriad of requests from the House to take note of the small farmer himself – and not just decide about him from a distance – these were all of no avail:

- the requests to have the Small Farmers Union partake in regular consultation fell on deaf ears
- the requests to give the pre-war Small Farmers Service a re-start *with an input from the farmers themselves* likewise were not granted.
- the wish to have a Chair, in the Agricultural University, for Small Farm & Farming was not granted – in complete contrast with the foundation and extension of a number of research institutes that nobody had asked for but that were deemed ‘necessary’ by the Department’s ‘experts’
- none of the requests for independent research into the field of Small Farmer, Farm & Farming were granted.

Reading those post-war discussions in the House, the fact seems evident that the eradication of the typical Dutch rural sócial landscape, with its rich variety of small-scale and local communities and human ecologies, was policy-related. As is the case with that parallel process, the eradication of the diversity of agro-ecologies.

10.16. Transformation - by elimination

An eradication of the agricultural landscape and diversity guided by policy would hardly have been possible some time before the war. It was after all only the Depression that had seen the first great extension of the government apparatus in relation to the economy at large, with the government bureaucracy as to farming and agriculture then experiencing a sudden growth. Much of this apparatus was aiming at control from the centre - part of it from necessity, another part socially and otherwise destructive. As to the latter, think of its factual interdiction of marriage for the unemployed (Wildt Meyboom & de Lamaar 1938 p.34, 47).
As to small enterprise, any relief was withheld for the time its owner needed to ‘consume’ his own house, even if there was nobody to buy it. Neither did his family derive any food from this decision, nor did the local economy attain to anything but dismantling from it (id. p.97 etc.). In that and in similar ways many a small farmer had to choose between outright hunger or being pushed into permanent dependence. It greatly embittered small farmers in e.g. Drenthe (Zegering Hadders 1947).

After some such rather haphazard efforts at ‘organization from the centre’ voices from the periphery to the centre made themselves heard. Among these, those of the small farmers, who formed the great majority of the Dutch farmers, yet were barely perceived by the Administration. This although many of them were in great distress and, worse still, rightly spotted disdain of their person and existence. But then e.g. reports from teachers, especially reports that had their origin in the diverse local & practical courses for farm women, started to inform the central government, while at the same time bringing some of its mistakes to its attention (van der Burg 1989, 2002).
And: to some in the ‘centre’ it was dawning that there were *agricultural* riches here, in these diverse agricultural regions, and with the big majority of small farmers. Not just liabilities. The farm-women’s experience with self sufficiency, with their own processing methods and their own local distribution network, and with e.g. vegetables and fruits in the wild, gained some recognition at least.

Indeed it is a fact that the government with its 1937 publication on Dutch agriculture (Dir.de l’Agric. 1937) proved to be ready to start from the real-life diversity of Dutch agriculture. As a first indication of doubts at the centralization indicated – as such comparable to the doubts of the Late New Deal in the US - the government now started considering in what ways local agricultural traditions and farming systems were an asset, instead of liabilities in need of ‘rationalization’ (but note that such ideas and practices still were voiced by then). Next the teachers and social workers experiences indicated, as well as the ongoing input from regional farmers’ organisations, indeed made for a gradual growth of the conviction that the regional and local levels had to be strengthened, not weakened.

Then war ruptured this process too. Rural social work was – just like other social and cultural activities - ‘transfered’ from its local base of (cooperation of) autonomous organisations towards a central department in The Hague. An ominous decision because, as the highly esteemed mss. Tjeenk Willink explained in february 1947 in the Eerste Kamer (Tjeenk Willink 1947), now the locally well adapted work had been strongly hampered. And with that the stream of information from the periphery to the centre had largely been cut. Both made the post-war redress urgent.
A redress that anyway had to come because the ‘centralization’ had been both against the wish of most farmers and rural communities, and against Dutch (constitutional) law. Farmers on the whole had rejected ‘unification’ after the Nazi model, with its certainty of a growing national-socialist influence in communities and in personal life (Geurts 2002 par.4.6-4.8; we touch here on a central aspect of the difference between Germany and the Netherlands).

The centralization indicated was widely perceived as embodying both a repugnant example of centralistic government activity ánd grave injustice (Boer 1949). Well-informed social case-work stressed the need to organise rural social work locally - in all respects. Yet the post-war redress of the injustice was not granted and the rural communities didn’t get this their re-start. Minister Mansholt, though admitting the war problems, once more defended his own officials (Mansholt 1947c p.320). In fact he previously had indicated his great contentment with his Department’s ‘social service’ (e.g. Mansholt 1946a p.9). And now he hardly was ready to give in: the chief of this ‘social service’, Platenburg, was one of his more able officials. So for reasons of ‘careful policy’ Mansholt refused to return to the pre-war structure: he was only willing to transfer the ‘service’ ‘in due time’ to the Department of Social Affairs - and evaded being any more specific. But then, that Department not being about farming at all, with this decision the central government's 'feeling' with the small farmer and rural communities would be cut in principle.

Nearly a year later the minister was questioned again about the matter, now in the Tweede Kamer (Voorl.Verslag 1947, p.5). The announced transfer to the Department of Social Affairs, as well as the complete lack of consultation about it, were emphatically questioned. Mansholt replied that only recently had the matter been mentioned to him in the organised consultation with the farmers (Foundation for Agriculture - Mansholt 1947 IIb p.8). This was not even a half-truth, and added to that also Mansholt's further response was in rather empty words. So he got once more questioned, very emphatically this time (Engelbertink 1947). It was pointed out that he put both the consultation with the organised farmers and that in the Central Coordinative Commission on Social Services off side. Also the integral socio-economic character of farming, that is always local and as such requires social work that interacts closely with the local community, was once more stressed by this Representative. In spite of this all, Mansholt once again answered in general terms and with 'good intentions' only, without any concrete engagements at all (Mansholt 1947 IIc p.903). Like in most of the examples given already, also here we meet a.o. the refusal to redress war injustice.

'*Admittedly, in history things go wrong time and again, and deeply so.*
Yet we have to live in the present: it simply is not possible to drag history along'.

From the recognition of e.g. the rights of aboriginals in Australia *including restitution* we know that fundamentally there is something greatly wrong with the attitude (towards history) indicated in this pronouncement. Yet we find it very difficult to think of any redress to e.g. the displaced small farmer. Was his displacement not largely a deplorable 'aspect of the times', with results that we cannot mend anymore, even while recognizing that things were wrong?
In what follows I take a further look at the historical background, for our impotence could very well stem from the lack of historical investigation that is such a distinguishing mark of High Modernity. Only with the past brought back, we can discern ways in which war injustices got actively prolonged into the present. Once we're that far, it will become apparent that we still áre responsible for this part of our history…

For somebody like Mansholt, not exactly a weak personality, this refusal means that he probably equated such 'redress' with 'regress'. But then, why did Mansholt experience the plurality of Dutch society – and especially that of its rural society - as an obstacle instead of an assett?

It is possible to think of an answer to this question: when considering the projects of the experts of his Department, he no doubt perceived they did not accord with an approach honouring local initiative and self-regulation. But then, these projects were of the kind that he saw upheld in those years also among US and FAO experts. His international connections made it easy for him to forego any further considerations.
Mansholt was a politician, but one who wanted his politics informed by presumably neutral experts. They would offer him the 'high position' from where to oversee the field of agriculture. Far above the small talk and strife of Dutch compartmentalized society, in his perception agricultural progress since the 19th century had been expert-led and only along that same road progress could continue. He was convinced that his own forebears had contributed to the erstwhile progress, and he wanted to follow their example, evidently oblivious to the fact that this was a partisan view indeed.

Some thorough re-evaluation of war-time occurrences could have cured him. But exactly that he did not allow. In this he was of one accord with the other members of cabinet, and with many of the renowned experts who were working together for the reconstruction and modernization of post-war the Netherlands.

10.17. Intermezzo: little people and their resistance

Most people in rural Netherlands – and not only there – lived before the war still lived in closely knitted local communities. Not that life was static: there was much initiative at the local level, both within the 'pillars' ánd - from the many people disquieted by the status quo - at their tangential planes. Though the miseries of the Depression greatly diminished people's material possibilities – all-out distress was great – discussions of a remarkably fundamental character increased, at all levels.
In a way such discussion was part and parcel of compartmentalized Dutch society. Remember that many a youth had his/her first training in 'speaking out' within the trusted confines of the own church or socialist youth group. Within the 'pillars' ánd at their tangential planes, at the local level first of all, people interacted. Here it was that most people had their emotional and psychic, if not religious, 'home'. In Blom's words (19 , p.316):

> *'In the 'pillars' many found their room, their certainties and therefore their feeling of self-respect; one did belong somewhere'.*

And he stresses that the activities the people were displaying were dependent on this specific way of being-at-home. When threatened by the miseries of the Depression, people sensed that their 'home' was in urgent need of maintenance by psychic investment. And indeed invested there was, in spite of the immense distress.
Public discussions like those between a pastor and a communist in the 30s – in the Amsterdam Concert Hall even – were drawing great numbers of attendants, plus all kind of commentary everywhere in the country. This was decidedly nó consumer society. To the contrary, in a rather existential way it often was '*I discuss so I am*', or at least *'I actively follow discussions so I am'*, and not our contemporary '*I shop so I am*'. In this remarkably diverse, small-scale society a 'Führer' was an unlikely figure.
It was here that the Nazis experienced small-type resistance of mostly little people. Van der Heijden (2001/03 p.277f.) relates of Hermanus Scheps, reformed ánd radical, writing pamphlets with a decided 'no' to any compromise with the Germans from the beginning of the occupation, his '*Political Catechism*' for the Dutch among them. When caught by the Germans they don't realize he is the author of such pamphlets, so much he is a queer 'tropical bird' to them (van der Heijden). An equally pious, radical and picturesque figure, Jan Dijkstra, we meet in the van Randwijk biography (Mulder & Koedijk 1988 p.146 f.).

The small-type resistance not only started early, but gradually learned how to make up for the lack of resistance at the top too (see e.g. Algra 1970 Ch.6). Not only on the subject of official and government collaboration, also on this small-type resistance and its growth most source materials still need exploration (as those in the Frisian *Ryksarkyf* – van Hennik 1996 p.504). Yet, as it was, the one neglect facilitated the other. Some of those picturesque people – e.g. Titus Brandsma who died in the concentration camp – were in a leading position before the war and therefore could become more widely known because of their plight in wartime. But most were little people known by their own community only, members of some local, small scale society of the type still so common in the Netherlands in (pre-)war times.

As it is, a portrayal doing justice to the picturesque and often non-pliable people who populated small-scale Dutch society in those years has still to be painted. Even a picture integrating equally motivated people of higher social standing – such widely diverging figures like Jan Buskes, Hebe Kohlbrugge, Jan Koopmans, Jan Eijkman, Hendrik Kraemer, Van Gelder, Miskotte – still stands out! Such painting asks of the historian to 're-live' the small-scale personal and social life of those people, including their participation in the local groups to which they belonged. Participation that had been under pressure in the Depression without bringing it to an end: it brought a weakening for some people in some places, but a strengthening for others in other places (cp. de Rooy 1981 p.32-35). Significantly, participation often became stronger during the war.

Only when such a total picture dawns on the historian (s)he can start forming a valid notion of 'Dutch resistance' in those fateful years. Still a common denominator of this small-type resistance becomes visible quite soon when we take a closer look. Because of its importance to our subject we will take the effort.

10.18. The dynamics of a compartmentalized society

As indicated, Dutch society up till the war was a mixed phenomenon indeed. In spite of its common use, this phenomenon is not well described with the concept of 'compartmentalization'. Quite likely, because the latter is a macro-concept that can do no justice to the strength people often derived from their small scale local communities. Moreover, it often is conceived of in a rigid sense, not allowing for a.o. the modest, but real mobility between social strata that was visible in the Netherlands.

> Just two examples: the Leiden professors Kraemer and Banning had both become academics though coming from extremely poor background.

As to the day-to-day level, not only there was within local groups and churches a mutual recognition of people from widely different social strata, but they often díd meet each other in their common endeavours (e.g. the church choir). That was one of the positive reasons why e.g. the christian trade unions, without denying any social problems at all, yet rejected the concept of class war.
It was exactly within the – mostly small-scale - corporate life of this 'compartmentalized' society, with its complex interactions, that people learned public responsibility (in schools, trade unions, churches, farmers' societies, etc). More often than not these people were from very common background: the well known fact that the compartmentalization of Dutch society was not so much petrifying it, as well as leading to the emancipation of many of its common members. And of course, this was decidedly not yet a consumer society: other aspects of life got accorded a big place.

So the Germans got confronted with a society in which independent thought was not confined to those people, like industrialists and professionals, who maybe thought it their prerogative. Exactly because of its typical small-scale character, a great number of common people had their own specific training in thinking and acting responsibly. Tradition and independence were here connected in a way that a theory construction that limits itself to an approach within the bounds of Modernity is not able to grasp. How this led to the 'little people resistance' quite typical of Dutch society, and what is the connection of this with e.g. the policies of the Secretaries-General, I shall now try to relate in an examplary way.

To begin with some solid fact: there were many sources of information giving the Secretaries-General, the High Court judges, and the high police officers the express injunction to end any pretended neutrality and refrain from collaboration - both in the legislative and in the executive realm. More than that, they also called upon them to protest clearly in specific cases, e.g. in connection with the treatment of the Jews. Two of the better-known examples are:

- Jo Eijkman's pamphlet of August 1940 *'Wij bouwen verder. Maar op welken grondslag?'* (van der Linde 2003 p.276 f.)
- Jan Koopmans' pamphlet, distributed October 1940, *'Bijna te laat'* (Mulder & Koedijk 1988 p.140 f.; see Kohlbrugge 2001 for first-hand information).

Of the latter pamphlet some 30.000 were printed and successfully distributed – most posted on the same day and hour - throughout the whole of the Netherlands. It was sent also to all high government officials, judges and police officials.

The first pamphlet in a way has been decisive in getting many of the Dutch alerted to the factual goals of national socialism. On sunday the 7th of July 1940 Eijkman preached a very sharp sermon in the big hall of the Amsterdam Society for Young Men. It was the first time that somebody like him took position in public and used such sharp words. Next sunday he preached the same sermon, now in the Great Church of Hilversum. Banker and friend Dudok de Wit was in the audience and emphasized he had to make a pamphlet out of the sermon and get it distributed throughout the Netherlands. When it got published (van der Linde 2003 p.276 f.), Koos Vorrink, who was a leading socialist and a friend of Eijkman, gave it as a present to a good number of friends.
The talks that Eijkman and Vorrink had, in those first weeks of july, dispelled the latter's last uncertainties about cooperation with the national socialists. This helped Vorrink prepare the meeting with Rost van Tonningen, who wanted to 'integrate' the array of socialist organisations within the national socialist unity organisations (Naarden 1989 p.134; van der Linde p.277). Vorrink c.s. not only refused any cooperation at all, but their action was also the signal for the self-liquidation of local sections of the SDAP and related organisations to prevent any compromise with Nazism (cp. Naarden 1989 p.115 f.).

Unfortunately quite some administrators of the (socialist) National Society of Trade Unions, for one reason or another, submitted to the national socialist demands, if only after some leading officials had been arrested. (The danger to be treated like the communists was of course big enough). These cases of 'induced collaboration' then made e.g. board members and administrators of the protestant and Roman Catholic trade unions think it over thoroughly. That helped them, when in late spring '41 it was their turn, to express themselves very clearly to the German Beauftragte, professing they would not cooperate even when risking imprisonment or death (van Dijk & Werkman z.j., Stokman 1946). Indeed, some of them were to loose their life. Notice that this common stance was unexpected for the Germans: in large measure it confused them in the execution of their plans.

> In a way they could have known (e.g. from the often quoted analysis of Dooyeweerd 1936). It was this stand-from-conviction that, at the end of the war, made people ponder the foundations and goals of their unions and other organisations, and made them stick to pre-war positions. The charge of 'conservatism', though often made about this post-war continuation of 'compartmentalisation', is wide off the mark.

In other cases taking a stance took more time, e.g. within some of the farmers' organisations, or within the universities (in Delft and Leiden the students needed less time than many a professor). Yet also the regular farmers' organisations were quite clear in their rejection of the Führer principle.
The Nazis from the start aimed at total control, and soon started with their policies of centralization. To discover to their amazement that all sorts of societies and foundations preferred self-liquidation, with informal continuation as far as that was possible, above 'equalization'. And so the extent, in those years, in which all sort of local organisations at once became 'church groups' or 'Bible groups' was astonishing indeed. At the national level, the ongoing consultation of farmers' & farm laborers' organisations was at home in the office of (the Dutch Reformed Church department) 'Kerk en Boeren' (Church and Farmers).
And so, if we consider the diversity of Dutch organised social and economic life, we see quite a common picture. When people were open about their 'colours' – either within the 'pillars' or at their edges or tangent planes – they could more easily identify the Nazi colours too. Conversely, the stronger they held to 'neutrality', the more difficult it was for them to discern the need to resist the Nazi offers and threats.

Out of the array of **literature** see: Stokman 1946, van Dijk & Werkman z.j., Naarden 1989, van der Linde 2003 H.28 – and as to farmers' organisations and the like e.g. van Kamp 2005 p.207f., Geurts 2002 Ch.4.4-4.6, 4.8. As indicated, the churches with their active and diverse lay participation were able to offer a refuge, something post-war generations were soon to forget. And more still, they were voicing express protest where high officials in the Administration (or high judges) complied with Nazi measures (Touw 1946, Stokman (ed) 1945). Because so much of it was forgotten after the war, and in the long run even was denied by social scientific publications within the dominant High-Modernist framework, Snoek (1990) composed his careful overview of the subject.

As to the 'little people' especially, they pretty soon learned about the examples given (as about others). This contributed to it that already by the end of '40 many people, in those small-scale groups and societies that so much typified Dutch society, had made up their mind as to the need to reject the Nazi proposals. Thereafter, as Algra has documented, they in many respects made up for the frequent lack of resistance of high officials, businessmen, and judges. Often it was more than a lack of resistance: outright collaboration.

10.19. The collaborating Administration

Within two month after the liberation of the Netherlands the critical periodical De Groene Amsterdammer published a cartoon picturing the purge of collaborators as a storm for the little people and as leaving the big shots in complete peace (as reproduced in Meihuizen 2003). In his balanced 1948 review of the first post war years, Banning – in those years an important voice in the Netherlands - essentially arrived at the same conclusion (Banning 1948 p.317f.).

By then leading figures from the Resistance had protested already the discharge of e.g. high police officers who had participated in the brutal persecution of the Jews. And no less a person than the queen Wilhelmina, in e.g. her very unusual letter of 17-1-'48 to the Cabinet, had admonished the Ministers to make the purge of (especially) collaborating high officials their serious concern (Fasseur 1995; Meihuizen 2003 p.291 f.). In the letter she refered a.o. to the unanimous opinion of the Resistance as to the urgency of the purge of police officers. Yet it was all of no avail. The failure of the purge of officials who in fact had been responsible for

the growth of (criminal) collaboration with the Nazis indeed became a public secret (Sandberg 1950).
Because these were years of poverty the minister of justice could stop the process of persecution simply by not financing the investigations anymore – as he did (cp. Meihuizen 2003). Worse still, he tried to muffle the voices of those who had been working at the purge efforts. With the means at his disposal he and his colleagues indeed seemed to succeed for a time. Both by keeping archives closed for historians – even to the well known Presser and De Jong – and by forbidding the writing of reports (even such of a confidential character).

> One month before the final surrender of the Germans also in the north of the Netherlands, the *Tijdschrift voor Overheidsadministratie (TvO) 1('45)No.5*, issued by then in the south of the country, included an official explanation of the *Decree Occupation Orders* (Staatsblad E 93). It expressly stated that this Decree encompassed also the Ordinances of the Secretaries General, as '*it was only the occupying force that empowered them [S-Gs] to issue public, binding regulations: they did not derive such an authority from Dutch law*'. **This exposition of the lack of authority of the S-Gs conformed to the one given by Colijn in July 1940, by Telder in 1941, by the London Cabinet in e.g. 1943, and by Donker in 1947**.
> Then, in Bogaerts' account (*TvO 2('46)No.87*) of the address of Minister of Justice Kolfschoten at the installation of the Governmental Committee Occupation Law (8-2-'46), there is a slight change in sound, yet he concludes still with '*All what was injected into our people in regard to legislation during occupation – this abortive product, supposititious to our constitutional state – to quote the Minister, can not disappear soon enough*'.
> And yet in *TvO 4('48)317-319* we read an account that is far from clear, yet is completely clear in one respect: it endorses the complete non-application of the Decree Occupation Orders to all regulations of lower government issued during occupation. As if it is completely self-evident, it quotes the proposal of the Government to add a second paragraph to article 11 of the Decree, though this addition in effect striates the chief goal of the Decree. The return to positions of power of collaborating members of the Administration is only too evident.

As a result for a time the 'official history' of the economic collaboration was that written by somebody voicing government opinion and himself part more of the collaborative than of the resistance group, Kortenhorst (in '*Onderdrukking en Verzet*' II, 221 f.). Time and again this author stressed the constructive intent and practice of the highest Dutch officials during the war, the Secretaries-General, and of leading businessmen, yet an account of the dark sides of their collaboration was not given.
Quite soon high officials of the (state) railroads NS who had been responsible for transport of Jews to the concentration camps had been exempted form prosecution (Romijn 1989 p.115). Sooner still the High Court judges whose rulings had been unconstitutional at decisive moments, and by that had allowed Nazi measures to become effective, got exempted (id. p.120). All such exemptions were squarely against the rulings of the Queen's London '44/'45 cabinet.
If we look more closely we see that especially the co-responsibility of the highest echelons in the Netherlands for the persecution of the Jews is missing completely in Kortenhorst's account (and in similar ones). And yet most, by far, of (a) judges of the High Court (b) Secretaries-General and (c) high police officers, except then for some weak protests, yielded to Nazi measures against their fellow citizens, the Dutch Jews. Though the protection of also those Dutch citizens, and of their rights, was their highest duty, they failed them completely. (See de Jong 1972, Hirschfeld 1988 Ch.4; as to the Courts esp. Michielsen 2004 Ch.4; cp. also Moore 1997 and Gerbenzon & Algra 1979).

As to the persecution of the Jews, the head of the Sipo and SD in Amsterdam W.Lages' post war testimony on the decisive role of the Dutch police has been corroborated completely (cp. Meershoek 1999):

> *'The main support of the German forces in the police sector and beyond was the Dutch police. Without it, not 10 percent of the German occupation tasks would have been fulfilled.... Also it would have been practically impossible to seize even 10 percent of Dutch Jewry without them!'* (cited by Hirschfeld 1988 p.173).

Note that, with prosecution outphased, for some years it was only the Parliamentary Enquiry (Parl.Enq.1947-55) that shed some light on the collaborating practices of high officials.

Decisive as to collaboration from within the Administration – with a similar collaboration from parts of the populace in its wake - was the proclamation of the Secretaries-General of October 28, 1941. By then they were fully conscious of all the advices against collaborating with the Nazis:

- from Eijkman's and Koopmans' pamphlets in the first months of occupation
- from the clear message of the Political Convent (see under) of november 1940, that in fact had been convened by Colijn (cp. Colijn 1940 p.53; his active role was reason for the Germans to transfer him to Germany, where he died)
- via the obvious message of the February strike (against cruelties to the Jews) and its aftermath,
- up till the refusals of collaboration from within Dutch trade unions a.o. organisations.

Still they issued the following proclamation, signed by three of them (cp.Hirschfeld 1988 p.149):

> *'The general well-being of the Dutch people demands that each person fulfils his duty in the place assigned to him. To this end, the laws and regulations applying in the occupied Dutch territories must be adhered to without reservation... However, there are still fellow country-men who have obviously not understood the gravity of the current situation. In their blindness they believe they can damage the occupying power by act of sabotage, although in reality they only damage* ***the interests of the Dutch people****. This cannot be permitted. It is in the interests of us all that an end is made to the activities of those elements who are undertaking such practices as soon as possible... Understand that the German authorities cannot tolerate incorrect conduct on the part of the Dutch population and realise above all that the life of many people is brought into great danger by the reckless behaviour of a few... Help to ensure that no harm is inflicted on our people by the actions of* ***reckless and criminal elements'.***

Noting the results of this proclamation, former Royal Commissioner for the province of Utrecht, Bosch Ridder van Rosenthal – a coordinator of the Resistance – at last wrote an extended commentary on the *Aanwijzingen* that for the Dutch Administration had been issued by the pre-war government for use in war time. It was brought to the attention of all high officials; the constitutional law professors Verzijl and Rutgers were among the people helping in drafting it. As to the treatment of the Jews it expressly said (under Pt.8):

> '*The proclamations in regard of the Jews fail to have any foundation at all in law. Cooperation is therefore forbidden in regard to the tracking down and imprisoning of Jews'*.

The government in exile then soon made it perfectly clear that it agreed completely with the document. More, with biting sarcasm it described the practices of the 'loyal' Administration. On Radio Oranje everybody could hear its verdict of these officials (2-10-43):

> *'They had spent their whole lives accustomed to obey, they were always- and rightly – so proud of the impeccable execution of their tasks and conscientious fulfilment of their duties, that they brought the same conscientiousness and the same fulfilment of duty to the scrupulous organisation of the plunder of our country, to the advantage of the enemy'*.

There could be no doubt with anybody as to the need for the purge of high administrators, judges and police officers. After all there was no doubt that they got noticed the limits of cooperation quite in time - also from the remaining spokesmen of 'compartmentalized' Dutch politics. These signals from politics were there because some leading politicians started to convene, to deliberate about the limits of cooperation, as well as about the prospects for post-war politics. It is to this part of history that we now turn.

10.20. The end of the law

It is essential to realize that occupation measures, even when formally prolonged after the war, could constitutionally hardly be called 'law'. Remember that parliamentary discussion and consent, as required according to Dutch constitutional law, had been forbidden by the occupier, and that e.g. also the constitutionally obligatory advise by the Privy Council had been skipped. Therefore the queen's cabinets during the war prepared (what finally became) the Besluit Bezettingsmaatregelen of 17-09-44 (Stbl. E 93) that indicated what was to be done with the three lists of measures presented (Van den Brandhof 1986 par.2.1).

The Committee appointed to screen occupation law for sure had not the aim to see long lists of occupation measures prolonged for years – as many of the measures in the field of food & agriculture would be – or even to get prolonged indefinitely - as some decisive measures in the field of agriculture were in the end. Measures that as such had been issued by the occupying power were - as the Besluit expressed clearly - no part of Dutch law. But because of the cooperation of the Secretaries-General with the Germans, was the Besluit applicable to most of the war time measures?
Donker (z.j. p.366 f.) clearly explains it is (cp. also Gerbenzon & Algra 1979). Fundamentally all of the war-time 'law-making' was 'outside the law': introduction of laws similar to these products could only occur in a fully constitutional post-war setting. That much is what follows also from some of the clear advices received by the Secretaries-General that we will now consider.

The first of these (Parl.Enq.1947, p.98-101) came from the so-called Political Convent in which the six biggest political parties convened (on which e.g. Drees 1959 p.39 f.). Dated 6-11-40 it was addressed to the Secretaries-General, and an example of juridical clarity. Its formulation left no doubt at all: any effort to introduce new laws/regulations, also those that seemed worthwhile in themselves, had to be rejected, the only possible exceptions being (1) military demands by the occupying power (2) those strictly needed for maintenance of public life, as far as existing pre-war law would allow.
Expressly it mentioned the chaos flowing from first introducing measures as legally valid that then after the war for constitutional reasons had to be excised again (but hów, after they had grown into the body of law already?). Furthermore it stressed that any such law/measure would put coercion on the lawful Queen's government and its constitutionally legislative organs after the end of the war. Both points became only too true...

Evidently the Germans got informed about the main author's identity. For while they put most politicians partaking in the Convent in special camps, prof. Telders was the only one who was sent to a real concentration camp. He died in Bergen-Belsen shortly before the end of the war.

The second document is the '*Commentary to the Instructions*' that in may '43 was brought to the attention to all higher officials, as well as being published widely in the illegal press and expressly accepted by the London government as her legal opinion. As indicated it was from the hand of Jonkheer mr. Bosch Ridder van Rosenthal, who (with others) composed them because it was only too evident that collaboration by officials had become rampant by then (Onderdrukking & Verzet I, p.385f.).
To give just one example: p.28 of those Instructions stated once more that any and all property belonging to foundations or societies active in education, welfare, public worship, etc, ought to be respected equally as to private property. This it did because by then officials had allowed a long time the abolition of most of those foundations and societies, including the efforts at 'concentrating' their property in a number of occupier-controlled foundations that embodied the Nazi 'Führer' principle.

For completeness:
there were other high-level warnings against collaboration issued to high officials, judges, etc. See for that of Dutch law professors Cleveringa and Rutgers, e.g. De Jong 1972. And for that of high level Reformed Church spokesmen like Eijkman, Koopmans, Miskotte and Gravemeyer refer esp. to Touw 1946, Snoek 1990, also van der Linden 2003. For warnings issued by RC bishops see Stokman (ed) 1945.

Conversely much of the resistance of the Dutch had become expressed in actively **rejecting these new foundations and their hated Führer-principle.** This resistance included all efforts to let the property disappear somewhere.

A well-known example of this kind of resistance pertained to the freedom of schools and education. Refer to the names of Overduin, who survived Dachau, and of Titus Brandsma, who died in the concentration camp.

I mention this hatred of the 'Führer' principle expressly, because it is exactly this principle that got embodied in some important laws/regulations, as introduced by the Secretaries-General, and then carried-over into post-war years.
That it stood foursquare to the principles guiding the more active parts of the populace – major segments of society, to wit protestants, Roman Catholics and socialists, were very much alike in that respect – the Nazis experienced to their own astonishment already in autumn '40. And yet this Führer principle got embodied in decisive parts of Dutch law, and not only as related to agriculture...

10.21. Failed excision

The post-war Committee that had to consider excision of war time regulations from the body of law was external to the government bureaucracy (see Donker z.j. p.273). It could hardly be else – but this external position soon made its elbow-room very limited indeed.
For from the start it not only was underfunded – giving it e.g. a very limited administrative size – but importantly is was confronted from the very start with a government bureaucracy insisting that all but the most glaringly Nazi measures were of a technical character needing

(only) expert information & re-wording. That is, from the very start the Committee found its competencies in important ways denied.

And so the 1948-1949 parliamentary Budget Committee writes:

> *'It is the opinion of many members [of the Committee] that the speed in which the clean-up of the German occupation-law is occurring with nearly all Departments leaves much to be desired. The State Committee Occupation-law seems to have to wage an ongoing fight against the laxity and sometimes even against the obstinacy of the Departments'* (Voorl.Verslag 1948 II p.3)

Minister Wijers of Justice in his answer spoke of

> *'the extent and complicatedness of occupation law, that on one side makes a speedy liquidation only the more desirable, on the other hand make it the more difficult to execute'* (Wijers 1949 p.16)

This was exactly what Telders had emphasized.
Wijers then gave an indication of the complexity of the matter, following not the least from the ongoing post-war applications and including many post-war transformations. The solution of the Committee on Occupation-Law to get rid of hidden parts of occupation law was to publish an exact enumeration of those measures still maintained, with the express indication that any other measures were no longer valid (l.c.).
Yet Wijers had to admit that **many measures in regard to economic life, e.g. those stemming from the Nazi-inspired Wolterstom organisation, had been kept outside the mandate of the State Committee**.

> **Corporate economy research**: It evidently is an urgent research subject to investigate to what extent the post-war administrative hold-up of such corporate laws – leading to their factual dismission in a later stage – flowed from the unconstitutional application of occupation measures. This is the more important because apparently there was, already before the war, a broad agreement abóut such corporate restructuring of the economy. **Remember that e.g. guild socialist concepts, as wel as Just Price concepts, were closely interwoven with this concept of corporate restructuring** (rejection of commodification of labor is at the heart of corporatist views, also with the more conservative ones)**.** Yet, its implementation instead of being accomplished was gradually phased out.
> **Woltersom organisation**: Krajenbrink (2005 Ch.1.15) presents the Wolstersom organisation as a system of '*horizontal corporations with as characteristic their rule by employers from the branch of industry in question*'. Casually he mentions that secretaries-general were in charge '*according to the German model of the Führerprinzip*'. Significantly, Krajenbrink gives not an analysis of the totalitarian character of the system, but merely mentions that '*the "Woltersom organisation" and the Food Supply Service, cleaned up from fascistic elements, would remain in existence up to 1955 in changed format*'. His artlesness is exemplary of nearly all those authors that have written about the subject, e.g. Groenendal 1982. Note that the subject is not even mentioned by most authors who studied the (post)war economy, from Thurlings & Lubbers 1948 to Klemann 2006.

Though outside the range of the present research, it is evident that the 'Führer principle' of about all of those measures did conflict with the desire for a corporate renewal of economic life & law.

As to the body of occupation law that was within its mandate, here the State Commission's Achilles heel was the 'expert character' that government officials reserved for much of this body of law. That he sensed something was wrong is evident from Wijers' politely phrased promise to the Budget Committee (l.c.):

> *'As to obstinacy of the Departments, this is out of the question; yet there exists a certain scruple, sometimes, to attempt change of a certain law for the incorporation of occupation law when other, more definitely material changes of that law are still under consideration at the Department. Yet also where such problems do exist, the undersigned will not allow any opportunity to slip by, to press with his colleagues for acceleration of the process'.*

Yet by then many a minister had been busy already preparing the **non-evaluated integration of occupation law in the post-war body of law by way of Orders in Council, Ministerial Decrees, a.o. non-parliamentary ways of law construction**. So much Mansholt himself proudly announced.

In fact, only some very big pieces of occupation law could not be thus integrated. From the State Committee's 'List Occupation Measures' we learn that there still were a big number of them in '49 and also still in '53. There is no doubt that the occupation was thus prolonged by the various Departments, for years at a stretch. Till in the end their 'new' laws became 'acceptable' because they were in conformance with 'established practice'...

Conclusion: do *not* go with the flow

At the outset we wondered if some piece of history, though an evident example of grave injustice, still can be mended after a long time. Important as to an answer is that we saw Donker's judgment of the **illegality of occupation law** corroborated. It is not only that brute force doesn't legalize anything, but the whole trail of occupation measures and its aftermath is contra to Dutch parliamentary and legal practice. After all appearance, all we have here is a kind of derailment of bureaucracy for which Weber warned us already. Its '*might is right*' will not ever furnish a basis for justice.

The (small) farmer was one of the prime victims of this derailment of bureaucracy, and **his rehabilitation is urgent**. His land, resources and economic networks have been taken away from him along profoundly illegal ways.

Still he was not the only victim. As indicated, the post-war prolongation of the Woltersom organisation and the *Rijksbureaus* will have prolonged Nazi deformations of the economy at large. Was small, artisanal, enterprise another victim? If so, is not our view of 'the economy' in need of a big change?

Be that as it is, as to breeding and the small farmer we up till now gathered so much information that we can draw some more specific conclusions already. With an effort to do just that we will bring this chapter to a close.

10.22. Extreme contraction of breeding

After the war an ongoing execution of the Nazi Breeders Law had been assured by prolonging e.g. the Decree Cross-fertilization '42 (still mentioned in the List of 1949), an occupation decree that had been issued formally under the umbrella of the *Bodemproduktiewet 1939* – **that yet never had been introduced for this kind of purpose**. Still to be able to maintain the total(itarian) application of the '41 Breeders Decree this, together with an array of Enforcement Decrees (origin 1942 and later in war), was prolonged near-indefinitely: we still

find it in the 1954 List! By then the array of farmer-centred alternatives – to the centralized breeding circuit - had been 'outlawed' for more than a decade. In other words, **the 'definite' Breeders Law was founded on more than a decade of unconstitutional prolongation of Nazi law, with implicit Führer principle and all.**
Except for the private initiative of some enthusiasts outside of the agricultural circuit, within about a decade even the traces of former farmers' varieties got erased by official policy, as administered with the help of close control and prosecution of 'transgressors'. Up till the war these varieties had been the own assetts especially of small farmers. Thanks to the intervention of the Nazi regime they lost it all.

This long track of prolongation of Nazi law was part & parcel of the remarkable growth of a centralized, non-participatory expertocracy, that was so completely different from (pre-)war premonitions of a corporate-participatory democracy. This adverse growth after the war was soon clear to critical minds everywhere in Europe (like Ellul 1948; on Kurt Schumacher's opinions see Marow 1973 S.69f.). Next the near-constant efforts from the side of e.g. administrative law jurists to make 'expert' processes within public administration **open, accountable and participatory** (e.g. Crince Le Roy 1971) up till the present had limited results only. So it is hard to escape the inference that post-war expertocracy derived more of its power from occupation law & its prolongation than in any way can be called 'legal'...

In regard to breeding, the *short term* results of the war-time policies were in accordance with the Nazi stupidity: beginning 50s the whole Dutch collection of spelt landraces was thrown into the dustbin (Zeven 1996). A sign not only of the complete denial of the value of farmers' resources, but also of a near complete lack of understanding of their own research field by the 'experts' cooperating with the Department of Agriculture. As to the *long term* consequences I just touch some aspects.
Centralization of breeding meant that of necessity attention went to the 'genes' and not to:
- the richess contained in the 'individuality of plants'
- the patchy, heterogeneous & hierarchical 'environment' as an asset, or to
- the farmer and her resources.

And so even the microbiological interactions, however fundamental for plant breeding, got out of view, with the 'environment' thus methodically played down and neglected.

Collapsed breeding research: with already the plant-mycorrhizae symbioses 'the great unknown' in this mainline, post-war breeding enterprise, tripartite associations, e.g. those of plants, mycorrhizae, and rhizobiae, were completely out of sight there, even though they are very common in nature (Allen 1992). Tripartite interactions at the microbial level could easily have been intimated from the pre-war work of Waksman and Winogradsky, but did not fit into the reductionist research strategies. An example described by Lynch & Harper 1985 is that of fungi on straw with their depolymerizing providing simple compounds to a nitrogen fixing bacterium, that experiences the low-oxygen environment that it needs thanks to the mucus deriving from a polysaccharide producing bacterium.

That is indeed a startling characteristic of most of post-war breeding. There is more than a remnant of wartime administration in this...
To nevertheless demonstrate the expert's 'power' over the environment – that in fact his methods made it impossible to him to use as a resource – he had to construct it himself. This he largely did by focussing on irrigation and high fertilizer gift: the mineral fertilizer solution as the uniform 'environment'. We already looked at this historical phenomenon when focussing on American agriculture.

The confluence of European and American breeding is directly connected with the war-related dominance that was accorded to centralized breeding on both sides of the ocean. The Dutch were instrumental in the implementation of centralized breeding all over Europe. For example, Dutch officials and experts were the chief speakers at the European Productivity Agency sponsored Workshop in 1954, '*High quality seed: its production and distribution*' (FATIS 1955; eight of the eleven speakers were Dutch). Note that the EPA was the European side of the Marshall Plan administration.

Because we lived in a collapsed world (our NPK-cage) we lost sight of
- farmers' varieties with their
- associations of plants and microbes that are dependent on 'organic' soil management, and
- the (small) farmers using those varieties and the 'organics' as a resource base.

Unable to see the real world from our cage, we also lost sight of the sheer necessity for agriculture of farmer-centred breeding. It is only of late that we learned from examples in the Third World that we were gravely wrong.

Because it is a subject in itself, I am not going to describe it here. Yet it is enlightening to find that the practice of maintenance & extension of the diversity of landraces among Third World farmers does not even conform to our notions of 'usefulness'. Pragmatism is a narrow Western (American) notion that, from the point of view of e.g. the Ethiopian farmer, is hardly meaningful (cp. Shigeta 1996 for an extensive example).

Still, during the past decades, so much material from Third World farmers has come to us that we have ample reason to doubt our 'superiority' in regard to breeding etc. (So much is clear from examples as varied as e.g. Sperling 1992, Voss 1992 and Richards 1996). Maintaining our 'superiority', by e.g. 'helping them' to use our centrally bred high-fertilizer hybrids, brings the real danger that we not only loose their landraces, but also the diversity-based ways of breeding they can teach us. Ways of breeding upon which we, in the end, do depend no less than they do...
But if this is so, how could we ever loose sight of it? What in the history of breeding in the twenty century made us prone to it?

Breeding as a farmers' art was ages old, but as a separate discipline it was young indeed. In fact, most farmers and public breeders on the eve of World War II were still investigating what were the (dis)advantages of the separation of the labours of farmer and breeder. But in a few countries – the Netherlands among them – most big farmers were quite convinced that the new breeding was for progress and acted upon that assumption.
Still even in the Netherlands e.g. new potato varieties had only recently been bred locally by one of the respected local agricultural teachers, in close cooperation with local farmers. This cooperative model was still known at the eve of war, also with some leading people in the Agricultural University. For decades there had been some kind of discord between those 'public' breeders who recognized cooperation (with the local farmer), and the big seedsmen who wanted to have it their own way.

Then some farmers' organisations thought théy could have it both ways. The top-cooperation 'Centraal Bureau' at the end of the twenties integrated a breeder in its organisation, who with his in-service department was increasingly offering their own varieties to the clientele. This was centralized breeding, so unwittingly cutting much of the cooperation and transferring breeding power to the central breeder (if only within the own organisation).

Still it didn’t all seem that problematic. For decades the Agricultural Experiment Stations were not just in contact with agro-industry or commercial breeding, but often at least as closely with regional farmers and their organisations. When during the Depression economizing led to centralization, part of this cooperation was cut, but the changes were not immediately visible.

When we enter the war, some big breeders for years already are quite convinced that the law needs changing for their convenience . Then in Wageningen a proponent of public breeding dies, while some others are sympathetic towards national-socialism. *Quite decisive*: with most of the farmers’ organisations refusing ‘equalization’, the real farmers’ representatives are out of the way and central government officials feel free to implement what they see fit. And of course: the changes in breeding law do accord to a small group of experts both extra revenues & near-absolute breeding power. It soon becomes evident that those experts are eager to ‘cash in’. Finally, during the war centralized agricultural R&D uncoupled from farmer participation is heavily institutionalized, making centralized breeding part of this ‘general’ system.

When after the war the farmers’ organisations can come in the open again, they

(a) are confronted with post-war prolongation of food distribution not only (with all that it entails as to crop delivery to the government network), but
(b) also with the prolongation of the breeding/seed control network from occupation, ‘for reasons of productivity’.

So in those post-war years
(1) the bigger farmers are confident that they yet can have it their way (e.g. via Centraal Bureau),
(2) the small farmers soon experience ever greater troubles and next are displaced (in a ‘socially responsible way’), and
(3) the only research that is allowed is directed from above, to the support of central and fertilizer-intensive breeding only.
As a result breeding in the Netherlands experiences an extreme contraction.

> After the war, as far as there are still any traces of farmer breeding, with its use of landraces etc, those descriptions are anecdotal, and largely unknown to the expert network.

10.23. A dead end - and the way out

At present, there is a near-complete exclusion of farmers from breeding. In the Netherlands, for example, only for potatoes, because of its specific genetics, a network of farmers remains actively involved. But that means, that there is also an extreme contraction in methods: only such methods are left that can largely do without the local farmer. For reasons related to the war, this double contraction of breeding - as to participants and as to methods - in post-war years occurs in *most* countries in the West (US, UK, Germany, …). Soon this double contraction – methods and personnel - is imposed on other countries too, by way of international agreements on breeders’ rights.

> In Western Germany the government, in 1953, after nearly all officials from the Nazi bureaucracy have been exempted from prosecution and again are in leading positions, adopts the Law on the Protection of Varieties and Seeds of Cultivated Plants, granting breeders the exclusive rights to variety production and marketing (Pistorius & van Wijk 1999 p.80).

Some breeders, for sure, then signalize that e.g. Genome x Environment interactions, though of prime importance, do not receive the attention that is needed. Yet confident that high fertilizer gifts plus irrigation – the main focus of the new breeding – allow selection of varieties that primarily respond to that artificial 'environment', and not to that of the soil in which they are sown/planted, public and private breeding stick to their singular focus. Before long, not only any feeling of those breeders with real-life soil fertility is lost, but their products become undependable too:

1. The new varieties, even those of the famous IRRI (Manilla), '*became sickly, lost uniformity, and generally "fell apart" in the third or fourth generation*' (rice farmers in the Philippines, quoted by Frossard 1998 p.118; note similar complaints were voiced in post-First World War Germany)
2. Most of the widely hailed victories of our Green Revolution breeding are dated (Smil 2000 p.29) and have not been maintained
3. Very soon after introduction of HYVs even maintenance of yields required great increases in fertilizer gifts (deteriorating efficiency of fertilizer use, cp. e.g. Byerlee & Siddiq 1994 Fig.4)
4. Disease and pest problems aggravated because of fertilizer use (e.g. aphids and root worms, Tiles & Oberdiek 1995 p.167).

When in the 1960s van der Planck noticed the losses of pest resistance that the system of centralized breeding has occasioned, there started some evaluation, at least, among public breeders. The FAO program on horizontal resistance breeding in the 70s and 80s is a belated part of it, but gets phased out when big economic actors consistently incorporate the seed companies and (Western) breeding largely comes in their few hands. **Those breeders are unable to exercise diversity-centred breeding because it can only be done the farmer- and ecology-centred way**.
For obvious reasons, those breeders focus at hybrids breeding, which then brings another round of contraction with it. Not only because the possibilities of open-pollination breeding anow re hardly explored, but also because of the consistent use of only one, or a few, Cytoplasmatic Male Sterility (CMS) factors in hybrid breeding. This means a precipitous loss of diversity, and combined with the inherent weakness of those CMS-varieties under stress, bodes ill. Note that mitochondrial disfunction under stress is part-and-parcel of the CMS of those varieties (e.g. Virmani 1994 p. 52/3, Park et al. 2002).

Hybrid breeding: breeding for disfunctionality.
CMS is, quite generally, connected with deformation of reproductive tissues, Kaul 1988 Ch.2. See e.g. Gorman & McCormick 1997, Smith et al. 2002, Fei & Sawhney 2001. For CMS and dysfunctional mitochondria – that cause a lack of resistance to pests - see Brennicke & Kück (eds) 1993, Vedel et al. 1999, Ducos et al. 2001.
Note that the dangers are well-known from hybrid maize itself (Snetselaar et al. 2001). The ongoing non-consideration of open-pollination breeding can hardly be called rational - unless it is considered a specimen of functional rationality.
Komolong et al. 2003, for example, report the disastrous losses, in nurseries and parent seed-production blocks, inflicted by the fungus Claviceps Africana on hybrid sorghum breeding. '*Only the unfertilized ovaries are infected, and there is a high correlation between nonpollinated spikelets and ergot infection. Consequently, male-sterile sorghum lines are highly susceptible…*'. Others met this same problem of fungal infection and formation of toxic alkaloids with pearl Millet (   ) and with wheat (   ). Quite likely, it is general with hybrid, fertilizer-responsive cereal.

It is pathetic to see Komolong c.s. search for some escape within this hybrid breeding, instead of facing its utter weaknesses and exploring the possibilities of open-pollination breeding. (The result of Claviceps infection, ergot alkaloid contamination of the grains, is known since centuries as a great danger to human nutrition).
Such non-consideration of dangers and alternatives is a characteristic of institutional breeding that is subject to government and/or industry bureaucracy. The many doubts and uncertainties in connection with hybrid rice and wheat breeding (Virmani & Edwards 1982) point to the fact that once again the Gordian knot has been cut with bureaucratic swords. Note that it is generally acknowledged that of the known CMS factors only a very few have been integrated in commercial/official varieties, aggravating the dangers of epidemic crop losses.
A return to substantial rationality in breeding is urgent.

These breeders are stuck with their efforts at power and/or profit maximization – as are those governments whose Breeder Laws allow them the factual monopoly of breeding. Genetic manipulation of crop varieties is still part of this same effort at power/profit maximization, and (quite literally) an enemy to farmer- and ecology-centred breeding. **Starting once more from the denial of local biological riches, and aggressively promoting a construct that embodies the ultimate loss of biodiversity, genetic manipulation brings us only further into the same dead end. We better start from where it went wrong, and put farmer & ecology once more at the centre.** After all, some decisive reasons for 'participative breeding' are known a long time (Rice et al. 1996 and refs), e.g. the urgency of in-situ maintenance of crop genetic resources (e.g. Vaughan & Chang 1992).

As a matter of fact, this move is increasingly being made, at any rate in those countries where the 'law' does not make it illegal. Famous became already the *Seed Keepers* in India (Shiva et al. 1995; Shiva 1999 p.66 f.), but at least as impressive is the *peasant science* of the MASIPAG farmers in the Philippines (that includes hybridizations by the farmers, Frossard 1998), or the *knowledge of the peasant women* that is at the core of the KENGO Indigenous Vegetable Project in Kenya (Tiles & Oberdiek 1995 p.166 f.).

On the other hand, in most of the 'rich' countries Seed Acts now prohibit farmers to engage in their own seed production (cp. the case of organic farmer Josef Albrecht in Germany, Shiva 1999 p.51). That is one of the reasons that, before long, 'poor' countries will be overtaking the 'rich': the 'rich' will have a hard time re-discovering the local riches and cultural capital that their bureacracies moved out of the way. In many cases a better diet will result from re-introducing this 'traditional' capital (nutritive value see Nellithanam et al. 1998, Tiles & Oberdiek 1995, etc).

Of decisive importance is that with the re-introduction organic practices, also effective symbioses are re-introduced. With the real-life potential of G x E interactions being explored for the first time since the war, and the strangling effects of fertilizer dependency undone, the farmer is free again to 'make local history' by applying his expertise and care to soil and plant. We are in for a rejuvenation of agricultural traditions everywhere, if only government and industry, with its experts, allow the farmer to take centre stage again.

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11.

Reappraisal,

with

theses

This final chapter will not provide a summary of earlier chapters, but will point to some chief aspects of post-war agricultural policy and research instead. They will demonstrate that there was not an ‘agricultural necessity’ leading to ‘industrial agriculture’, but that a series of politico-ideological choices and historical contingencies are core reasons for the ‘industrialization’ of agriculture.
That this ‘industrial agriculture’ is an aborted project is mainly due to the fact that it lacks substance (‘*Sachlichkeit’*), in the context of life-systems. As it was designed by some far-away centers, the rich array of possibilities for sustainable, natural resource-based agriculture was not utilized. When it nevertheless imposed its industrial constraints on agriculture, they were not adapted to soil- and plant-life, with terrible devastation as a result.
As ‘industrial agriculture’ does not take into account the local soil and the natural resources – in theory and in practice it is dominated by industrial inputs – it has great difficulty in understanding issues of sustainability. Non-industrial inputs are outside its frame of mind. Yet there is a wide array of local resources, and the sustainable production of food and renewable materials depends on their use. With careful *in-situ* labor, fed by local expertise, these resources can be exploited, and this is the key to the socio-economy of the future, that of necessity is based on renewables.
We need New Peasantries to turn the key.

11.1. Modernity imposed

During my teen-age years, I used to cycle from our home in Amsterdam to my grandparents who lived in Frisia. Whenever I left the historical landscape of the old polders and entered the Wieringermeer, the first of the Zuiderzee-polders, I had to muster up my courage. This was not because I was tired! Exhaustion would only manifest itself halfway the seemingly endless IJsselmeerdam (Afsluitdijk). For a boy of my age the monotony of the new polder landscape took the adventure away. Here modern utopia had been realized – but it was dull. It had no memories of a distant past, nor did it have the perspective of a hidden, but beckoning, future. What met the eye was the planned, mathematical orderliness that the designers and engineers were after.

They had constructed it, at great additional cost, by turning the variegated lake bottom into a uniform surface after it fell dry. The technocrats covered the re-habilitated surface with a grid of roads and canals, ‘*claire et distincte*’. Everything had a modernistic design and construction, especially the farms and the agricultural landscape. Next, under the watchful eye of the Wieringermeer Authority (Directie Wieringermeer) and its successors, any impending chaos would be removed and be replaced, time and again, by the modern “grid”.

In fact, the technocrats had spoiled the landscape-under-water when they dredged canals in the lake bottom and tipped the mud and sand on the parcels in between. The waves did not follow their instructions and did not equalize the dredged material: when the lake bottom fell dry, it proved to be a moonlike landscape (cp. photographs in Mesu 1954). This failure of technocracy summoned the use of still greater technical efforts, to obtain an inhabitable world. Once more the choice was made for technocracy, in spite of the fact that the results of some research projects would have allowed a far more ecological approach. (For accounts of those projects see e.g. Zuur 1936, Feekes 1936, Bakker 1954, Prummel 1954, van Schreven 1954). In the end it was not technocracy that solved the problems, but the initiative and effort of navvy and technician (Mesu 1954). Yet, technocrats made the designs…

Modernity was imposed from the drawing boards, and as long as it reigned, history was not allowed to play a part. Here and elsewhere Modernity was after:

> '*a world in which there is an unambiguous (algorithmic rather than merely heuristic) recipe for every situation and no situation without a recipe attached. But to create a world matching such demanding standards one needed first to clear the building site of the scattered sediments of past actions, which, as it happened, all stopped short of the ideal. Modernity was therefore the era of … perpetual dismantling and demolition; the 'absolute beginning' was another face of the instant obsolescence of all successive states, and thus never-ending attempts to get rid of yesterday's history*' (Bauman 2001 p.65).

There is a deeply tragic element in this proud example of Dutch Modernity. Its experts expected that modern technology would bring their 'ideal designs' within reach (read van de Wall & van de Bom 1954). Candidate-farmers had to prove that they were willing to adapt to those new designs and to modern technology, and experts made sure they hand-picked the very best of them. They intended their selection and direction to prevent the economic failures and human suffering that was certain to follow from a *laissez faire* approach, yet were unrestricted 'modernizers' at the same time (cp. Hofstee 1954). It is a moving aspect of the technocratic faith of officials and experts, that they expected their designs to bear fruit in a higher level of rural life. They were aware of the sturdiness of our farmers, and were convinced that the modern-minded among them were the best, so they had great expectations of the culture that would unfold:

> '*The selection, that presently takes place with the colonization of the IJsselmeer polders, presumably brings with it, that this phenomenon [of the development of the new culture ideal] will manifest itself still more strongly. For, here not only an unrestricted growth of the new culture ideal is possible, but up above that the representatives of the new culture ideal are elected as polder inhabitants*' (Hofstee 1954 as quoted by Constandse 1960 p.83. For agricultural economist Maris and rural sociologist Hofstee see also their contributions to Groenman et al. 1953).

In their opinion, a progressive farmer followed the 'science-based' methods and designs (e.g. Hofstee 1953). 'Rationalization' and 'mechanization' were the leading concepts of experts and officials. At a great distance (in their bureaucratic centre) from local ecology and natural resources, they were no longer able to discern the diverse anthropo-ecologies that, for ages already, had been at the core of sustainable farming systems in their own country. This distance was largely of their own making: after the war they had introduced a theoretical system in which standards were derived from a concept of 'productivity' borrowed from mechanical production in industry, e.g. 'standard hour' and 'labor effect' (as introduced by Maris c.s. in 1951, cp. Hofstee et al. 1959 Ch.III). Though they sensed the distance to the small farmer (Hofstee et al. 1959), this did not induce them to bridge the gap by e.g. postponing theory formation till after they had researched ‘traditional’ farming in its own right. They talked about the small farmer, but not with him. A bad pre-war custom evidently had rigidified in wartime and post-war years.

It did not dawn on the “experts” that the minimal ecology they left for the farmers, together with the enforcement of an industrial-type of agriculture, left no room for the growth of ruralities. In mature ruralities, the anthropo-ecological and the socio-cultural aspects of farming have historically grown into a complex whole. But the choice of candidates used to thinking in ‘industrial’ terms, to the exclusion of those with a small-farmer background, caused the knowledge of ecological factors, natural and communal resources, traditional techniques, and community participation, to be largely lost. In the words of Verschave (1939 p.313):

> ‘*Sans doute, en excluant les paysans modeste, s’est-on privé d’une classe d’individus énergiques, ayant plus que d’autres le sens de la collaboration, et qu’on eût pu aider financièrement par l’intermédiare d’institutions privées, prêtant à un intérêt peu élevé sous le contrôle et avec la garantie de l’État. Une telle solution eût permis à toute la paysannerie d’être représtentée sur les nouvelles terres, sans compter que les capitaux qui affluent actuellement aux Pays-Bas y eussent trouvé un placement*’.

The costs of the new land were anyway so huge that the broad representation indicated by Verschave would have been the normal one: there was no way in which any of the new ‘colonizers’ could pay the real costs (Huygens 1939 p.139 f.). Such a broad representation, with at least a proportionate number of small farmers, anyway was the normal one for anybody acknowledging that the primary expertise on farming was embodied in the farmer population. Yet, the Wieringermeer Authority followed the opposite course, and became an autonomous body of ‘experts’ that, for years, was not even subject to constitutional law (Huygens 1939, Blaauboer 1948). It prescribed crops and farming methods, spatial planning and architecture, etc. etc. War and occupation then strengthened that regime greatly.

The ecological deterioration and product impoverishment, to become a broad characteristic of Dutch agriculture from the 50s on, first took shape here in the Wieringermeer (though the fresh, fertile soil would hide some long-term effects for a time). Before long the socio-cultural life would also start to wither, and the great expectations of experts like Hofstee, the sociologist, would be crushed in the process. In the course of the 1950s and 1960s, Hofstee realized that rural socio-economic life was getting dismantled. And yet, he still hoped for the coming of the Modern Farmer who would be able to carve out ways of Modern Farming resistant to take-over by large-scale industry-like production (Hofstee 1962). But, in the new polders the farmers had been hand-picked for their willingness to conform to Modernity, and distantiate themselves from traditional farming with its anthropo-ecologies and communal practices (note that many farmers in the Netherlands were not willing to conform, see Hofstee et al. 1959). Though those that were selected all belonged to one of the ‘pillars’ of Dutch compartmentalized society, it was Modernity that ruled the minds and actions of these farmers.

So once again Modernity did not stop at imposing rigid, regular and transparent patterns on the *environment*. As Bauman reminds us (2001 p.60), it was already Destutt de Tracy who in 1795 stressed that ‘*It is the task of the ideologist to create a conscious, rational, ideological order*’. The construction of the ‘*claire et distincte*’ environment aimed at the creation of the ‘*claire et distincte*’ *man*. Post-war High Modernity was a belated attempt to create this Modern Man, using the greatly extended means that war and reconstruction had made available to the government. The post-war expert literature devoted to the creation of this

Modern Man is vast, and it is at least equally boring as the expert literature advocating the Modernization of landscape and ecology.

High Modernity's projects are not about life. Due to the fact that nature and history deviated always from the course 'prescribed' by Modernity, Modernity had to be re-imposed time and again by the government's legislation and jurisdiction. In Bauman's words (l.c. p.66), the modern mind

> '*has entertained the project of replacing history with* ***legislation****; of substituting logically cohesive legal norms for the uncontrolled, perhaps uncontrollable, 'laws of history' … The modern mind is legislative reason, and modern practice is the practice of legislation*'.

In this connection Bauman (l.c. p.67) points to the 'hidden, yet notorious totalitarian tendency' of Modernity, explaining as to its strategies:

> '*harmony between wants and abilities could be truly achieved, if at all, only under conditions of concentrated legislative power, ubiquitous and comprehensive normative regulation, and the de-legalization and disempowerment (and in the end elimination) of all countervailing authorities…*'.

It is safe to say that in the East and the West this totalitarian tendency manifested itself, first of all, at the expense of the peasant and small farmer, then also of the poor in urban areas. Its stranglehold became so stifling (as Günther Grass indicated with his '*the false fifties*'), that the West reacted with protest songs, soon followed by the 'student revolution' and 'flower power' of the late 60s. But by then Modernity's engines of production had been heating up, and, in spite of the flower-power idealism, we increasingly came in the grip of a consumerist life-style. Turning a deaf ear to some very clear voices, we were only too eager to believe that Modernity had guided us into the Age of Plenty, in which we could be free by indulging in unbridled consumption. We entered our present culture where regulation got continued, but now in a consumptive disguise. As Bauman summarizes Bourdieu:

> '*needs creation is taking the place of normative regulation, advertising replaces ideological indoctrination, and seduction is substituted for policing and coercion. We may say that the bulk of the population is integrated in contemporary society in their role of consumers, not producers; and integration of that sort can only hold fast as long as the wants exceed the level of their current satisfaction*'.

But that is a make-believe world that is in continual need of the production of new consumers, about which Bauman (2002 p.199) warns '*More often than not, the production of consumers means the production of 'new and improved' fears*'. And the totalitarian regime is still there, but now distinguished as a 'government of consumption' dominated by the Transnational: '*"McDonaldization" would not work unless it was complemented by "Monsantization"*' (Bauman l.c. p.200). When Bauman adds that this consumer society is 'rooted' in '*the anxieties born of and perpetuated by institutional erosion coupled with enforced individualization*', we sense that 'sustainability' is out of reach, both socially and ecologically.

As to agriculture, post-war governments enforced their 'industrialization' by compulsory changes at ecological and community-level. Of course, society has changed since that first introduction, but note that these early ecological and socio-economic changes are still severely hampering any true alternatives in farming. These changes were first of all <u>legal</u> changes, and whoever takes a look at the body of rules and laws confronting the farmer, in countries like the Netherlands, will soon discover that the 'totalitarian' character of these modernist laws is still blatantly obvious. The vast, post-war, social and ecological changes, together with these modernist laws, now determine the 'action setting' (Weichhart) for the farmer. The ‘liberalization’ and globalization of our age is completely dependent upon this ‘action setting’, that thwarts the farmer’s use of ecological and community resources. Modern Man lost his ecological consciousness, but note that Postmodern Man followed in his footsteps. For example, we still believe that as to the physical side of life we can change things by decree:‘*Technik wird meist einseitig als gestaltender Eingriff in die physische Welt verstanden*’, Zierhofer c.s. state (2008 S.147). And they stress:

> ‘*Sich durch Techniken auf physische Bedingungen einzulassen bedeutet aber immer auch, Interaktionen und Kommunikationen auf nicht-soziale Gegebenheiten auszurichten und damit soziale Ordnung durch eine physische Dynamik steuern zu lassen*’.

And surely, half a century of ‘industrialization’ of agriculture, as pushed by the government, obliterated ecologies and rural communities until few were left. Then when the agro-concerns took over, all-out commodification and scale-enlargement were only further intensified. We are left with an ’ecological regime’ (Zierhofer et al. 2008) that squares (a) with ecological revival and (b) with the renewed growth of ruralities.

Thesis 1

The ecological regime that dovetails with industrial agriculture – as a forcefully imposed design from the government’s drawing boards - has few roots left in agro-ecologies of proven viability. To the contrary, industrial agriculture is intimately connected with the accelerated urbanization and the hypermobility of the past half-century.
Industrial agriculture (a) causes the profits of the farmer to plummet (b) parasitizes on what is left of local ecologies and ruralities for its crop growth, yet (c) effectively dis-empowers the farmers in their maintenance and development of agro-ecologies, and in their building of ruralities.

11.2. Lacking substance

Compounding all this is the fact that our modern experts, when constructing the present ecological regime, were speaking a **professional language** that was at odds with a healthy relationship between farmer/farming and the ecology. Convinced that their new, centrally devised farming methods were the way forward, the experts focussed their rural sociological research primarily on the question why farmers did or did not accept the new methods brought to their attention. Consider Hofstee’s sketch of the post-war Dutch situation (Hofstee 1953 p.61 f.; Hofstee was close to Mansholt the initiator of the EU Common Agricultural Policy, see e.g. Mansholt 1959 and Hofstee 1959):

> ‘*Especially since the Second World War, from the side of the Department of Agriculture people did their utmost to promote an accelerated development of Dutch agriculture by way of education and extension. Now for sure important results have been attained, but, on the other hand, there was considerable opposition too. In certain parts of the country and with certain groups of the farmers population, new and better agricultural methods are being accepted far slower than is desirable*’.

A true expert would, first of all, acquaint himself with the local farmer, the ecology, and the community, and he would only develop new approaches in close cooperation (§11.7, on Carl Sauer). But post-war publications are silent about it, although before the war the importance of such acquaintance and cooperation had been acknowledged by many members of the older agricultural advisory network.

Government-promoted **agricultural extension** experienced a boom after the war, especially from about 1950 on. Its extensive use of modern media (e.g. film) made it easy to overlook that it lacked an essential characteristic of the old network of agricultural consultants: the direct contact with the local farmer, leading to a two-way exchange of knowledge and experience (cp. representative Louwes' praise of those consultants, Louwes 1950 p.432). The renewed Extension Service prided itself that its modern media approach was sure to bring an accelerated acceptance of 'advanced methods'. Only too late it dawned that this approach was hampered greatly by its American origins (advertising; war propaganda), that made it unfit to 'handle' those types of knowledge and experience that are 'local' in essence (like the knowledge of real people and soils).

With government and its experts convinced that the S & T from their central institutes was pointing the way forward, in regard to the 'esssentials' of agriculture farmer and ecology were hardly considered an essential source of information. Next the accelerated development of centralized research and centrally directed extension – that both had the connections with farm and ecology cut - made sure that the new experts spoke a professional language that was at odds with 'traditional' agriculture and its farmer, the ecology, and the community.

In this connection the decision to shape rural sociology after its American example was a historically contingent decision with ominous consequences. For as Rösener pointed out (1994 p.17), '*Villages such as Europe had known them since time immemorial did not exist in rural America*'. That is, the close connection that had existed for ages between village, sedentary agriculture and local ecology in Europe and elsewhere, was largely unknown in the American 'frontier society'. When yet during the Late New Deal research unearthed some farming systems within the US that had much in common with 'traditional' agriculture as known from other continents, political forces ensured its suppression during the war. When Georgescu-Roegen later approached agricultural economy on the basis of his broad knowledge of village-centered agriculture (esp. in Romania), his fellow economists did not follow him. Neither did his colleagues in American social science at large, convinced as they were that 'just like natural science' their own science was independent of time and place. Before long, social scientists in Europe followed this American example.

We touch here on the **dominance of scientism in American social science** in the first half of the 20th century, its history well described by Dorothy Ross (1991). Convinced they were emulating natural science, mainline social scientists in fact had no notion at all of real-time physics and chemistry, but followed the mirage depicted by e.g. Karl Pearson in his 'Grammar of science'. The results were hardly encouraging.
Because they eschewed historical-comparative analyses, they lacked distance to their own American society. Ross summarizes the results (l.c. p.472 f.). '*American social science has consistently constructed models of the world that embody the values and follow the logic of the national ideology of American exceptionalism'.* And she continues: *'The most striking outcome of exceptionalist history has been scientism itself. The aim of scientism has been to establish prediction and control of the historical world'.* [These social scientists assumed determinism after it had been laid to rest in the natural sciences, e.g. by Kohnstamm]. *'Blind to what cannot be measured, they* [the 'quantitative methods'] *are often blind to the human and social consequences of their use. The manipulators of social scientific technique, intent on instrumental rationality, cannot notice the qualitative human world their techniques are constructing and destroying*'.
In the Netherlands Kohnstamm criticized the 'quantitative methods' and its specialists (esp. De Groot), yet shortly after his death the new wave of institutional research appointed De Groot 'methodologist' of its version of social science. 'Measuring the non-measurable' became standard practice in the Netherlands (and in Europe at large).

In the Netherlands, Hofstee in post-war years had to gain recognition of rural sociology as an academic discipline, facing government officials and 'leading' economists who up till then had considered agriculture in a narrowly economic way (taxes!). In his inaugural address (1946), Hofstee tried to define the sociological research field in order to escape from this 'economic imperialism'. But, he largely followed the example of the American sociologist Parsons, who in the years before the war had constructed a 'general', a-historical, sociology (as an express complement to Robbins' equally a-historic construct of economics, cp. Hodgson 2001 p.184-203). With its a-historical character, Parsons' sociology implied a rupture with the work of e.g. Weber and Mannheim (even with that of his compatriot Veblen). When for some decades Parsons' work was widely accepted as authoritative, this was due to High Modernity's lack of historical sense (not to this work's inherent qualities).

Notice further that (western) social science at large had been weak in dealing with nature and the ecology. Dissatisfied with the crude social Darwinism at the end of the 19th century, social scientists had preferred to use social arguments only. Thus, not prepared for High Modernity's lack of ecological links, sociologists like Hofstee endorsed a full-blown Modernization, based on the hope that this was the way to train strong farmers, who would guarantee a burgeoning rural, social life. But note that for decades Hofstee put his trust in High Modernity, not in traditional agriculture in all its diversity. He was far from indifferent to this rich diversity, yet was at times pessimistic about its future, because as a modern man he was of the opinion that (Hofstee 1962 p.268):

> '*In agrarian quarters one still under-estimates, in my opinion, the possibilities that specialization, division of labour, and systematic scientific control offer for the replacement of love for the industry, personal devotion, and 'Fingerspitzengefühl' that are strong qualities of the common farmer*'

For some decades experts like Hofstee endorsed the 'modernization' of agriculture and rural regions, even though they realized that beauty and diversity suffered from it. Yet, they were slow to detect how extensive the deterioration was of community and ecology, in spite of the fact that others had noticed it for years already.

> Hofstee 1972 gives a broad exposition, in some aspects linking up with the Club of Rome Report, yet, he shows no consciousness that the problems are **part of our very system** of industry and industrial agriculture, and shows not a trace of doubt that 'environmental control' **within** this system is feasible. Only later, when the systematic character of the problems starts penetrating, Hofstee still tries to change the focus of his research.

11.3. Taking the world for a test tube

Why then did it take such a long time for Hofstee and his colleagues to receive the signals that others saw a long time?

Quite decisive was that the dominant reductionist approaches to research and policy made the reception of signals difficult, if not impossible. Remember the example of stable manures with their many soil fertility-building characteristics, that yet had their 'standardization' promoted in terms of (only) the mineral-N delivered by it. This was essentially a negative 'standardization', because it did nothing to express the manifold soil fertility-building qualities of stable manures. Mainline research lost the concepts, the very language, that it needed to think about soil fertility and its enhancement by traditional agriculture… So there were reasons *internal to their system* why many experts, trained only in reductionist S & T, were at a loss with those adverse signals. They literally 'made no sense' to them, even when they perceived that something was gravely wrong.

Furthermore, our experts had not just been individually educated in reductionist science, but as professionals they met reductionist approaches everywhere. These were the approaches chosen by government, that wanted to be sure it could exert its powers with the means the experts could provide, with only reductionist science sounding that promises. Within this reductionist paradigm, farming, soil and ecology seemed wholly perspicuous and therefore were amenable to total reconstruction, based on designs made in the government's centres of expertise.

When environmental problems were reported, the government's experts still approached these from their disciplinary paradigm and with reductionist means. This even led to a system of **toxicity testing reflecting standardized laboratory systems**:

> '*However, all of this standardization meant reduction of variability and, thus, less and less environmental realism. Somehow, the fact that these environmentally unrealistic test containers and limited array of test species were being used to predict what happened in complex, highly variable systems escaped the attention of the regulators and decision makers. Thus, because of the drive towards standardization, the connections to natural, highly variable complex systems not amenable to standardization are markedly diminished*' (Cairns & Pratt 1995 p.72/73).

As a result, one of the primary ecological problems of the age, the precipitous losses of amphibians (Laufmann 2008), for a long time had a contested relation to 'industrial' agriculture. As Rohr et al. (2008) summarized at the close of their publication:

> '*Notably, the potentially important nexus between amphibian parasitism and pollution demonstrated here would not have been detected in standard studies used to register chemicals in the United States and Europe because these studies are typically conducted on individuals isolated from other species, such as their parasites*'.

The fact that '*most science is reductionist and only a small percentage is integrative*' (Cairns & Pratt l.c.) is without any doubt closely connected with the government-directed growth of S & T and with the introduction of reductionist science studies in secondary and higher education in post-war decades.

The expert must leave his government-provided (or TNC-prescribed) ivory tower and descend to the level of peasant and ecology – paradigmatically as well as practically – to re-establish contact. He will have to (be)come 'down to earth', for too much of his present expertise is not geared to real life, but to well-isolated process facilities.
In the words of Weichselgartner (2006 S.23):

> '*Wenn Nichtlinearität und Komplexität die vorherrschenden Charakteristiken von Natur- und Sozialzuständen sind, welche Konsequenzen hat dies für die Theoriebildung und die Objektauswahl der Forschung?' … 'Wenn die schulische und akademische Ausbildung durch Linearität geprägt sind, natürliche und soziale Umwelten aber vorwiegend durch Nichtlinearität, wie kann dieser Antagonismus durch geeignete Wissensprogramme aufgelöst werden?*'.

Limited predictability is at the core of chaotic systems, including that of the tides (!) (Terra 2005). '*Fluctuations over several orders of magnitude*' despite constant external conditions are characteristic for chaos in food webs (Benincà et al. 2008 refs). In short, 'linear science' is of limited value indeed in agriculture and ecology.

With even the pendulum easily induced to chaotic behavior (as related in Ch.1), it is clear that also within mechanical systems we have to maintain narrow specifications, if we do not want to cross over into complex behavior. For there our 'industrial' means of control fail (e.g. feed-back control), and the limited scope for new means of control requires an approach that is completely different. Computers did not change anything here, for

> '*Leaving the limitations of computers aside, it is impossible to collect all the data needed to characterize a system exhaustively; that is, without any degree of error or uncertainty creeping in. In turn, that uncertainty rapidly blows up when systems iterate within themselves*' (Peat 2002 p.131)

In fact explorations in **chaos theory** make us aware that industrial control (e.g. feedback control) itself is embedded in a system that is more easily chaotic in character than we are ready to admit. Polanyi 1957 gives many examples of 'the same' factory, when built or even re-constructed in another region or country, failing to operate as expected. That means that the control attained in the original location was not of a global, but of a local kind. And it is chaos theory that makes us aware that, quite generally, many practical engineering problems allow a local solution, even where 'classical' control rules (e.g. feedback control) do not apply, because they presuppose both a loose coupling of the parts of the system and an uncoupling from its environment that is not strictly valid for the factory system at hand (cp. Giona et al. 1994).

The systems that can be steered in a technical way, with the help of 'proportional interventions' (e.g. linear feed-back), are those that experience has taught us to keep within narrowly defined bounds. That is the reason why manufacturing at large is situated in the 'loose coupling/linear interactions' quadrant of an interactions/coupling chart, evading the quarters with tight coupling of system components and/or complexity (Perrow 1984 fig. 3.1). Taking these facts to heart, any real-time technologist uses small steps, accompanied by exhaustive testing, in up-scaling or otherwise extending a system. Systems that run out of control are only too well known in the history of technology, and we learned the hard way that

> '*Although it is always possible to adjust and fine-tune a linear system, things are entirely different when it comes to non-linearity. In certain regions of behavior the system may respond to a corrective manipulation; in other regions a small correction may push the system in an unexpected direction*' (Peat, l.c.).

With manufactured systems that are both complex and tightly coupled, e.g. nuclear power reactors, there remains a risk that they will spin out of control in unsuspected ways (Perrow 1983, esp. Ch.3 '*Complexity, coupling and catastrophe*'). Where loose coupling can be (re)introduced and complexity reduced sufficiently - e.g. in oil plants which grew more and more complex in the course of their history - operator control can be re-established provided that (Hopkins 2000 esp. Ch.11):

> (a) '*maintenance departments … become central locations for organizational learning*',
> (b) the focus will be on the failures and not on presumed success
> (c) proper and well-trained staff is locally available because experienced people are needed to spot small and initial failures and to deal with possible break-downs.

So even in an industrial setting, process control is only regained when both maintenance personnel and operators with ample experience are re-introduced. Here the compatibility with other stories of 'good technology' (cp. those of Petroski) is remarkable. It is clear that there are some close similarities with the way in which traditional farmers handle agricultural system complexity, and yet, the government denies them the key-position they deserve in the agricultural process.

> The present Recession maybe will initiate a change. Management strategies that only now can be scrutinized, because of their evident failure, have for years been eating away even the maintenance personnel in our traffic and communication infrastructures, calling 'redundant' what in reality is strictly needed. We are faced with the consequences of managerial strategies that pretended 'monitoring' could replace 'management by walking around'. Loosing contact with life at shop-floor level, they erected castles-in-the-sky.

There are definitely important differences too, between agriculture and good technology. First of all because systems connected with life are complex in distinctly other ways than technical systems (e.g. Ricard 1999, West 2006):

(a) they are usually (fractal) hierarchies, with immature precursors that grow in complexity from the inside out

(b) there is an increase in stability that derives '*from the nesting of systems within systems*' (West 2006 p.86; it pertains also to e.g. human health)

(c) maintenance and renewal of complexity is also accomplished from the inside out and is based on carefully system-regulated flows & partition of energy and materials.

That is, the *substance* of these life systems is non-technical (take the growth of the cellulose fiber hierarchy, with its superior humidity behavior, as an example). And their maintenance doesn't require deconstructing the system and working on its parts, but it requires the whole directing (in vivo) the examination and maintenance/renewal of the 'parts'.
As both their substance and their maintenance are non-industrial, efforts at all-out industrialization of life-systems betray ignorance.

> Even an industrial bio-reactor in essence leaves growth and maintenance to its microbial inhabitants (which as a rule have been 'disabled' for the industrial purpose). But of course, when we really want e.g. soil microbial life, we need a functional soil system ('nested systems-within-systems') for its growth and renewal and not a bio-reactor.

Note that even *enzyme systems* in cells and organs are life systems. E.g., when situated on membranes they *show*

> '*multiple steady states and oscillatory dynamics'* that are not *'the consequence of the properties of an individual enzyme, but rather of the spatial organization and the complexity of the living cell. Compartmentalization of the cell and the occurrence of metabolic processes at the surface of membranes may generate instabilities and oscillations that rely solely on the existence of this compartmentalization*' (Ricard 1999 p.290).

The study of 'purified' enzymes in solution makes sense only when the researcher is able to re-position the enzymes in the system of which they are a 'part'. A close study at systems level is indicated, to decide about the importance (or lack thereof) of studies of 'purified' enzymes in solution.

> Thesis 2
>
> Where the farmer essentially works with life systems, he must be an adept in the care of their *in-situ* growth and maintenance.

Furthermore, his creative exploration of alternatives hinges on the cautious and prudent try-out of new combinations of local life systems that are non-technical to the core (think also of heterogeneity and diversity).
Corollary: the concept of 'industrialization' of agriculture is at odds with both the maintenance and the development of agricultural resources. It was flawed from the start.

Notice that controling agricultural soil and plant growth from a remote centre never functioned adequately. It was self-suggestion, entirely dependent on mentally transforming the soil into some technical or laboratory-system. Central to this mental transformation was the 'shrinking' of the soil agro-ecology to a mineral nutrient solution. It never worked, but it took nearly a century of failed efforts to get back from this laboratory solution to the soil. There was, in fact, never a justification for our presumption. But we learned two things:

First is that the 'agricultural' research that is based on industrial (laboratory) principles is of uncertain value at best. **High Modernity's 'industrialization' of agriculture has clear elements of tragedy, and part of that tragedy is that much of its 'agricultural expertise' is not about real-life soil, plant growth, and farming**. The after-effects of the failure of 'precision agriculture' are still very much with us.
Second is that we missed out on the great majority of the natural resources that the farmer has at his disposal. If we acknowledge and accept this fact, we can as yet leave High Modernity's 'NPK-cage' and enter the multi-dimensional world of the peasant and the small farmer. This means that we enter the world of their ruralities, where care gives access to, and builds, rich resources. **Because these resources, and the agro-ecologies in which they are embedded, are not perspicuous (reductionist approaches do not apply), careful hands-on approaches can open up perspectives that remain hidden from the distant expert who pretends he can 'act from a distance' (equating rationality with de-relationality).**

11.4. The dream of the age

In post-war decades there were clear signals, coming from both soils and societies, that the world was essentially different from the world in our Modernist Dreams. But within the Modernist paradigm these signals were interpreted as proof that these soils or societies had not yet received the scientific treatment they needed to become Modern. Import of 'modern scientific agricultural technology' from the West led to disappointing results in the tropics, yet Kellogg's reaction in the mid-sixties was:

> '*In fact, millions of cultivators … are using soils that never have been examined scientifically. No one knows where they belong in a system of soil classification or how they could be expected to respond to any system of management besides the local one in use. This lack greatly handicaps the use of new technology by cultivators …* ' (cited in Myrdal 1968, II, p.1253).

That careful soil studies up till then had indicated the *historical* character of local soils, defying any reductionist 'genetic' system, did not register. Kellogg c.s. were sure that the goal of perspicuity was within reach in 'advanced' countries and that the 'underdeveloped' countries just had to follow their example. Then, when careful research confirmed that soils within 'advanced economies' were no less resistant to the reductionist approach than those in 'underdeveloped countries', this careful kind of soil research fell from grace with our modernizers, but they did not change their paradigm. Surely the 1960s were an ill-fated decade…

Yet, careful research in the US itself, as distinguished from ‘applied research’ directed by the fertilizer industry, had shown at an earlier stage that fertilizer application often brought disappointing results.

> One of the reasons was known from Winogradsky’s authoritative warnings in 1927: applying mineral-N fertilizer means the end of the soil’s own biological nitrogen fixation. The nitrogen syndicates of France, Italy, Norway, England, and Germany had reacted with an attractive ‘Second International Nitrogen Conference’ on a cruise in the Adriatic, where everybody could conveniently forget these inconveniences – cp. Baer’s proud report, 1928. By then industry everywhere had followed the German example and established its own agricultural stations, that focused at the exclusive use of industrial fertilizer (Staatsmijnen in the Netherlands established such experimental fields as soon as is started with N-fertilizer production, at the start of the 30s). Bear was Dir. of Agr.Res. for a big fertilizer company; after ’40 he was a central figure in the exclusive focus on fertilizer in agricultural research.

When after WW II the same disappointing results of fertilizer showed up in the tropics, ’*a fact apparent to any visitor in the villages*’, (Myrdal l.c.), our western advisors still stuck to their reductionist image of the soil. For example the 1964 UN ‘Economic Survey of Asia and the Far East’ stressed (cp. Myrdal l.c.): ‘*Soil analysis must be made in different parts of a country before the correct types and dosages of fertilizers can be prescribed*’. Governments everywhere had proclaimed ‘industrial’ agriculture the door to the realm of plenty, and a crowd of enthusiastic experts had been recruited for its design and implementation. Any research paradigm would have been difficult enough to change - but here we had a paradigm that was protected by law and institutionalized to a high degree.

Yet, in those very same 1960s some very perceptive reviews were published, to wit reviews on (a) the risks of high-fertilizer maize hybrids breeding (b) nitrate accumulation and nitrosamine formation (c) erosion and eutrophication (d) pesticide & fertilizer effects on propagation of birds & amphibians (e) the meagre results of oil-based, high-input agriculture for the food situation (as compared to the farming systems it displaced) (f) the displacement of great masses of people from the land to urban slums. Quite enough to prove the dangers of ‘industrial’ agriculture.

> Desai and Pillai in their ‘*Slums and urbanization*’ write (1970; Preface):
> ‘*After the Second World War, the Third World consisting of the newly-liberated colonial and semi-colonial countries have launched a concerted programme of industrialization and urbanization… The rapid growth of big cities in the Third World has ‘bred those refuse dumps of human misery known as slums’ in Calcutta, Delhi, …., Kuala Lumpur and other cities. These slums are spreading like white ants.... What is ironical is that the very economic and social planning pursued by these Governments, instead of counteracting the growth of slum conditions, are aggravating them. Studies … reveal that the pattern of city growth … is exhibiting an ever-increasiung deterioration of the total material, social, and cultural atmosphere*’. Their thorough publications proves quite well that the official policies, instead of bringing some light, aggravate the problems. Note that by 1970 research into the problem is well underway for years – Lewis Mumford is an authority for decades already – and has resulted in e.g. some penetrating UN documents on the subject. Doubts about the inability of industrialization to absorb the displaced rural multitudes are clearly voiced – and recent research proves that things have grown only worse, e.g. Breman & Shah’s ‘*Working in the mill no more*’ (2004). Desai and Pillai stress the ‘*gross distortion in our sense of priorities*’ (l.c.) seeing the enthusiasm displayed at the goodwill tour of the Apollo-11 moonlanders in comparison with the lack of effective attention to the slum problem. This is true enough, yet, we need to probe further into the ‘dream of the age’ that effectively paralized action.

Note in this connection that the ‘industrialization’ of agriculture is a **core project of post-war progress** itself. It was ‘unthinkable’ that it could fail, for then we ought to think it all over again. Now ‘industrial’ agriculture is doomed without ample fertilizer. If doubts are allowed here, what about post-war progress itself?

The denials that followed the reviews referred to above are in a way reminiscent of those presented by the mainline industry in the case of asbestos and tetra-ethyl-lead. Yet there was a major difference. While governments in general were not at ease with the ‘disproval of progress’ that the indictment of e.g. asbestos implied, these specific subjects were not a core element of their policies, but the ‘industrialization’ of agriculture was.

> Who dares to doubt the rationality, not just of the application of tetra-ethyl-lead in petrol, but of the fleet of private cars and of our massive road infrastructure? It took decades for the government to forbid the application of tetra-ethyl-lead, and yet this component of ‘high octane fuel’ does not even touch on the crucial subject of the role of the private car (plus road infrastructure) in transport and mobility. But of course, mineral fertilizer is not a ‘fuel component’, but the ‘fuel’ itself of industrial agriculture. So who will indict our fertilizer use, and with that the rationality of ‘industrial’ agriculture?

‘Plenty of cheap food’ was the promise of agriculture’s ‘industrialization’, and governments and their citizens were greatly pleased. With the government and its experts professing that this ‘industrialization’ was a prime example of ‘rationalization’, they could hardly avoid decrying critical discussions and evaluations as tokens of irrationality. An ideology could come of age that was at least as strong as any that had been prevalent in previous centuries. As with every ideology, there were some partial truths at its core, and as was the case with so many previous ideologies, there was a government that forcefully spread its ‘gospel’ – that contained an embarrassing pretension: **during the 1960s the governments, as united in UN/FAO, pledged to secure fertility and food for their nation**.

> Jacoby, after nearly two decades of hard labor on land and tenancy problems, in his ‘*Man and land. The fundamental issue in development*’ (1971) offers not only a valuable overview of post-war decades, but also an insider’s view of the efforts of FAO and UNCTAD officials in those years. His account and analysis of Technical Assistance (agricultural development) is required reading for both experts and policy makers.
> He notes that (p.13) ‘*The discomfort of thinking about social issues, combined with the simple attraction of new ways of production, have together induced many economists, and still more politicians, to slide into wholehearted support for the new technological approach. At times, it seems as if technological fantasies monopolize current economic thinking. Agricultural development, envisaged only fifteen years ago as a process of socio-economic advance, has been narrowed to the scope of increased agricultural output, while man… seems gradually to be losing his relevance to agriculture; and this, despite the fact that in the underdeveloped countries there is no altenative place for him in urban industries and services*’. The enthusiasm for e.g. the Green Revolution derived, at least partially, from ‘*the discomfort of thinking about social issues*’ …
> But note that Jacoby himself partakes of the ‘dream of the age’ in that he does not question the ‘attainements’ of post-war agricultural research. When accounts are brought forward about the inverse relation between ‘gross output per acre and size of farm unit’ he rejects these indignantly (Ch.3). He has his hopes fixed on the ‘industrialization’ of agriculture, no less than the policy makers he (rightly) criticizes.

Even in the 1960s there were people around crying that the emperor was not wearing any clothes. Yet this time the clothes had been massively institutionalized already and all that the public could see were big institutes where crowds of experts performed their impressive rituals. In fact by then perceptive minds (e.g. Godfried Bomans in the Netherlands) candidly expressed their opinion that this 'rationality' was alien to life itself. But for the less astute observer the impressive institutes certainly harbored important secrets.
The experts working there were severely restricted by the tenets of their paradigm, up till missing out on the perfectly normal symbiosis, that with mycorrhizae, as well as on agro-ecologies at large. These experts' rationality was functional, not substantive: they produced whatever was requested. What is more, sharing the same ideology with their superiors, they in general did not doubt their assignments. With their paradigmatic labor and functional rationality, they definitely were Modern Men (!).

> Ingrid Palmer's UNRISD report '*Science and agricultural production*' (1972) is a rare exception: she is willing and able to discern problems at the core of the Green Revolution, e.g. (a) disease proneness due to N-fertilizer (p.65) (b) pest pressures, e.g. that of the brown planthopper (p.72), soon to become manifest in Indonesia (c) yield decline of the HYVs that by then is manifest already in Pakistan (p.82) (d) seed degeneration (p.85).
> Yet, even she is captive to some paradigmatical 'self-evidencies' of mainline agricultural research, e.g. (a) as to plant nutrition, factual substitution of soil by a mineral nutrient solution (p.25) (b) the expectation that soil classification will soon be able to provide 'soil indexes' that can help determine fertilizer gifts (p.33) (c) the assumption that, thanks to modern methods of measurement, 'precision agriculture' is soon to be realized (p.43). There is a distinct absence of soil microbiology (e.g. mycorrhizae) and rhizosphere biochemistry (e.g. exchange of root exudates) in her report and an apparent oblivion in regard to soil (micro)morphology. That is, Palmer's confidence in the Green Revolution, in spite of its massive problems, stems from a research paradigm that suggests our ultimate power over soil and plant *because* it is oblivious to real-life soils and plant nutrition. A neat world has been projected that is amenable to manipulation with our industrial means, yet, it is a world disconnected from the real life of soils, plants, and farmers.

Still those experts were humans, and so at least co-responsible for important aspects of their life and labors. For everybody to see there was something that was bad in the new policies that dispelled the small farmer/peasant from the land. Exactly here we see these modern experts display less positive tendencies. Solemnly declaring that traditional agriculture was a thing of the past, they never deigned to take the peasant/small farmer's practices and resources seriously. They were not even able to assist him in the development of those practices and resources, yet, they were quite ready to cut off the small farmer's access to his local resources and to displace his practices.

11.5. Total war

The parallel with the more aggressive colonial agricultural policies, or with discriminating practices of big landowners in the South of the US, is a close one. In regard to the Third World, Jacoby points to the fact (l.c. Ch.5) that

> '*for the peasant population*' the direct as well as the indirect '*system of colonial penetration meant concentration of land ownership, sharecropping, unsatisfied credit needs and the vicious circle of usury, indebtedness and permanent bondage which turned many small owners into tenants and tenants into landless agricultural laboureres*'. Next, political independence hardly changed things:

'*The process of decolonization may be interpreted as a return to the* <u>*indirect rule*</u> *of early colonial periods since, now as then, Western business interests operate more or less amicable trade relations with the ruling élite in underdeveloped countries. … With a few exceptions, the new governments seem less balanced and less efficient than the former colonial administrations though certainly no less eager to seek their own advantage through cooperation with outside economic and political forces*'.

Note in this connection that many a colonial expert formerly used to accept and apply the expertise of traditional farmers in the colonies (e.g. King, Timmer, a.o.), whereas the Modernists rejected it.

Post-war 'agricultural development' started wholeheartedly from the concept of centralized research and expertise – to which the peasant had to submit (under capitalist, communist, and non-committed regimes). It is this change-over to extreme centralization of 'expertise', and its concomitant denial of local expertise (with its relation to local self-government), that is a distinct characteristic of the post-war system. Its sudden appearance derives from its origins in wartime and hits peasants in the warring countries before it reaches the peasant populations of non-belligerents. For it is from e.g. the USA, England and the Netherlands that this denial of local expertise - 'legalizing' an ongoing dis-empowerment of the peasantry - is 'exported' all over the globe.

Consider the Netherlands. There after WW II government and its experts refuse any contact with the small farmer. M.P.'s insistently are questioning Mansholt about his small farmer policies, but he feels strong enough not to consider their proposals. When Engelbertink 1948 reminds the minister that no use has been made of the extensive material collected about the small farm, Mansholt refuses to initiate any research. Schilthuis 1948 points to the fact that the scale of production in agriculture is hardly correlated with its efficiency, or its socially desitable character: 'returns to scale' are only too often correlated with usurpatory practices. Yet, the socialist (!) minister not only keeps silent on the issue, but maintains Occupation Ordinancies forbidding e.g. self-marketing of e.g. milk and meat.

Small farmers – the big majority – because they work their land themselves (a) are excluded from the Farmers' Works that, at low cost, would help upgrading the local rural resources and economy, and (b) ask in vain for (cost-neutral) adaptation of the government Service placing the unemployed into Public Works to the needs and possibilities of the small farmer and rural communities (e.g. Mansholt 1949). So when income fails because the fixed prices have been set below true costs (cp. den Hartog 1948), they have to transfer to large-scale re-allotment (and similar) projects, at a great distance from home and for at least six months at a stretch (facts stressed by e.g. M.P.'s van Dis 1948, Haken 1948).

Those policies are less destructive to the bigger farmer, yet also he perceives at times that things are amiss, as is evident from some remarkable clashes between the Foundation of Agriculture and the Department of Agriculture. But before long, the minister is consulting only his co-believers in 'modernization' of agriculture: the 'Green Frontier' is born.

As to the roots of its all: during World War II agricultural policies were one-sided to the extreme, amounting to complete transfer of power to the centre. We already looked at (a) the wartime imposition of industrial agriculture in England (Ch.1) (b) its mid-war 'victory' in the US disempowering any and all other approaches (Ch.9), and (c) at the totalitarian organization of agriculture and the food system imposed by the Nazis, both in Germany and in the occupied countries (Ch.10). In post-war Europe the need for a directive food economy was translated, quite generally, in transfer of wartime regulations into peace time. For example, breeding remained strictly forbidden to the farmer: the wartime centralization of 'breeding power' was maintained forcefully (in France it henceforth stayed with the Comité Technique Permanent des Semences that had been established in 1942).

Wartime regulations in the Netherlands (e.g. the 'Kweekersbesluit 1941', Staatsblad No. S.700), were completely restrictive, with complete transfer of power to e.g. the Secretary General. There is no doubt that they expressed a distinct change-over to Nazi 'law' with its Führer Prinzip, yet, after the war government bureaucrats pretended these were expert-centered laws/regulations. Already Ordinances of the year 1940 (S.701, 711, 718) leave no freedom to the farmer. Then Ordinance S.735 (Jan. 1941) removes any loopholes: the farmer with all of his possessions and activities is under complete government control, with heavy punishment for any infringement of the 'law'. **Note that there is nothing that can compare with the totalitarian character of this system** (e.g. the relation of feudal lord and serf left a good measure of freedom to farming proper).

Yet, the most repressive manoeuvre comes after the war. Then the bureaucracy, under pretext of food scarcities and the needs of distribution, manages to re-introduce a whole list of wartime ordinances (Staatsblad F.157, 162, etc, Sept./Oct. 1945; the re-introduction of the Kweekersbesluit is part of F.162), repealing their previous striation (F.101 etc). In November 1945 the **re-introduction of the system of wartime control and punishment** completes this re-introduction of occupation 'law' (Staatsblad F.284). With its wartime control apparatus it 'legalizes' the far-stretching interference with the practices of the common farmer that the government next will display for more than half a century.

> Food scarcity was real, so there were good reasons to re-establish close control of food distribution, and the Military Authority in the liberated part of the Netherland had indeed issued such an Ordinancy in september '44. In fact Unilever was among the first to try to take advantage of a 'liberated food economy', and the condemnation of a number of its managers by the Court of Justice in the Hague brought relief to both farmer and common citizen (Ned.Jurispr.1946 No.602). The sting was evidently not in this temporary control, but in the totalitarian character of the occupation ordinances. With their complete transfer of power to the centralistic bureaucracy (Führer Prinzip) they (a) choked the local farmer's initiative and negated his development of local resources and (b) put 'expertocracy' in its place. Especially the small famer was hit by it, because his specific strengths were all in the development of local resources (de Hoogh 1932 is a careful study). Note that the centralistic character of pre-war agricultural relief measures (issued because of the Depression) had initiated a discussion at all levels, with near-unanimous rejection of centralism and strong advice for decentralization as a result (Committee prof. van Loon, cp. Minderhoud 1943 p.518). The Nazi ordinances embodied a complete denial of this general opinion, and their maintenance initiated half a century of rigid top-down policies by the Department (cp. Bekke et al. 1994).

In a similar way, the common concentration policies in the occupied territories would transfer to post-war decades, e.g., when the Nazi policy of cutting university research, and financing research in central institutes instead, was maintained after the war. That occurred in the Netherlands when the government economized ruthlessly on education and research at Wageningen University for years, while research within the new institutes was well-financed (the repeated complaint in Parliamant).

As a result any ambitious professor planning extensive research was restricted to one of those institutes - where research policies were under supervision of the Department of Agriculture or run by the fertilizer industry. And so Schuffelen concentrated at the use of feeds that had been obtained with high fertilizer gifts. That was not just because he wanted to limit himself to industry-like product research (embodying functional instead of substantial rationality). He also refused adamantly to consider 'traditional' practices, biological nitrogen fixation, etc. (Tideman 1975 p.16). Schuffelen's research, the only type allowed by the Department of Agriculture and the fertilizer industry, was not just limited in scope, but **negative in character**. From the start presented as leading to feeding practices superior to those of the traditional farmer, it left no room for self-criticism. When problematic effects showed up, there was no reconsideration of assumptions, methods, and results. At the start Schuffelen c.s. had refused to consider long-known problems of ruminants with feeds obtained with high fertilizer gifts (e.g. Sjollema 1942). Now they refused to look failure in its face.

Thesis 3

Some of the most striking political and institutional reasons for the post-war growth of 'industrial' agriculture are:
(a) the rejection of the knowledge and practices of the small farmers (the great majority), especially by the war-time regimes;
(b) the centralization of agricultural & food industries rooted in the same war economy, afterwards set down partially in law, and subsequently implemented by transnationals;
(c) the accelerated growth of a centralized (unbalanced) research & expert circuit, again with important roots in the war situation, and directed by the government and industry;
(d) the premature introduction everywhere, along UN/FAO channels, of hybrid maize responsive to high fertilizer gifts;
(e) the choice of governments everywhere to implement the principles of High Modernity, with the industrial approach to agriculture at its core, leaving no room for evaluation.
These and similar political and institutional factors suffice to explain industrial agriculture's 'success'.

As to the Third World scene, Myrdal's extensive review shows clearly (Myrdal 1968 esp. Ch.26 '*Agricultural policy*') that in the 1960s

(a) '*The hope … that a large proportion of those who will join the labor force in decades to come will become productively employed outside agriculture is illusory. … the absorptive capacity of the non-agricultural sectors is severely limited*' (l.c. p.1242)

(b) '*An increase in labor input … would raise yields, even without any technological innovations or additional investment. An indication of this comes from farm management studies that regularly imply that yields per acre are inversely related to farm size*' (l.c. p.1254) [Nevertheless]

(c) '*General statements outlining plans for raising agricultural production are completely taken up by descriptions of technological reforms. Reports by Western officials and economists, particularly those who have not done actual field work in the villages, also tend to focus on technological problems. Western, like South Asian, governments continue to support resolutions in F.A.O conferences and other similar inter-governmental assemblies urging land reform, but this is only a gesture*' (l.c. p.1257).

In short: by then it was clear that focussing on (Western-style) technological reforms was not needed to raise agricultural productivity, but that it for sure would lead to displacement-without-perspective of great crowds to the urban slums (Myrdal 1968 §10.11 '*Urbanization*'; Desai & Pillai 1970; Mangin (ed) 1970; Breman 2001). For governments, nevertheless, to focuss on technological reforms was easy, giving the impression of effective policy without getting entangled in the problems of institutional reform.

But note: it meant an immense **shift in resources**, from local natural resources and the knowledge and experience of local people, to the use of outside resources. Governments envisioned they could direct the use of the new resources 'for the common good'. But where is this 'common good', when this resource shift (1) displaced crowds of people (2) lead to the loss of their resources, and (3) aggravated the nations' dependence on external economic actors?

Favellas/slums: by the end of the 60s it was evident that de-ruralisation/urbanization concentrated an immense number of people in slums, without any perspective. For example, from 1957 to 1970 Lima (Peru) saw its percentage of slum dwellers increase form 9% to 40%. Likewise, Lusaka (Zambia) saw its slum dwelling percentage of inhabitants increase from 9% to 47%, between 1962 and 1969. Yet most policy makers by then had been overcome by the High Modernist dream, in which agriculture's 'industrialization' is coupled with urbanization, and the combination is a hall-mark of Modernity. It was a grim ideology: the esteem for Modern Man brought disdain for the common (wo)man (e.g. with the US-endorsed military regime in Brazil).

In the 1960s, experts and policy makers in the West were celebrating the impending victory of industrial fertilizer, with the UN and FAO participating in the celebrations, and the Third World eager to introduce this short-cut to food sufficiency. Yet it was a package deal, with loss of human and natural resources in its wake, and including the centralization of the seed and food production.

Breeding, as officially endorsed, had become a process that depended on the use of huge amounts of fertilizer, an obligatory and ample supply of water, and an intensive use of pesticides. The pesticides were obligatory, because breeding had focussed on increases in yield with the application of huge gifts of fertilizer, and these gifts greatly diminished resistance to pests and plagues. Moreover, the big seeds companies had acquired an extremely powerful position because they had become the owners of the new varieties. Industrial fertilizer was not the miraculous, but politically neutral, product that was suggested. This, in itself, is hardly surprising, as the origins of this industrial N-fertilizer lie in the large-scale production of explosives during the war.

In real-life soils, organic-N (both plant- and soil-derived), as well as plant-microbe associations, are important features, that determine their N-supply. That supply embraces not only a very broad range of N-compounds, but is a living process, with plants and micro-organisms participating actively, and the soil humics far from inert. Industrial fertilizer, on the other hand, was only tested on a "virtual" soil, which is inert matter drenched in a laboratory mineral nutrient solution. The real soil, its organic matter, its microbial life, and the building & maintenance of its hierarchy, was greatly neglected for decades. Soil deterioration became undeniable, erosion as well as loss of fertility being prime aspects. We rediscovered that agricultural soils are always in need of close care by people acquainted with the specific, local, soil. Evidently we are in need of New Peasantries.

<u>Thesis 4</u>

Even in the turbulent 1960s it was evident that
(a) revitalising traditional agricultures opened perspectives for food sufficiency, whereas
(b) choosing for industrial agriculture would lead to unsustainable urbanization and to dehumanizing slum-life of great crowds of people.
If the governments had chosen for a more social policy,
that would have benefited their common citizens,
they would have realized at a much earlier stage that the future lies in a revitalization of the local life of soils and ecologies. Unfortunately, they wasted all their energy on their attempts to centralize the food production and distribution.

11.6. Basic care - and the re-localization of expertise

Nothing illustrates better that plant life – and human life as well - always depends on gentle, micro-local labor than the innumerable flower visits of pollinators: '*Eighty percent of food plants worldwide … depend on pollination by animals, almost all of which are insects*' (Wilson 1996; see Free 1993 for an overview, Hears 1999 for a special group). With white clover as a mostly cross-pollinated crop plant Darwin already found that 20 flower heads covered to prevent insect visits produced only a single aborted seed, whereas 20 heads visited by bees produced 2290 seeds (for this example and the following refer to Free 1993 p.292 f.). In one ha of this crop there are about 430 million flowers; with each bee making 16 trips a day it needs more than 57 thousand bees to have all flowers pollinated in one day. And yet 'industrial' agriculture neglects those little laborers and risks their lives and abundance (Kremen et al. 2002).

Rape seed, the most important oilseed crop in many countries, offers a clear example (refer esp. to Morandin & Winston 2005). Without adequate cross-pollination it will not produce high yields (see also Abrol 2007). And yet herbicide use in 'industrial' agriculture, combined with several other of its practices, result in a precipitous reduction in weedy and non-weedy farmland plants for pollinators to thrive on (through the season). If anything, herbicide-resistant GM rape seed makes things worse still, and of course it requires a weird type of rationality to neglect the pollinators' free increase of yields and improvement of quality and to introduce a high-tech product that harms both plant and farmer. But note that already without GM we face an evident pollination crisis in 'industrial' agriculture, due to its own breeding- and fertilizer-techniques (Kearns et al. 1998).

This 'pollination crisis' indicates that breeding for 'industrial' agriculture lacks substantial rationality (is not 'sachlich'). As to breeding, the inherent conflict has been indicated time and again, e.g. (Singh & Sharma 2007):

> '*Large-scale cultivation of advanced varieties and hybrids is causing erosion of traditional land-races, weedy, and wild relatives of crop plants' .. 'Therefore, there is an urgent need for in situ and ex situ conservation of wild, weedy, and landraces of [crop] species*'.

Indeed, this 'industrial' breeding is progressively cutting its own roots. E.g. when Morandon & Winston have to indicate a way out, they have to admit '*In general, seeds of oil crops have short viability … Thus, maintenance of rapeseed-mustard requires frequent regeneration,* *which is very expensive and laborious …*'. And yet, they refrain from mentioning the one option that is the (substantial) rational one: making breeding farmer-centred again.

'Industrial' agriculture's main fault is an inherent denial of the central position of local knowledge and care in agriculture. Its high-flow energy use is incompatible with soil (micro) structure, with soil deterioration (e.g.erosion) as a result. Its centralized breeding makes it miss out on the local associations and symbioses with soil microbes, with e.g. debilitated nutrient acquisition as a result. And its high-fertilizer approach weakens the individual plant, with e.g. lodging and decreased resistance to drought and disease as a result.
Unable to deliver the (micro)local care that is needed for sustainable crop growth, 'industrial' agriculture lives from its denial.

There has always been something inherently tempting *for the farmer as well* in the prospect of large fields where machines do most of the work. The change-over from the use of some select machines to all-out mechanization, was definitely tempting to those farmers who could envision gaining more land that could easily be leveled. And it was especially an irresistible idea to the government-related agricultural expert who agreed with the government policy of 'rationalization' to increase 'labor productivity'. This, for example, was the case with Josef Müller (1950), quite a critical author, who was e.g. at pains to disclose the historical origins and magnification of the 'scissors', the ever-widening gap between the prices of industrial and agricultural products. Müller rightly points to the need to start from a just reward for (socially necessary) labor to deal with this huge problem (l.c. S.47 f.). And yet in his fascination with mechanization he misses out on any and all ecological and community resources!

A just reward for the farmer's labour is in fact an essential part of the 'just price' concept that is at the heart of serious efforts to undo the labor injustice brought about by the unbalanced industrialization starting from the late 18th century. Dippel (1952/53) emphasized its urgency because it concerns all labour that has care as its core. Just letting some party, be it government or business or even industrial labor, claim increase of machine productivity for itself, instead of giving credit to the machine as a social product, soon makes all care-centred labour 'too expensive'. In due time e.g. maintenance and recycling of machines and technical products will also become too expensive. There is bound to be something deeply irrational in the common concept of 'rationalization'.

This applies especially to agriculture because it implies the enlargement of crop fields and the leveling of land also where that is physically impossible. Note that in most countries, both within and outside of Europe, farming small fields proved perfectly possible for most regions, whereas farming extensive and level fields is possible only in selected regions. Everywhere the change-over to policies emphasizing the farming of large crop fields amounted to an immense, policy-generated concentration of agriculture in some select regions and the depopulation of most rural regions. The policy of 'rationalization' of agriculture once more proves quaintly irrational.
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In plain fact any agricultural policy benefiting the poor majority in this world must start from the rehabilitation of the large tracts of degraded soil (that more often than not are a result of short-term policies). And such rehabilitation always involves a careful, small-scale approach. Take as an example the improvement of traditional planting pits to recover barren and crusted soils in Burkina Faso, as related by Reij (1994 p.148/9):

> ‘*The improvement consisted of increasing the dimensions of the planting pits and by putting some organic matter in them. In this way water and nutrients are rationally combined. … Organic matter is applied to the pits well before the start of the rainy season in May or June. This attracts termites that dig holes up to 40 m deep. The termites not only increase the porosity and the water holding capacity of soils that used to have an almost 100 percent runoff, but they also transport nutrients from deeper layers to the top and the other way around. The termites have become major allies of the farmers in the Sahel in their struggle for the rehabilitation of degraded land*’. And he adds:
> ‘*Farmers have rehabilitated thousands of hectares of degraded lands and by doing so have invested in lands always considered to be of low potential. … Farmers do not invest in their best lands, which are cultivated permanently, but they invest in rock hard, barren land* [about which] *most experts believed that those lands could only be rehabilitated with heavy machinery (deep plowing) and the economic merits of such an operation were deemed doubtful*’.

As is the case with pollinators, main-line experts knew next to nothing about all of those little creatures that come the rescue of the farmer who is laboring with care instead of heavy machinery.

But then, a growing population also means there is greater opportunity for hands-on care, and indeed where that is practiced we see ‘*More people, less erosion*’ (Tippen et al. 1994). Quite to the contrary the use of heavy machinery (with the other measures that accompany it) causes the greatly enhanced erosion that characterizes ‘industrial’ agriculture. Borke c.s. point out that the redistribution of land (scale enlargement) and the changing of the rotations led to disastrous effects in Germany during the 1950s and 1960s:

> ‘*Die Ackergeräte wurden schwerer – sie verdichteten die Böden oftmals in bedeutender Maße. … Diese Veränderungen der Landschaftsstruktur und der Landnutzung wirken ausnahmslos erosionsauslösend und erosionsvervielfachend*’. And from these and their other (wide ranging) examples they conclude: ‘*Die vorgestellten Fallbeispiele belegen den gravierenden schleichenden, d.h. oft von den Nutzern und der Öffentlichkeit kaum bemerkten Verlust ausgedehnter fruchtbarer Äcker und Weiden – der Ernährungsgundlage*’.

There have been warnings for a long time now, Jacks & Whyte’s becoming widely known in 1939. The problem is immense in Europe, where ‘industrial’ agriculture caused accelerating compaction and erosion everywhere. It is disquieting that clear proof of the phenomenon (e.g. Roose & Masson 1985 for France) did not lead at all to adequate agricultural policies (cp. Larue 2001). Indeed, current economic policies, as induced by an economics theory that has no concept of care-centred labour, are out of touch with biophysical reality. The power shift to agro-concerns aggravated the situation. No limits to machine harvesting in forestry and agriculture are acknowledged even where this is evidently destructive (Febo & Pessina 2002, Vossbrink et al. 2002, Weisskopf & Gysi 2002):

> ‘*Heavy agricultural mechanization degrades soil physically and reduces soil productivity … Mechanization often leads to more frequent use of heavier equipment to achieve a deeper mellowing of soil the structure of which is degrading. However, the resulting macro-porosity does not last long* [especially not] *in unstable environments, such as loess…*’.

Indeed, Heinonen et al. (2002) find, for a clay soil in south-western Finland, that grain yield and, more still, nitrogen yield are superior where a light, unmanned tractor is used (ploughing is superior to stubble cultivation). '*A light, unmanned tractor appears to be an interesting option for avoiding soil compaction due to tractor traffic*'.

With a 'gentle & slow' agriculture evidently superior over our post-war 'power-agriculture', there is indeed much to re-evaluate. At present (2009) the financial/economic policies are under attack because of their destructive impact. It is evidently prudent to take a closer look at the monetary disarray and to investigate the viability of our post-war policies. What if those policies lack life-support qualities? What, in fact, did we achieve with all of our impressive research and expertise, and why?

At the heart of the historically unique phenomenon of the post-war accelerated growth of centralized research and expertise, there is this quaint equation of 'rationality' with 'de-relationality'. It in effect exposes the impotence of the newly installed, centralized circuit to deliver the essential care that must always be in local hands. We still experience a huge fly-wheel effect of all of this de-relationalized expertise, in e.g. the 'rationalization' policies in health and education that stem from a blunt denial of the relational character of labour there, or in the promotion of GM-crops 'to feed the world' (as in the 2008 article of Oxford economist Collier). It is essential to recognize that it is the great lack of objectivity (*Sachlichkeit*) of this 'expert knowledge' that leads to its inability to deal with real-life issues. This lack of objectivity is not just typical of disciplines like main-line economics, but of large segments of our proud post-war 'knowledge economy' (e.g. labor analyses and management theories used to impose 'rationalization-by-de-relationalization' on health and education). It is that lack of objectivity that leads to designs that effectuate the debilitation of people and plants – after which the 'experts' once more have to come to the rescue.

Note that this 'expertise' depends on the reductionism that is at the core of its specific version of S & T. We saw that this reductionism is an error, except in some very specific cases (e.g. some solution chemistry systems). Quite generally it is of minor value in life-related systems. That was clear enough at the introduction of all of this 'expertise', but since then the closer study of hierarchy, chaos, etc, as part and parcel of complex systems, brought it home more clearly still (ecosystems are complex systems – Bradbury et al. 2000).

> People who have doubts about reductionist S&T and its schemes are often accused of having a hostile attitude towards science. Yet, this accusation stems from a lack of understanding of the character of objectivity ('Sachlichkeit') in science and more especially of contemporary foci like hierarchy and chaos. Yet, the government and big business, which as a rule mutually support each other, justify their interference with society on the basis of reductionist S & T and are powerful enough to publish their criticism of their opponents (as in Collier 2008).

Capitalism and communism were both committed to an industry-like approach promising them the great power they saw exemplified in the factory (high-energy, high-throughput approaches). For half a century capitalism and communism tried to surpass each other in spectacular projects based on reductionist S & T. In this turmoil it was not easy to see that these projects were doomed from the start.

But, the real life of people and plants takes place locally and on a small scale, in spite of all our efforts at self-suggestion. The businessman or politician flying from one continent to another will only meet people if he takes time for them, and will only understand them if he meets them in the circumstances of their daily lives. Hopping from one similar hotel to another is of no help at all. What really matters is the local care for local people and plants. If that is denied long enough, they will wither.

Yet, centralized expert institutes sticking to reductionist approaches experienced an accelerated growth in post-war decades. With their links to the local people and plants cut off, they were not able to deliver care. The imposition of some destructive regiment, to the contrary, was a constant temptation. Now such imposition is not just fictitious. In retrospect it was a century in which an ideology, which claimed self-professed greatness, gained the upper hand, but was in essence flawed to the core. More than in previous generations it is essential that this kind of ideology should be exposed for what it is and should be replaced by, as Simone Weil put it, a radically different one.

There is much at stake in the choice to re-focus on the real lives of people and plants and to emphasize re-localization and '*Sachlichkeit*'. It implies the avowal that the expert cannot do without relational care as the core of his concepts and practices - in agriculture as elsewhere. That is, we need the 'rebirth of the expert', from a distant know-it-all to one at the service of the local people involved. It goes without saying that also our agricultural education, policies and regulations have to change greatly. In both theory and practice they will have to start from the care-labor of the local farmer, with his local resources.

Agriculture's foundation is not in financial capital, but in care-dependent, local, non-monetary, natural and human forms of capital. An economy that gives priority to financial capital is a stranger to biophysical reality. More specifically, for agriculture it is a stumbling block, rather than an asset. Both the economy in general and the agricultural economy are in for a big overhaul. In the power-hungry 20th century we tried to stand the world on its head. Therefore, re-formulating our agricultural and food economics, in terms of care and of local non-monetary capital, is an urgent matter. **The true agrarian 'capitalist' is the local farmer who by his care-full labors opens up the local natural and human capital**.
We will consider one more example of his/her resources.

11.7. Natural resource plenty

Biological nitrogen fixation (BNF) is an illustration of the abundance of natural resources that is at the New Peasantries' disposal. We will first look at its importance for the cultivation of rice.

A primary reason for rice cultivation had always been that high yields could be harvested when water supply was favorable. Note that these yields were/are obtained with traditional, local varieties unrelated to the fertilizer responsive varieties of Green Revolution origin (Fujisaka 1990 and Meertens et al. 1999 provide some examples). Note also that many of those traditional, local or regional, rice-farming systems were sustainable without needing much of a fertilizer input. It had been known for a long time that cyanobacterial biofertilizers are a valuable source of N here. Then in post-war decades it became evident that BNF in the rice rhizosphere can also contribute substantially to rice growth. Recently, in wild rice species as well as in landraces, endophytic colonization by certain nitrogen-fixing bacteria was clearly established, as was the inferiority of modern varieties in this respect. Evidently the recent surge in this exiting type of research is still hampered by the use of varieties and approaches deriving from Green Revolution breeding (as in James et al. 2002).

BNF and rice: see Laporte & Pourriot 1967 for an early review of cyanobacterial BNF, Vaishampayan et al. 2001 for an extensive overview of its use in rice farming, and Meeks & Elhai 2002 for growth states etc. Recently certain *Nostoc* strains proved able to enter into BNF-associations with rice roots, Nilsson et al. 2002. Nitrate destroys BNF with Nostoc, short-term as well as genetically (cp. § 5.). Also e.g. pesticides do harm, Hammouda 1999. Rinaudo 1974, Balandreau et al. 1975, Diem et al. 1978, Döbereiner & Boddey 1981, Watanabe 1981, describe early discoveries of BNF in the rice rhizosphere. Chaintreuil et al. 2000, and Ebeltagy et al. 2001, studied BNF in wild rice varieties; Engelhard et al. 2000 compared wild varieties and landraces as to their BNF with modern varieties.
The general plant growth-promoting association of certain Rhizobiae with rice roots that is part of a century-old wheat-berseem rotation in Egypt (Yanni et al. 2001) became widely known as well. This was likely the same type of association as in e.g. traditional wheat-wild legume rotations in the Mediterranean. Here new discoveries about the Rhizobial life cycle are important, Müller et al. 2001.

The Green Revolution rice varieties need a huge input of industrial fertilizer, yet allow most of this to go to waste: fertilizer N-use efficiency with wetland rice HYVs as a rule is less than 30% (van Nieuwenhove at al. 2001). Furthermore, these HYVs failed to maintain their yield potential (a decrease of some 30-40%). In other words: as to rice farming and its N-provision a return to natural resources is indicated.

If we consider maize (see von Bülow & Döbereiner 1975 for an early publication), we find the same superiority of certain traditional varieties (as compared to HYVs) in terms of harboring endophytic nitrogen fixers (Palus et al. 1996, Estrada et al. 2002). Again efforts to achieve the same with HYVs are largely failing (e.g. Riggs et al. 2001), hardly a miracle if we think of the dominant role of industrial fertilizer in their breeding history. Investigation of BNF in wheat started quite early too, with many positive results through the years (Larson & Neal 1978, Elliott et al. 1979, Vandebroek et al. 1993, Iniguez et al. 2004).

Summarizing: had mainline post-war researchers looked for BNF and other such abundant natural resources, they would certainly have found them. But as it was, they paid no attention to the peasant/small farmer or his resources, and stated that industrial fertilizer was all we had. They suggested that the peasant of old had been very poor, that he lacked natural resources, and had no good methods of processing and distribution. On the basis of that multiple falsehood they started 'bringing fertility' from their center to soil & agriculture, and supplanted small-scale, local activities with distant, centralized ones.

These researchers did not take the peasant/small farmer's resource-based farming approach seriously. They did not study his many rotation- or agroforestry-based farming systems, or his small-scale approaches to processing and distribution. Instead they declared it all primitive and outdated in comparison to the industrial approach to agriculture. Yet, BNF is ubiquitous, even in alpine ecosystems (Jacot et al. 2000), and in processing and distribution of farm products the wife of the peasant/small farmer always dispayed great qualities.

Thesis 5

As mainline research & policy started due to a rupture with 'traditional agriculture', it
(a) could no longer build on the array of natural resources at the disposal of peasant and small farmer, or

(b) adapt to their local experience, networks and ingenuity in processing and distribution. Instead it induced, at great expense (public, energetic & ecological)
(c) an 'industrial' system in which perishables are transported over long distances and receive 'value-adding' treatments that are cosmetic at best.

As to BNF, we meet it not only with leguminous and actinorhizal trees and shrubs (e.g. Olesniewicz & Thomas 1999, Clapp et al. 2001, Gentili & Huss-Danell 2003; Guan et al. 1998), but also with e.g. pines and oaks (Rózicki et al. 1999). BNF-based N-provision to trees is the core of the many agroforestry systems that have been developed by peasants and small farmers (e.g. Kass et al. 1997, Cairns (ed) 2007). Here, as in natural ecosystems, mycorrhization and BNF preferably go together, as a rule reinforcing each other (Atkinson et al. 2003, Barea, Azcón & Azcón-Aguilar 2005). But as soon as high-external-input agriculture is introduced BNF is stopped and mycorrhizae are made dysfunctional (Jansa et al. 2006):

> '*The role of ... mycorrhizal fungi in plant production has been marginalized in high-input agriculture through the use of pesticides and fertilizers, creating scenarios where the symbiosis may even be causing growth depressions when the cost (carbon drain) of the symbiosis becomes higher than the benefit…*' (Larsen, Ravnskov & Sorensen 2007 p.123)

But then: who needs this 'modern agriculture' with such natural resource plenty?

For **maintenance & reactivation** farmers always used this same combined natural capital of mycorrhizae and N-fixers:
(1) Fallowing – thanks to e.g. ubiquity of wild legumes (Sanogho et al. 1978) and of their N-fixing symbionts (Mutch & Young 2004)
(2) Rotation with legumes – with far wider occurrence of symbionts than envisaged (Chen et al. 2003), high organic-N enrichment (Khan et al. 2002), and close adaption of rhizobial lifestyle (Müller et al. 2001)
(3) Grassland maintenance - dependent on legumes (Spehn et al. 2002) as well as on other N-fixing endophytes (Hurek et al. 2002, Miyamoto et al. 2004).
The natural symbiotic BNF capital is far greater than imagined: non-rhizobial symbionts showed up (Rivas et al. 2003) and rhizobial symbionts proved always more diverse and polyphyletic (Sylla et al. 2002, van Berkum & Eardly 2002).

For example, BNF and mycorrhization together enable (re)vegetation in harsh environments and poor soils (Valdenegro et al. 2001, Quatrini et al. 2002). They form the natural capital with which e.g. desertification can be halted and with which the peasant can re-establish agriculture on derelict soils. **Here an influx of New Peasants could achieve what is completely out of reach of 'modern agriculture'**.
But note that such an influx is also needed for the **common maintenance and reactivation of soils in agriculture**. Modern agriculture has not fulfilled its promises (those of the government expert), but induced widespread erosion, loss of soil structure, and loss of fertility. A renewed utilization of 'organic' approaches is called for.

Quite decisive is here that the potential capability for nitrogen fixation now is deemed to be present in most soil bacteria (Rózycki et al. 1999 p.248). But there is a caveat: '*the expression of nitrogen fixation genes may be hindered by environmental conditions*' (Rózycki et al. l.c.) – with the conditions imposed by 'modern agriculture' looming large. Note it was already Winogradsky who in 1927 clearly sounded a warning here, with a.o. Commoner repeating it in 1971 (cp. Commoner 1972). Yet, post-war decades were fascinated with the presumed constructability of nature and society, and so were not only sure that industrial fertilizer use was the way forward, but also neglected BNF in their models: '*It is only relatively recently that N_2 fixation has been explicitly represented in ecosystem and biogeochemical models*' (Capone et al. 2005 p.13). As a result, the **first-ever** truly specified, integrated estimate of BNF in (terrestrial) natural ecosystems is that of Cleveland et al. (1999) – who arrived at a "best estimate" of 195 Tg N yr^{-1}.

The decades of neglect of BNF potential, and its obstruction by fertilizer (and breeding) practices, consistently led to low estimates of bio-fixation in crop fields, with even authors like Smil (1999) arriving at estimates that are low compared with industrial fertilizer gifts. Then, failing to consider our own obstruction of BNF potential, and only taking note of the steep post-war rise in fertilizer application, it was considered as proved that fertilizer was needed 'to feed the world'. But it was a deplorable kind of logic, as if someone notices a continuous increase in the average speed of cars on the German Autobahn and presents that as proof that this phenomenon is 'strictly needed'.

The steep rise in fertilizer application in 'modern agriculture' is part and parcel of its conceptual reduction of the soil to an inert substrate plus mineral nutrient solution. That is a reduction that made it careless as to e.g. soil erosion and quite generally made it negligent of most rhizosphere processes. Renewed attention to those processes is of a recent date only. Here the Australian researchers who ever kept research in these processes going, had a leading role (Bowen & Rovira 1999; Pinton et al. (eds) 2001 give a wealth of material). The importance of the subject is easily gathered from titles as:
'*Soil organic matter mobilization by root exudates*' (Nardi et al. 2000),
'*Can plants stimulate soil microbes and their own nutrient supply*?' (Hamilton III & Frank 2001), and
'*Importance of rhizodeposition in the coupling of plant and microbial productivity*' (Paterson 2003).

But of course, when plant nutrition is narrowed down to mineral nutrient provision as determined by the distant expert, all of this delicate interplay between plant, soil and rhizosphere organisms is lost sight of. That is, **'modern agriculture' missed out on those interactions that are of decisive importance for plant health and nutrition**.
Where it paid any attention to microbial life in soil, it still did not follow e.g. Winogradsky's injunctions (and protocols) to focus at low-nutrient, soil-like conditions. So it is only (very) recently that the re-introduction of nutrient-poor soil-extract media assisted in opening up the vistas of the rich diversity of plant health & growth assisting micro-organisms (e.g. Aagot et al. 2001, Janssen et al. 2002, Kaeberlein et al. 2002).
Evidently we were not helpless before the introduction of 'molecular environmental studies' (Ward et al. 1995 is an early review), but mainline agricultural research did not follow the low-nutrient soil microbiological researches of Winogradsky a.o. for half a century. That was a chief reason that it missed out on soil microbiological associations and processes of primary importance to (crop) plants.
But of course, those associations and processes are always (micro)local in character. It is, in other words, a rich fund, but it is available only to those who act knowledgeably and care-fully at the (micro)local scale. Only when the centre of expertise and decision-making is shifted to the local farmer who is conversant with the local plant and soil, that careful agricultural use of those processes becomes a real possibility. **Here as elsewhere only an influx of New Peasants can do what is out of reach of 'modern agriculture'**.

Thesis 6.

We need to re-engage New Peasants to:
(a) maintain and reactivate our agricultural soils
(b) develop the agricultural use of the wealth of rhizosphere processes
(c) re-establish vegetation in desertified areas and re-establish agriculture on derelict soils.

Now note that this whole system of 'modern agriculture' is built from cheap oil, as is the cheap transport on which its production, processing and distribution depend. With the end of (cheap) oil quite near, this inter-relationship forces us to reconsider transport and industrial agriculture together.

11.8. Energy and transport

As oil is getting scarcer, we first of all realize that we will need bio-energy, and a bio-compatible life-style as well. Yet in the present our 'industrial agriculture' is 'guzzling oil', with an energy balance for e.g. bio-alcohol in the US of almost 1 (as much fossil fuel goes in as biofuel gets out, Baldani et al. 2002). Indeed quite generally 'industrial agriculture' is incompatible with the coming age of bio-materials and bio-energy (Pimentel (ed) 2008). Most large-scale distribution and processing of foods & feeds likewise is very energy-intensive.

With oil running out there are pressures to e.g. revive efforts at extraction & processing of tar sands. But surely this has been tried before, with the Canadian and US projects of the 70s (in the wake of the 1973 oil crisis) as the most notable examples. These projects could command both high technology and near-limitless finances. '*Both programmes have dissolved in the heat of excessive costs and environmental burdens*'. Besides accelerating costs '*Huge water requirements, land destruction, and release of hazardous air pollutants made the projects even more controversial and there should be little regret about their abandonment*' (Smil 1987 p.75; cp. also Saether et al. 2004).

Note it was all due to 'oil addiction' and not to any real need. Our cheap-oil economy is barely half a century old and took the place of what was primarily a natural resource-based economy. That natural resource-based economy also furnished a wide array of materials that could feed a bulk & fine chemical industry, and in our time 'green chemistry' reverts to the use of such renewable bio-resources (e.g. Tundo & Anstas (eds) 2000; Hardy 2004). From a chemical point of view there was no need for the massive post-war change-over to an oil-based chemical industry. Quite to the contrary, it led to power concentrations which arguably caused a lapse in chemical research and development. So a transition to a natural resource-based energy- and fuel-economy would not only be possible, but could be wholesome as well.

As to our oil addiction, besides e.g. high-rise buildings with their immense energy intensity it is especially truck transport and commuter traffic that devour energy. This evidence of unsustainability is part and parcel of our ongoing urbanization. Re-ruralisation in combination with re-localisation of the economy can avert this dangerous trend. Indeed, any deeper analysis of the future economy points to the importance of re-localization (Daly & Cobb 1993, Pretty 1998).

(Bio-)energy: within the factory engineers try to minimize the need for transport wherever possible, but the modern agricultural and food-economy forgot all about it, and made choices that led to the opposite of a truly technical development. At present the impending end of the cheap-oil economy forces us to return to true technical standards – and that implies the end of our agricultural and food-system that is 'guzzling oil'. Cp. the '*Energy use estimates for major components of the US food and fiber systems*' for 1991 (Cramer et al. 1997 p.369 f.): farm production uses only 15% of the total, and even this 15% is oil-based. Decentralization to the local level will open up the human and ecological perspectives that we need, because it will do away with most of transport & distribution and industrial food processing.
Within the framework of a decentralized economy, bio-energy (Arshadi et al. 2004) and bio-fuel will be reintroduced. But note that Brazilian bio-alcohol offers the only successful example to date (Pimentel & Patzek 2008, Koplow & Steenblik 2008). It depends on the BNF in Brazilian sugarcane varieties for attaining a useful energy balance (Boddey 1995, Reis et al. 2000 esp. Pt.III). See Ruschel et al. 1978 for an earlier, James & Olivares 1997 and Baldani et al. 2002 for some recent overviews, and dos Reis et al. 2000 for the crippling influence of fertilizer on both cropping and processing of sugarcane. BNF and rotation with legumes (e.g. Chen & Lee 2001) have always been the methods of choice for the production of sugarcane and bio-alcohol. The fertilizer-intensity of present plantation growing of e.g. bananas and pineapples shows that the same choice is urgent for many other crops too (cp. Weber et al. 1999).

The link between 'industrial' agriculture and cheap-oil transport is crucial. Both are central post-war government projects, shaped by functional rationality instead of substantive rationality. Being core elements of the post-war ideology, they were crucial in the rise of the post-war technocratic system with its excessive imbalances. These imbalances originated in the faith in the constructability of nature and society, a faith that was incompatible to the human and the ecological dimensions (cp. the expectations of helicopter transport in the 1950s - Dienel 1997 - and those of weather modification in the 1960s).

In regard to road infrastructure and truck transport I will just mention two **technical** aspects, to highlight the fact that also here functional rationality led us into a dead end. Note that the negative **social** effects of automobility have been well researched - and have proved to be enormous (Delucchi 1997).

1. The *energy efficiency* of the car is not even 2%.
The 'need' to move say 1000 kg dead weight for the transport of one human being is the prime reason of this blatant inefficiency. Oddly enough the **technical** need for completely different designs had been clear for decades, and yet it was resisted time and again, both by the industry and by the public at large (see Tempelman 1999 for alternatives as to materials and construction). Of course, this 98% waste applied to an ever-larger car fleet and caused a soaring carbon dioxide production. In other words: it is a chief cause of our global warming problem.
But note that at present the problems are already immense. Two Irak wars made us conscious of the immense geopolitical costs of oil. At home, it was foremost the steep rise in diesel powered cars that in recent decades caused the great increase in concentration of minus 10 micron particles in the air. A rise that had devastating consequences for the lungs of the populace at large, and especially for asthmatics.

The **externalized costs** are huge.
Taking a very close look at the year 1990-1991 for the US, Delucchi and co-workers arrived at an estimate of one-year social costs of 1.67 to 3.31 **trillion** dollars (Delucchi 1997). And it is quite sure they did not include all the costs, e.g. most of the ecological costs.

Focusing on the social costs in the UK, but more limited in scope than the US study, is Maddison et al. 1996. It e.g. leaves out the immense geopolitical costs of oil – for which see contributions to the April 2003 issue of The Ecologist, and Bromley 2005. Note that by the 1990s even the OECD had admitted that the transport sector had externalized its costs (OECD 1992).
As for myself, this sobering side of post-war trans-port and transport policies came to my attention before the start of the present researches and so was a help to consider if maybe not only our transport policies, but also our agricultural policies were not 'true to life'.

2. If the technical standards had been applied and maintained from the start, the road construction would have taken another course as well.
As it was, with the boom in car numbers in Europe starting in the 1950s, the road taxes were channeled to the construction of always bigger roads, fit for trucks and not just for cars (or just for light weight vehicles). The *demolishing effect* of cars and trucks on our roads is roughly proportional to the fourth power of their ax load, an empirical fact known from at least the 60s (Maddison et al. 1996 Ch.6, a.o.). That is, the effect of an average car with ½ a metric ton ax load leads to a proportionality factor 1/16, but a truck with 3-ton ax load to a proportionality factor 81.
And of course, while cars stand idle most of the time, trucks are 'earning their money'. In other words, construction and maintenance of our road system ought to have been financed largely by the trucks from the very start.
Instead of that, it was public finances and car owners that bore the costs – while the immense social and health costs of truck transport were externalized. Evidently traffic and transport expertise after the war neglected *substantial rationality* and developed along lines of *functional rationality*. Jane Jacobs indicted it for that, for half a century.

As indicated, the health cost are mind-boggling, with the fine particular matter (esp. the minus 2.5 micron fraction) the chief villain. The findings have been checked thoroughly – Chow & Watson 2002 - and as a result the relation with fossil fuel utilization in automotive traffic now is quite clear. McCubbin & Delucchi (1999) in their thorough study arrived already at impressive health costs, but note that relevant medical research probed ever deeper from the late 1990s on. Some studies from the beginning of the present decade are: Hiura et al. 2000, Iwai et al. 2000, Le Prieur et al. 2000, Hauser et al. 2001, Juvin et al. 2002, Roux et al. 2002, Takano et al. 2002, Rengasami et al. 2003.
Though the WHO has stressed the problem for years now, governments are slow to act, due to the fact that transport experts still can't really imagine that people could do without cars (e.g. Handy 2006). Yet, for years we have had now the clear example of quite some cities that shows that that the reduction of automobile dependence is perfectly possible (e.g. Newman 1996).

Of course, the moment we start charging the costs to the people who are actually responsible, the trucks-owners and –users first of all, road transport will become extremely expensive, and this will be one more important reason to re-localize production and distribution (cp. Bürgenmeier 1996 p.162).
Note that both those post-war projects of High Modernity, industrial agriculture and road transport, were not organized on the basis of truly technical standards, but on the basis of bureaucratic goals implemented according to 'functional rationality'. Bureaucrats and their experts shared a common ideology and were working hard to extend their powers. As to the 'liberty' provided by the car, Rajan intimates (2007 p.88):

> '*While the state's patronage of auto-mobility might be seen as facilitating the universal exercise of individual liberty, the massive public expenditures and*

> *extraordinary support for auto and oil corporations could also be regarded as a way of extending state power and monopoly capital*'.

In regard to transport and agriculture we were not building 'the best of all possible worlds', but a world that was not sustainable at all. We evidently better apply *substantial rationality*, with true '*Sachlichkeit*'.

Our cheap-oil based standards are not for real:
'*energy input for fabrication of goods from wood requires only 3-14 GJ/ton, whereas plastics or metal manufacturing requires considerably more energy, e.g. plastics 60-80, steel 20-25, aluminium 190 GJ/ton'* (Haberl & Erb 2006 p.164).
And the economy that we constructed is not for peace: '*The Netherlands, for example, requires almost six times its domestic bio-capacity to sustain prevailing levels of net consumption. … The enormous purchasing power of the world's richest nations enables them to finance their ecological deficits by extending their ecological foot-prints deeply into exporting nations and throughout the open ecosphere. Wealthy and powerful nations can now achieve through global commerce what used to require territorial occupation*' (Rees 2006 p.149, 150).
And: '*looking at climate-damage alone, rich countries might already have imposed costs on poor countries greater than the poor countries' existing foreign debt. The people bearing these costs include the one billion or so who already lack daily access to safe drinking water, electricity, secure food supplies, and basic education*' (Turner & Fisher 2008).

'Modern Man' in many ways was an emperor without clothes (the ones our bureaucrats and experts helped weaving). Cheap oil and twisted road transport ultimately brought us 'in the clouds', in our 'dream land', and it is urgent to come **down to earth** again. We can - with the small farmer.

<u>Thesis 7</u>

Foods & feeds, renewable bio-resources, and bio-energy, based on the use of local, renewable resources, will soon provide the material foundations of our socio-economies.
Their local/regional processing and distribution will be the norm.
The centre of this sustainable socio-economy, also in terms of innovation and development, will be established around New Peasantries.

But note that main-line research and government are completely out of touch with this small farmer. There even is hardly any written history of the small farmer in the Netherlands, or in post-war Europe at large. The 'success' of scale-enlargement and mechanization figures on most of the accounts of post-war agriculture, but if the small farmer is mentioned at all, it is as a 'problem'. In the Netherlands people like Maris and Herweijer (van den Brink 1991) do not tire from stressing that 'there are still too many of them'. They are proud 'not to look backward', but to 'look forward' instead, and to anticipate a future where agriculture will have acquired all the characteristics of an industrial process. There is not a trace of interest with them for 'traditional' agriculture and its expertise. To the contrary, they use all the means of technocracy to reach their futurist 'industrial' agriculture. With their laws and ordinances they make sure that the small farmer is excluded from that future.

11.9. Agrarianism reconsidered

> The '*new generations of farmers and scientists are unaware of the depth of experience to be found in many traditional cultures. Agriculture is an ancient activity in the tropics. Farmers in these areas have selected species, methods, and techniques that have proved vital for their survival. … Some traditional systems of agriculture have a history dating back thousands of years to the many ancient civilizations that developed in the tropics. In some areas population densities were much higher than today and provided food for their people without tractors, chemical pesticides, or fertilizers*' (Gómez-Pompa & Jiménez-Osornio 1989 p.233f.)

Their statement about traditional agricultures was hardly new – think of King's '*Farmers of fourty centuries*' (1911) about Asian agricultural systems that was widely known already before the war – and yet the authors were completely right: post-war agricultural research, policy, and education embody complete negligence of traditional knowledge systems. That negligence is 'scientific' only in a Cartesian universe, in which research has to start from scratch, but not in the real world of people and plants, where we are dependent on the transfer of 'traditional' behavior and experience (from our very first days on), exactly when we want to make progress.

As indicated before, there was a sizeable fund of knowledge about traditional systems of agricultural after the war, both with several of the older agricultural consultants in Europe and its colonies, and with the small band of researchers in e.g. anthropology and geography. Publication of research was mostly in broad-ranging periodicals, e.g. Condominas & Haudricourt's 1952 account of Indochinese ethnobotanics in the *Revue Internationale de Botanique Appliquée et d'Agriculture Tropicale*, while most, by far, of the new-style agricultural research was published in its own brand of specialist periodicals. The 'new researchers' (and their bosses) paid little attention to the 'old' circuit, that anyway soon got dwindled by the burgeoning new research. But note that the 'old type' of research did not just disappear. To the contrary, at least part of it developed in its own way, with often remarkable results (cp. Burnham 1980 and Condominas 1980 on savanna agricultural systems). For sure, some broad ranging consultants still were at pains to locate such publications, yet, specialist agricultural research was completely disconnected from them.

To understand this strange phenomenon, we have to allow that this mainline research made conscientious choices in its own way. Everywhere the possibilities that were open to US research were greatly envied, after the war. In countries like the Netherlands official policy soon took the US for the shining example, especially in connection with agriculture and agricultural research. Some leading researchers, at least, took good notice of US authors who, apparently from their own research, seemed able to evaluate 'traditional agriculture'. So when they read in e.g. Nash' '*Primitive and peasant economic systems*' (1966 p.22):

> '*The productive output of a society is first a function of its technology and the width of the division of labor, and hence the simpler and more rudimentary these are, the lower the gross product of the society is. Other factors, of course, influence the actual level of output of goods and services of any society, but no society can transcend the limit imposed by its technological and organizational format*'

they were maybe a bit sad, but nevertheless became convinced that henceforth history was hardly relevant for their own research. The more so because leading US researchers like

Schultz voiced opinions that were very similar to that of Nash: traditional agriculture hit a 'ceiling' that only our modern S & T was able to break. Of course, to be able to evaluate those pronouncements of Schultz and Nash, researchers needed a point of view from where they could oversee the different 'schools', each in its specific historical context. But High Modernity's equation of 'true expertise' with de-temporized and de-localized knowledge blocked the entry to such a historical point of view. A system could grow that, because of its Cartesian (a-historical) approach, lacked a foundation from the very start, and that was also inherently unable to evaluate its own course and accomplishments.

Every era has its own 'dream of the age', and all eras are inclined to take their own dream for the real thing, yet, our age of High Modernity made itself impervious to criticism and evaluation by declaring history irrelevant for any of its projects. Only now we started to realize that much, if not most, of the 'growth' that up till recently was our pride, could very well be not a sign of 'success', but of our own inability to take distance from our endeavours (to evaluate and maybe stop them). The fact that we did not link up with solid research about traditional agricultures – including those of European peasantries – was due to the wrong premises.

In plain fact our agricultural research and extension has an arrears of at least half a century, and we have to consider the possibility that its institutions, that we formerly thought impressive indeed, in fact are suspended in the air. We urgently need to study the small farmer/peasant of old and his practices if we want to get down to earth again (instead of experiencing a fatal fall). As indicated, also in post-war decades there was some solid research that at least touched the existence of this peasant and his practices. There were, for example, some close studies of the problems faced by agriculture in comparison with industry, but most were in German, and not part of the American translation program (that encompassed chiefly subjects of military interest). So neither American agricultural economists were conversant with them, nor the many specialists elsewhere who took these Americans for their guide (e.g. most FAO experts). When Mansholt c.s. then argued that 'rationalization' (e.g. scale enlargement) is essential for European agricultural policy, they started from assumptions that by then had been disproved in the German-language literature (e.g. Kahler 1958).

Relevant was also part of research by institutional economists, especially those that started from Karl Polanyi's achievements (cp. e.g. Dalton & Köcke). Yet, for decades mainline agricultural economics proved unable to link up. At present attitudes vary from sofisticated denial of ecological problems of industrial agiculture (Lichtenberg 2002), via sincere efforts to treat the acknowledged problems with mainline's faulty methods (Heal & Small 2002), to conceptual and methodical renewal (Ostrom 2002 – note she is a political scientist).

An example from the 1980s:
Bennett & Kanel (1983) payed lip service to Polanyi's historical approach (l.c. p.205), next to revert to sketching developments in the US as a kind of Iron Law that other countries would have to emulate (p.210, Fourth point), apparently not even aware that Polanyi rejected the existence of such Iron Laws (Dalton & Köcke 1983 p.38). They then stated (p.219):
'The point is that from the perspective of microeconomic analysis, the firm and its entrepreneur do not own anything; the firm organizes production with resources it has to obtain at market prices' (emphasized, in the original).
Here the authors proved not only unable to link up with Polanyi (A), but also with Chayanov's small-farm economics (B), with important contributions from feministic economics (C), and with a range of studies of household economies (D)

> (They use the term 'Household firm' in a way discordant with the use that Chayanov and his followers made of it – cp. Maclachlan (ed) 1987). It is especially baffling to note that they proved completely oblivious to the peasant's natural, communal, and human resources, for which even their concept of 'market prices' is ridiculous.

As indicated time and again, mainline agricultural economics co-opted mainline economics' denial of history, as well as its refusal to discuss its fundamental assumptions. For example, in 1948 de Vries' historical-economic analysis would have been very usefull for policy development, yet, within a few years nothing of the kind was left in agricultural economics. As a result de Vries' emphasis that '*the farm is, from an [market] economic point of view, the most vulnerable enterprise of all*', was soon forgotten, and we had to wait for the rise of feministic economics to be brought back to basics again.

Feministic economists have stressed that mainline economics proves unable to 'see' the household with its fundamental labors-of-care. Likewise, mainline agricultural economics is unaware of agriculture's main resources and care-practices. In short: it is simply not about the (physical and human) world that we inhabit (Norgaard 1989).

> It is well to remind that mainline economics gives also no clue to the complete distortions of the economy that originate in the government's extreme favors to big enterprise and to industrial agriculture – for which see McCarthy & Rhodes 1992 and Scott 1997. Mainline (agricultural) economics does not inform us about the real-life economic world!

The small farm, that exemplified those care-practices and the use of local resources, largely disappeared from the scene in countries like the US and the Netherlands. We saw that it did not just disappear: it was forcefully removed from the scene, one of the sad accomplishments of our age of total war. There were no 'evolutionary forces' at work, but blunt government forces, and what they achieved was ultimately to the detriment of society at large, because the system (and its expert) was completely out of balance. Governments and their experts were sure that by deploying the industrial power that had played such a prominent role in the war, they would be able to make great strides towards reconstruction, food sufficiency, and a higher living standard. Their enthusiasm was unrestricted.

But that choice for centrally designed 'power solutions' implied a great distance from people and plants. A great distance, that is, from a nature and a society where problems as well as solutions are largely location- and time-specific. As a rule, the careful exploration and use of ecological, socio-cultural, and human resources is profitable 'on the spot' only, and truly adapted practices can emerge only from a local, timebound context. The concept of a reality that would allow definitive research and design in far-away centres was stillborn from the start. We ended up with forcefully applying protocols that did not fit the reality of people and plants.

Industrialization/urbanization was conceived as an all-inclusive protocol, and for decades presented as a certainty. Yet, it did not take long to prove a costly mistake, that brought an always bigger part of mankind and of nature in deep misery. The 'industrialization' of agriculture was likewise conceived as an inclusive protocol. Because it approached soils, plants, farms, and rural communities with the 'logistics of warfare', it hardly could fail to distort landscapes, to impoverish ecologies, to ruin soils, and to liquidate rural communities. Even its concept of industrial production was so wide off the mark, that it hardly could fail to induce wrong choices.

With about everybody convinced that this was the road to the Kingdom of Man, for more than half a century there was a historically unique singularity in our policies and approaches. Thanks to this singularity, we were able to fill our horizons with our ideology's institutions, and yet, the doubts about our 'Kingdom' multiplied, not the least because nature proved not to conform to our projections. Gradually it dawned that we had been warring against diversity in nature and society, instead of exploring its riches.

For some time now, and especially in connection with agriculture, much of this has been common knowledge (except for those experts and officials who never even tried scrutinizing High Modernity and its projects). Witness the top-level call (FAO/UN) of the 'International Assessment of Agricultural Knowledge, Science and Technology for Development' IAASTD in 2008 (cp. www.agassessment.org):

> *'The IAASTD therefore calls on the international community and national governments to systematically redirect agricultural knowledge, science and technology towards sustainable biodiversity based agriculture and agroecological sciences, while addressing the needs of small farmers'.*

Likewise the UN High Level Task Force on the Food Crisis points to western-style industrial agriculture, agribusiness, and food speculation as causes, and hardly solutions, to the present crisis. A similar message, '*Global food responsibility*', was recently (May 2009) sounded by the IATP (Inst. for Agric. and Trade) and the CIDSE (Coop.Inst. for Development and Solidarity).

The rebirth of (attention for) agrarianism that we witness was the reason for Allan Carlson to give his article in the Spring 2008 'Intercollegiate Review' the title: '*Agrarianism reborn: on the curious return of the small family farm*'. And indeed, the brighter side of the present situation is that we realize that, in our age of High Modernity, we hardly ever used the local resources and possibilities at our disposal. They are still there, open to the small farmer.

Our efforts to rule agriculture and food distribution from our centres of power (with the help of reductionist science and technology) prevented this use. An extreme impoverishment – in which social impoverishment followed that of plant, soil and ecology – was connected with the ascent to power of our seed- and food-giants (note that China's party top in fact is one of them). Their hybrids, in themselves a physiological and genetic disaster, are used to block the farmer's ascent to his resources everywhere. Yet, once we realize that we are confonted with extreme impoverishment (in disguise), we are free to turn around and take a closer look at the rich local resources that are still waiting to be used. Sustainablility of agriculture and food production is not a chimera - if only we allow the small farmer the access to the resources that our effort to wield absolute power from our centres of 'expertise' and government denied to him.

After half a century in which 'agarianism' was considered a concept from some distant past, we start realizing that we simply never tried to explore anything like it. Our singular choice for industrialization/urbanization precluded the exploration of alternatives. Quite likely, the active exploration of 'agrarianisms' can open up a multitude of perspectives. We are free then also to explore the manifold possibilities of craft and manufacture that our age of industrial power concentration left unused. Such exploration is urgent, if only to offer the growing generation the perspective of worthwhile labor that our present system of far-away production of gadgets is not able to offer.

Once we understand that our post-war, all-out industrialization efforts stemmed from a greatly distorted view of man and nature, and of science and technology, we are free to take a fresh look at nature not only, but at society also. It could very well be that, ultimately, nature and society are not in perpetual enmity towards each other.

11.10. Outlasting our age

For a close I want to stress that I have no intention to ascribe the role of a 'saviour' to the New Peasantries, any more than to the Proletariat of communism of old. Communism refused to accept and work with the down-to-earth person, whose life is defined by a specific time and place, and made the common (wo)man to live in a gray 'non-time' instead (N.Mandelstam). But note that our own post-war High Modernity likewise pushed a 'Modern Man' without time or place. What Carl Sauer, the famous geographer, said in 1951 about post-war American social science was pertinent to all of our post-war High Modernity projects (cp. Sauer 1951/63 p.386):

> '*Of late years social science has been occupied with removing time from the pattern of our thought, and similarly also, place. … This turning away from the meaning of time and place is a rather peculiarly American folkway of the day. I should derive it from our unique national history and geography, the late and rapid exploitation of the most richly and variously endowed land mass in the world*'.

When he added, '*The older Western world has not similarly simplified the study of society to universalize its present self*', he thought of a Weber, a Scheler, or a Mannheim. Yet by then governments all over Europe were increasingly dedicating themselves to the pursuit of High Modernity. Social science, as well as science at large, was increasingly switched on into its projects.

In the same lecture Sauer described the development of tunnel vision that accompanied the accelerated growth of this government-slanted science (l.c. p.385):

> '*With the growth of central advisory, planning, and granting agencies …. it has come about that a reduced number of directions are selected for approval and support. This has introduced a grave and growing disorder into the body of our scholarship. When preferments and rewards are being posted for doing certain things and not doing others, the pliable and imitative offer themselves most freely, and the stubborn ones hold out. Local authority is impressed by the objectives expressed by the distant patron. He who is not deflected from his chosen direction to take part in the recommended enterprise is the unhappy guest who sits out the party. Thus conformity to a behavior pattern comes to prevail*'.

Under Thatcherism and Reaganomics this tendency to insist on a narrow research focus was shifted from government to TNC. It saddled us with the current 'disappearing science' of the present: science, in the TNC's leading-strings, that is removed from sight (and scrutiny). Its products are often sanctioned by the government and empowered by its regulations, yet, they are based on functional rationality and we have no reason to think them 'objectively valid' ('*sachlich*' - Hengstenberg).

But then, Sauer had emphasized before the war already that '*The whole occidental commercial system looks like a house of cards*' (Sauer 1938/63 p.149). Sauer thought first of all of the commercialization of agriculture, with its extinction of species and varieties, its soil destruction, etc. (Sauer 1938/63 esp. p.149f.). More generally, he emphasized at the end of his 1938 lecture that '*We have not yet learned the difference between yield and loot*'.

It is worth quoting him in full (l.c. p.154):

> '*The doctrine of a passing frontier of nature replaced by a permanently and sufficiently expanding frontier of technology is a contemporary and characteristic expression of occidental culture, itself a historical-geographic product. This "frontier" attitude has the recklessness of an optimism that has become habitual, but which is residual from the brave days when north-European freebooters overran the world and put it under tribute. We have not yet learned the difference between yield and loot. We do not like to be economic realists*'.

Unlike 'modernizers' like Parsons, Smelser, etc., Sauer was able to see the limited and historical character of his own culture. That made him willing also to meet people in their own time and place, instead of putting them on the imaginary axis traditional-modern. He made "*homefolks*" from '*the people among whom he first came as a stranger … by sympathetic involvement in their everyday-activities, however humble*' (Leighly 1963 p.3).
In that way he got involved with e.g. Mexican peasants (some major publications in 1934 and 1935). As he told later (1956/63 p.392):

> '*In my days of field work in back areas of Mexico I learned to accept confidently the geographic and natural history competence of the native guides. They knew how to interpret the lay of the land, to keep a mental map, to note almost any change in the scene. They were usually able to identify the plants and were right as to systematic grouping and ecological association*'.

Based on close acquaintance with Mexican peasants and their agriculture, Sauer stressed when addressing Rockefeller Foundation officers, that those peasants were not in need of the Foundation's breeding or development programs (that he was an experts in the field we see also in Humlum 1942, Vorwort). Yet, these programs would soon be recommended everywhere as the solution to the world's agricultural and food problems. To the people who pulled the strings, the idea that they could control it all from their centers of power was irresistable. To the common people, the promises of plenty proved alluring as well. Yet our post-war half-century of High Modernity lacked substance. In Sauer's words (1951/63 p.387):

> '*Do we think that we dominate time, as an upward spiral that we have under control, our increasing knowledge confidently shaping its development? Or is this faith that we are shaping progress by material skills and building an ever expanding system in truth the great "phantasm" of our day, the "brave new world"? Have we set up an economy of waste, which we call the miracle of American production? Can we disregard our deficit spending of natural resources because we shall continue the triumph of mind over matter? Are other times and other places of importance only in so far as they can be related to our egocentric and ephemeral position? Are we the cleverest people of all time or the blindest because we think neither whence we came nor whither we are bound?*'

In a curious way, Modern Man is a vulnerable and transient phenomenon, due to his reluctance to tie himself down to a specific time and place. His projects deny, more often than not, the difference between 'yield' and 'loot'. His concept of 'greatness' is warped and transitory.
Yet, 'little people', who are ready to invest in their neighbors and in their environment, discover the local resources that are available to them. Though the current generation has saddled them with a 'house of cards', they still can re-build 'from the ground up', on a small scale, and always on a local level.

Like true New Peasants.

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12. Looking back and ahead. A summary

> '*Do you know, Fontanes, what astonishes me most in this world? The inability of force to create anything. In the long run the sword is always beaten by the spirit*'
> (Napoleon to Fontanes, his Minister of Education)

The ruthless 20th century wars created a greater abyss than any of the previous wars in history. Yet, mainline agricultural research in the Western world forced its "progress" upon the world with the industrial means and managerial methods that had their roots in these heinous wars. Post-war idealism failed to overcome this puzzling lack of logic. It is true, the biblical text "*They shall beat their swords into ploughshares*" was engraved on the statue in front of the UN headquarters, but the conception of industry-based agriculture stemmed from warlike attitudes, the forcefulness of which was reminiscent of the "sword" rather than of the "ploughshare" . When a rupture with traditional, farmer- and ecology-centered agriculture was brought about, it squared perfectly with the policies of the Western governments aiming at accelerated development. These "progressive" policies led to the substitution of large-scale injections with mineral-N fertilizer for the organics-based soil fertility building of farmers everywhere.

Humanly speaking it is practically impossible to look fathomless evil in the face, so there is nothing strange in the *myth-construction* (Judt) that after World War ll had to ward off the memory of all its frightening evil. The determination to go for "progress" linked up with it. Government bureaucracies, greatly extended during the Depression and the War, used the powerful means at their disposal to find (their presumed) shortcuts to predetermined goals of development. The rationality of this global endeavor was of a peculiar nature: it consisted of the application of the most forceful means to accomplish these shortcuts. This quick-fix rationality (functional rationality sensu Mannheim) was at odds with the sound rationality that should underlie patient and thorough studies of contexts and backgrounds. As industry-based agriculture was a core project of governments everywhere, the dominance of this progress-oriented rationality in agricultural research led to the labeling of farmers' practices and local ecologies as primitive. Research institutes everywhere were molding a root-less agriculture in stead.

The result was an extensive body of knowledge and practices that was not earth-bound. It projected a holographic world in which technocracy could rule from distant centers of government. When reality did not conform, the lofty goal of progress required its reconstruction. For decades reductionist versions of science and technology were held in high esteem, because they provided the "certainty" that the set goals would be attainable. Yet, nature and man defied the reductionist analysis and aims (e.g. chaotic character of most physical systems). In time the government-induced body of knowledge and practices proved to lack roots in soils, ecologies and proven agricultural practices. It virtually embodied e.g. a complete neglect of mycorrhizal symbiosis, soil micro-hierarchy, and organic interactions in the rhisosphere (incl. organic-N nutrition of plants). As such, it offered no access to the real riches of plants, soils and ecologies of traditional agriculture.

In the meantime, based on the certainty that their predetermined goals were attainable governments everywhere induced massive urbanization/de -ruralization. Centrally devised protocols allowed the abolition of the knowledge and practices of local farmers. However, half a century later, we realize that we need these local farmers, their careful labor and their expertise, if we want to regain access to the riches of plants and soils. The aim "to feed the world from the laboratory" has proved to be grotesque. Yet, even though we have failed to achieve most of the realistic possibilities, there is no reason for despair. Where policy makers and researchers are willing to take a step back from center stage, farmers can regain their crucial position in the careful development of local resources. Reinstating the peasant/farmer would not be that difficult: we can build upon centuries of experience. The all-out centralization that was part-and-parcel of our 20th century totalitarian regimes and led to global warfare has proved to be a complete failure.

The problem lies in giving up the "progress" oriented rationality with its presumptuous claim to central expertise, rather than in the reinstatement of truly rooted agriculture. There is an urgent need to evaluate the body of post-war institutional and legal tracts which, unfortunately, will be a painful process due to the fact that so many of the participants in the centralized structures were motivated by true idealism. Our age was destined for tragedy when it opted for the "progress" myth instead of time consuming evaluation. This lack of evaluation did not only cause a failure to exploit most of the riches of plants, soils and traditional agriculture, but caused the system that we built to be unsustainable. At this point it already presents us with some severe dangers, which were simply overlooked within the paradigm that dominated our post- war policies and institutes. Therefore these dangers were able to grow unchecked. Some of them, like those of nitrosamins, have been known for decades by researchers outside the government and industry-directed research circuit. Other such dangers have resurfaced more recently, even though warnings had been issued in earlier decades, e.g. soil fertility losses due to mineral fertilizer application. Such losses already manifested themselves in the course of World War I, and accompanied agriculture's accelerated "industrialization" after World War II (cp. already Röpke 1948).

Grand-scale industrialization was the linchpin of economic policy everywhere, especially after World War ll. It proved to be a dead end (see Breman and Shah's "*Working in the Mill no more*"), but it forced enormous numbers of people to move from rural regions to mega city slums. The high hopes of these people were dashed. We stuck to the war-related structures and methods which had led to the deformation of the economy and society. There is no doubt that in its wake, and not due to some "natural"evolution, the big industry-approach was promoted by the government and gained a dominant position. Yet, post-war social policy, being a serious part of government policy, for a time hided the harsh aspects of this dominance.
In the meantime the intrinsic limits of industrial production and its incompatibility with the life of plants and soils had been forgotten. As a corollary the flawed economic calculus of big industry with all its abuse of natural, social and human capital was applied to agriculture. At government level a uniform economic calculus was introduced that was alien to the central aspects of the lives of people and plants. Recently this has showed up in the grotesque efforts to make education and health care more "business like" , efforts that do not engage the relational core of those human activities.

Decades before governments had already become oblivious to agricultures' dependence on the local care and experience of farmers who acknowledge the individual and non-mechanical character of soils and plants. As a result the clear evidence of the wrongful economic approach towards agriculture was ignored (Kahler 1958). Before long both industrial and agricultural policies reached a dead end.
It is essential to realize that our age was one of historical contingency, no less than previous

periods. Its terrible wars made it unique only in the sense that it required a more profound evaluation. When we nevertheless succumbed to "presentism" and self-confidently declared our "take-off" into the realm of plenty, all we accomplished was an accelerated approach to a dead end.
Still, part of this profound lack of evaluation is understandable. Even people with more insight into the ethical dilemmas than the average researcher (e.g. von Weizsacker) were baffled when the abyss created by World War ll revealed itself, and afterwards did not take enough time for evaluation. It was the wide-spread myth construction that prevented thorough analysis like Weil's and Bonhoeffer's from becoming the starting point of policy making after the war.

In the Netherlands the economist Boeke (Leiden) had published a detailed analysis of National Socialist economics (spring 1941), and the Nazis had put him in a concentration camp for it. Even so he was marginalized after the war, and Hirschfeld, the man who epitomized the Dutch Bureaucracies' collaboration with the Nazis, was not only acquitted, but was quickly repositioned in the center of Dutch economic policy making. It is easy to find countless absurd examples like this one in other countries as well. They are part of the specific "dream"of our age (Butterfield), and make us aware that it is conceivable that we have been engaged in constructing a nightmare.

As to agriculture, the fallacy of our "dream" was evident from the start. From their first appearance in th fossil record terrestrial plants have been soil-bound and rooted in the wondrous riches of soil resources and soil life. The same can be said about agriculture as a whole. And now at this point in time those who are in charge have claimed that it is possible to "rise above" the need for plant rrhizosphere and soil life and have reduced plant growth to a test tube process. Great effort as well as much idealism was invested in making this dream come true and the agricultural landscape itself was refashioned to align itself with the findings of test tube research.
In the end the paucity of these constructions constituted a shrill contrast with the dazzling riches of soils, ecologies and traditional agricultural practices. The powerful positions that got bound up with these constructs, namely agro-industry and food chains, are of a legal and institutional nature only. Because they offer no access to the real riches of soils and plants, it is urgent that they should be evaluated for what they are, the products of root-less constructs and policies. The insatiable thirst of agro-industry and food chains for energy is a strong indication that they are not adaptable to the imminent period of low fossil energy.

During this period the oil supply will gradually dry up, but, fortunately, there will still be plenty of untapped sources of energy left. The potential of wind energy, for example, far outstrips the present extravagant use of fossil energy (Lu et al. 2009). With regard to plant growth and food production, our oil-guzzling 'industrial' agriculture needs mono-cultures even though they cut a poor figure compared with the productivity that derives from diversity in eco-systems (e.g. Fornara and Tilman 2009). Likewise, our present industrial "fertilizers" do a poor job compared to the fund of mineral and organic resources that are available to plants in a farmer and ecology- centered agriculture (add footnote).

Ultimately it boils down to our "concept of greatness" (Weil) and its implementation. Our 20th century opted for centralized power and harvested devastating wars. Restoring soil to its original richness and giving priority to the lives of children and plants can help us harvest food and beauty instead.

Notes:

(1) ‘Functional rationality’ *sensu* Karl Mannheim
(2) Remember the ‘chaotic’ character of most systems
(3) W.Röpke 1948 – Die Gesellschaftskrise der Gegenwart – Erlenbach/Zürich
(4) Cp. J.Breman, P.Shah 2004 - Working in the mill no more – Amsterdam Un.Press
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For Weil etc. refer to the body of this text.

13. Summary in Dutch “Samenvattend overzicht”

The “Samenvattend overzicht” is issued as a small companion volume to this thesis.

Cover illustration:
The Bride, 1912, by Marcel Duchamp (1887-1963)
Philadelphia Museum of Art, the Louise and Walter Arensberg collection.
(Scientific fantasy – a 20th century art style using devices to create fantastic illusions or to substitute real objects for the illusionistic treatment of reality)

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