SPIROCHAETES SEROLOGY AND SALVARSAN

LUDWIK FLECK AND THE CONSTRUCTION OF MEDICAL KNOWLEDGE ABOUT SYPHILIS

Henk van den Belt

STELLINGEN

I

Gezien het feit dat er in Nederland jaarlijks meer geld wordt uitgegeven aan reclame-uitingen in de meest brede zin dan aan HBO-onderwijs, universitair onderwijs en onderzoek en wetenschapsbeleid bij elkaar, is de dikwijls gebezigde aanduiding 'kennismaatschappij' nogal geflatteerd en lijkt de benaming 'reclamemaatschappij' eerder op zijn plaats.

II

De wijze waarop vele natuurbeschermers aankijken tegen exoten vertoont frappante gelijkenis met de manier waarop de partij van Janmaat allochtonen beschouwt.

III

Wie te horen krijgt dat hij ergens *op* afgerekend zal worden, moet vrezen dat er op zeker moment *met* hem zal worden afgerekend.

IV

Gezien de gretigheid waarmee de verantwoordelijke politici ons in het riskante avontuur van een Economische en Monetaire Unie willen storten, wordt de mogelijkheid om een verzekering af te sluiten tegen de gevolgen van 'political failures' node gemist.

V

De uitdrukking 'ja, mits' is logisch gelijkwaardig aan de uitdrukking 'nee, tenzij'.

VI

Om als werknemer je *employability* over langere termijn zeker te kunnen stellen, moet je tegelijk je eigen werkgever zijn.

VII

Om het intellectueel eigendom van de resultaten van wetenschappelijk teamwork wordt vaker gestreden dan men gewoonlijk aanneemt.

VIII

Het adagium 'Volg de actoren' is een ontoereikende methodische leidraad voor het doen van wetenschaps- en technologie-onderzoek.

IX

Het is niet de ontdekkingscontext maar de rechtvaardigingscontext welke een ontdekking tot een ontdekking stempelt.

X

In een constructivistische benadering kunnen de constructie van feiten en de (de)constructie van artefacten op een symmetrische wijze worden behandeld, maar dat betekent niet dat feiten tot artefacten worden gereduceerd.

XI

Het onderscheid tussen kennis en werkelijkheid dient ook door constructivisten te worden gerespecteerd.

XII

Het wetenschappelijk realisme van Michael Devitt houdt zich niet aan de eigen regel dat ontologische kwesties voorafgaand aan epistemische vragen moeten worden beslist.

Stellingen behorende bij het proefschrift
SPIROCHAETES, SEROLOGY, AND SALVARSAN:
Ludwik Fleck and the Construction of Medical Knowledge about Syphilis
van Henk van den Belt
Wageningen, 18 december 1997

SPIROCHAETES, SEROLOGY, AND SALVARSAN

LUDWIK FLECK AND THE CONSTRUCTION OF MEDICAL KNOWLEDGE ABOUT SYPHILIS

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SPIROCHAETES, SEROLOGY, AND SALVARSAN

LUDWIK FLECK AND THE CONSTRUCTION OF MEDICAL KNOWLEDGE ABOUT SYPHILIS

Henk van den Belt

Proefschrift
ter verkrijging van de graad van doctor
op gezag van de rector magnificus
van de Landbouwuniversiteit Wageningen,
dr. C.M. Karssen,
in het openbaar te verdedigen
op donderdag 18 december 1997
des namiddags te vier uur in de Aula

Van den Belt, H.

Spirochaetes, Serology, and Salvarsan
Ludwik Fleck and the Construction of Medical Knowledge About Syphilis / Henk van den Belt
Thesis Landbouwuniversiteit Wageningen - With summary in Dutch
ISBN 90-5485-765-X

Printed in the Netherlands by Grafisch bedrijf Ponsen & Looijen b.v.

PREFACE

The preface offers the author of a book the possibility to acknowledge his intellectual and other debts to all those who in one way or another have been instrumental in helping it to come into existence. It is the perfect occasion for professing humility, because it makes one realize how many others have actually contributed to the completion of a product which nevertheless is usually claimed as one's own intellectual property. Perhaps the idea of being an 'author' and being considered as such is indeed no more than a conventional illusion. As long as even the French postmodernists who have allegedly deconstructed the entire notion of authorship continue to publish books and articles under their own names, however, I have no scruples to do the same.

After having solemnly declared myself herewith to be the 'true and only' author of this book (to prevent possible postmodernist misunderstandings from arising), let me now accomplish the more grateful task of acknowledging my debts to all the persons whom I owe special thanks.

The origin of this book can be traced back to the early 1980s, when Ton van Helvoort, Bart Gremmen and I, all three of us then at the University of Nijmegen, met in an informal reading club to discuss the recently rediscovered work of Ludwik Fleck. Thanks to Ton's perseverance in gathering and checking Fleck's sources, we soon found out that the latter's historical reconstructions were not beyond dispute. Unless my memory plays a trick on me, Bart was the first to discern the contours of an alternative story in the material pertaining to the struggle over the intellectual ownership of the Wassermann reaction. Subsequently, each of us inevitably went their own separate ways. Ton completed a thesis on the history of virus concepts, whereas Bart wrote a philosophical dissertation about the mystery of the practical use of scientific knowledge. Being intellectually more inert and slow, I eventually decided to work up the heterogeneous materials relating to Fleck to a full-fledged thesis in its own right. Ton and Bart had offered me the important initial stimuli to embark on this project. As godfathers they stood at the cradle of this book. In addition I have to thank Bart for permission to use our co-authored article on Fleck's serological thought style ('Specificity in the Era of Koch and Ehrlich', *Studies in History and Philosophy of Science* 21 [1990]: 463-79) as a basis for Chapter VIII of this book.

On several occasions I have given oral presentations on the development of the Wassermann reaction and benefited from the critical remarks of various audiences to elaborate and sharpen my ideas. Such presentations have been given at the First World Congress on Medicine & Philosophy in Paris in 1994 and before Karin Knorr-Cetina's 'Laborstudiengruppe' at the University of Bielefeld also in 1994, at the Science Studies Unit of the University of Edinburgh in May 1996 and during an international workshop on diagnostic practices in medicine held in February 1997 at the Hamburger Institut für Sozialforschung. I have to thank in particular Jens Lachmund for inviting me to the latter workshop and for exchanging

ideas with me on our respective themes of historical research. During my earlier stay at the University of Edinburgh David and Celia Bloor were very obliging by generously offering me hospitality at their home for almost one week. I experienced this stay as an enormous privilege, not least because it enabled me to find out the truth about the apocryphal story disseminated by Ian Hacking, that David is such a total fan of Wittgenstein that his office at home is an exact copy of the latter's office at Cambridge. Since my stay and despite the burden of his official duties as director of the Science Studies Unit, David has maintained a regular correspondence with me and given critical and constructive comments on drafts of nearly all the chapters comprising this book. Even if I have not always been able or willing to follow up his useful suggestions, I must certainly express my special gratitude to his constructive criticism and intellectual encouragement.

Other sociologists of science have also commented on draft chapters of this thesis. Andrew Pickering, working at the University of Illinois at Urbana-Champaign and representing a brand of constructivism different from David's, subjected drafts of Chapters V and VII to critical examination. Although he could not condone my defence of 'interest explanations' and my use of the notion of 'constraints' (as was to be expected from his point of view), his judgements were nevertheless quite jubilant and encouraging. Finally, Robert K. Merton, Professor Emeritus at Columbia University in the City of New York and the reputed 'father of the sociology of science', has rendered me the honour of giving a 13-page commentary on an earlier, and much different, version of Chapter VI. I have taken his criticisms to heart and followed up many of his specific suggestions for change, although I suspect that the final version will not be entirely to his liking because I concede still too much, from his point of view, to constructivist positions. I must also thank Professor Merton for the many 'exhibits' which he sent me along with, and in support of, his written comments - books and copies of several, sometimes not easily accessible papers and materials. In this special sense too, his commentary was solidly documented.

Anthony S. Travis at the Hebrew University in Jerusalem and Deputy Director of the Sidney Edelstein Center in that city read and commented on an earlier version of Chapter VII. His suggestions for linguistic and stylistic improvement have been gratefully accepted. I am glad that we now share an interest in Paul Ehrlich's life and work, in addition to our long-standing common interest in the history of the synthetic dye industry. For what little I possess of the historian's craft skills, I must thank my two former colleagues involved in the research project on the development of the synthetic dye industry (1979-1983) at the Catholic University of Nijmegen, Wim Hornix and Ernst Homburg, who showed me in their different ways that the work of a historian of science can be both thorough and relevant.

I thank Bea Prijn for turning my text into a decent manuscript. It took her a lot of trouble to restore the scars I had inflicted upon it out of sheer computer illiteracy.

In registering my acknowledgements to various persons, I should not forget, of course, to express my gratitude to the two men who were officially in charge with leading me to a successful completion of my thesis project: Professor Michiel Korthals at Wageningen Agricultural University and Professor Henk ten Have at the Catholic University of Nijmegen. Both allowed me much free scope to follow my own inclinations, but on occasion did not hesitate forcing me to make the main line of my argument more clear to myself and to them, particularly when the forest threatened to become invisible because of the trees. It would be pointless to claim that a study which pretends to span the different fields of the philosophy, history and sociology of science is without tensions and imbalances.

Finally, I want to thank my companion in life, Liz Pigmans, for the emotional support and encouragement she has given me during the fairly long period that I needed to complete this dissertation. It is a cliché, but no less true for being so, that such work often strains personal relationships and demands much tolerance and patience from the partner. Liz surely has had to bear her part of the burden. Paraphrasing Holland's foremost constructivist, Wiebe Bijker, let me therefore conclude by exclaiming: Liz, do not despair, there is life again after the writing of a thesis!

TABLE OF CONTENTS

CHAPTER I

FOLLOWING MEDICAL SCIENTISTS THROUGH LABORATORY AND SOCIETY, OR HOW TO REPLICATE AND EXTEND LUDWIK FLECK'S EXAMPLE

This book offers a constructivist analysis of several episodes pertaining to the genesis of modern medical knowledge about syphilis. Part of it goes over old ground that had already been covered in Ludwik Fleck's now classic study from the 1930s, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*.[1] Fleck's monograph deals with the emergence of the modern concept of syphilis and in particular with the construction of a serological test for detecting this disease, the so-called Wassermann reaction. What is remarkable about Fleck's book, at least for a study written during the 1930s, is that it approaches its subject matter from the perspective of a sociological theory of knowledge. This was without precedent. Fleck, a practising physician and bacteriologist but an amateur in philosophy and sociology, drew his inspiration from the Durkheimian tradition in the sociology of knowledge to develop what he called his 'theory of thought styles and thought collectives'. At the beginning of the 20th century, Emile Durkheim and his followers, Marcel Mauss and Lucien Lévy-Bruhl, had started to study the relationship between the social structures of primitive tribes and their world-views or classification systems.[2] Despite the intellectual stimulus he derived from them, Fleck criticized the Durkheimians for their apparent reluctance to extend their sociological approach from the study of primitive belief systems to the analysis of modern scientific knowledge. It was left to Fleck himself to overcome such scruples. Upon its first appearance in 1935, Fleck's monograph largely went unnoticed, but it has since been rescued from oblivion by Thomas Kuhn, who in 1962 noted that Fleck's essay 'anticipated' many of his own ideas.[3] It was however only in 1979, when an English translation appeared, that Fleck's pioneering work was made accessible to a wider

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1 L. Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*, Basel (Benno Schwabe), 1935.

2 E. Durkheim and M. Mauss, *Primitive Classification* (translated and introduced by Rodney Needham), Chicago (The University of Chigaco Press), 1963 [original French essay: 1901-02]; L. Lévy-Bruhl, *How Natives Think*, New York (Washington Square Press), 1966 [French original: *Les fonctions mentales dans les sociétés inférieures*, Paris (Alcan), 1910]; E. Durkheim, *The Elementary Forms of Religious Life*, translated by Karen E. Fields, New York (Free Press), 1995 [French original 1912].

3 T.S. Kuhn, *The Structure of Scientific Revolutions*, Chicago and London (The University of Chicago Press), 1970 [1962], pp. VI - VII.

audience.[4] Soon it then became apparent that Fleck not merely 'anticipated' Kuhn's ideas but went beyond them in important respects. Indeed, it could be argued just as well that Fleck 'anticipates' several of the central ideas of the various strands of constructivism that have emerged in 'post-Kuhnian' science studies from the 1970s onwards.[5] Many proponents of contemporary constructivism view him as a worthy precursor and praise his work as an early contribution to the sociology of scientific knowledge and the constructivist analysis of scientific practice. Fleck is thus widely recognized nowadays as "a pioneer of the sociologically-oriented constructivist approach to history and philosophy of science".[6] Of course, despite the close affinity between Fleck's approach and the work of contemporary constructivists, there are not only similarities but also differences. In Chapter II I will give a detailed comparison to spell out both similarities and differences.

So this book aims at a constructivist description and analysis of the genesis of modern medical knowledge about syphilis. But why go over old ground again? Isn't it a waste of time and effort to do a restudy of some episodes in the history of medical science that have

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4 L. Fleck, *Genesis and Development of a Scientific Fact* (edited by Thaddeus Trenn and Robert K. Merton), Chicago and London (The University of Chicago Press), 1979. In 1980 the German original was republished as L. Fleck, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (edited by Lothar Schäfer and Thomas Schnelle), Frankfurt am Main (Suhrkamp), 1980. References to Fleck's monograph will be given in the main body of the text as a pair of numbers, e.g. (42/58), where the number(s) before the / sign indicate(s) the page number(s) of the English translation and the number(s) after the / sign indicate(s) the page number(s) of the German Suhrkamp edition.

5 *A note on terminology:*
The term 'constructivism' is used here to denote a variety of schools in the 'new' social studies of science and technology. They are 'new' in the sense that they date from the 1970s or later, in contrast to the older Mertonian school which dominated the sociology of science in the 1950s and 1960s. Rather than giving a precise characterization of the defining tenets of modern 'constructivism' (if that would indeed be possible!), let me indicate its extension by simply enumerating some of the schools and approaches that can be subsumed under this term:
- The Strong Programme (David Bloor, Barry Barnes, Steven Shapin);
- The Empirical Programme of Relativism (Harry Collins, Trevor Pinch);
- Discourse Analysis (Michael Mulkay, Jonathan Potter);
- Actor-Network Theory (Bruno Latour, Michel Callon, John Law);
- Ethnographic Laboratory Studies (Karin Knorr-Cetina);
- The Reflexive Programme (Steve Woolgar, Malcolm Ashmore);
- Ethnomethodology (Michael Lynch);
- Symbolic Interactionism (Susan Leigh Star, Joan Fujimoro).

This enumeration reflects only the division of schools existing at a certain moment in time. Indeed, younger constructivist researchers are trying to move beyond such divisions (e.g. Andrew Pickering with his 'turn to practice'). The expression 'sociology of scientific knowledge' (acronym: SSK) is sometimes used to denote the first two approaches. Some investigators (e.g. Karin Knorr-Cetina) prefer to speak of 'constructionism' rather than 'constructivism'. Outsiders often employ the expression 'social constructivism' indiscriminately to refer to all of the above-mentioned schools, but this is inadvisable as representatives of some approaches would certainly object to this label (e.g. Latour, Lynch, Knorr-Cetina).

6 I. Löwy, *The Polish School of the Philosophy of Medicine: From Tytus Chalubinski (1820-1889) to Ludwik Fleck (1896-1961)*, Dordrecht (Kluwer), 1990, p. 125.

already been studied before, presumably in a thorough way? Ideally, the importance of repeating empirical inquiries for testing and validating knowledge-claims should need no special defence, of course, but in the social sciences replication studies are nonetheless often held in low esteem. This common judgement may reflect a serious bias. A study by the American sociologist of science, Susan Cozzens, contrasting the citation records of two classical papers in neuropharmacology and sociology of science (her own field) respectively, gives one food for thought.[7] Whereas the references to the paper in neuropharmacology - the classic 1973 publication by Candace Pert and Solomon Snyder on the opiate receptor[8] - paid extensive attention to the experimental procedures and empirical details associated with the main knowledge-claim (before the latter got stabilized and codified), the pattern of references to the paper in the sociology of science - the 1966 article by Joseph Ben-David and Randall Collins on the influence of role-hybridization in the emergence of psychology as a new discipline[9] - was completely different. A large part of the citations to the Ben-David and Collins paper were of an 'interpretive' or 'conceptual' sort, that is, they linked the paper to more general ideas. Virtually no attention was paid to the empirical details of the work. As Cozzens observes: "The authors might as well have written a short note, or even a letter to the editor, rather than a full article, for all the attention their data received in the citation record. In short, their empirical material was delivered to an empty house".[10] Although I did not do a systematic citation context analysis as Cozzens did, my personal impression is that much the same holds true for the reception of Fleck's work. Almost all commentators have fastened on the conceptual and theoretical issues raised by his pioneering monograph, but have neglected to discuss the empirical adequacy of the case studies used to support his sociological and philosophical views.[11] And while there is nothing wrong with conceptual analysis *per se* - I myself will engage in it, especially in

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7 S.E. Cozzens, 'Comparing the Sciences: Citation Context Analysis of Papers from Neuropharmacology and the Sociology of Science', *Social Studies of Science* 15 (1985): 127-53.

8 C.B. Pert and S.H. Snyder, 'Opiate Receptor: Demonstration in Nervous Tissue', *Science* 179 (1973): 1011-14.

9 J. Ben-David and R. Collins, 'Social Factors in the Origin of a New Science: the Case of Psychology', *American Sociological Review* 31 (1966): 451-65.

10 Cozzens, op. cit. (note 5), p. 147.

11 There is one exception of which I know, and this exception is only partial. Ilana Löwy, who has published a book and several articles illuminating the philosophical and professional backgrounds of Ludwik Fleck (e.g. the work cited in note 6), has also written an article on the history of the Wassermann reaction. See I. Löwy, 'Testing for a Sexually Transmissible Disease, 1907-1970: the History of the Wassermann Reaction', in V. Berridge and P. Strong (eds.), *AIDS and Contemporary History*, Cambridge (Cambridge University Press), 1993, pp. 74-92. This article, however, covers a longer historical time-span than Fleck's monograph and is also primarily based on French and American sources, whereas Fleck analyzed the genesis of the Wassermann reaction in its original German context.

Chapters II and IX -, such a massive overall imbalance in favour of conceptual analysis and to the detriment of empirical discussion is surely not a sign of intellectual health. A reconsideration of Fleck's empirical case studies might contribute to redressing the imbalance. I also believe that a 'replication' study may be illuminating and fruitful in that it offers the possibility to discuss theoretical and conceptual issues raised by Fleck's work in relation to empirical questions. This may be a preferable way to make those issues more tractable. After all, the proof of the pudding is in the eating.

This thesis, however, attempts to be more than a replication of Fleck's original study. By examining several episodes in the genesis of medical knowledge about syphilis I intend to explore and evaluate the usefulness of concepts and theories derived not only from Fleck's work but from modern varieties of constructivism as well. I think this extension of theoretical concerns follows quite naturally once one seriously tries to establish Fleck's important insights and contributions. In determining what Fleck has to say to us, we are unavoidably guided by our own lights and prejudices.[12] In other words, to give an account of Fleck's ideas is to interpret them.[13] It is therefore not remarkable that different strands of constructivism have produced different readings of Fleck's work, emphasizing one aspect or another of the overall theoretical structure as the crucial feature of his approach. Without pretending to offer the definitive reading or ultimate synthesis, I think that the risk of an unduly restricted interpretation can be minimized by taking the views of modern varieties of constructivism into account as fully as possible.

A useful way to simplify the contemporary picture of a bewildering diversity of different constructivisms is to follow Rob Hagendijk and distinguish two broad varieties of constructivism: *moderate constructivism* and *radical constructivism*.[14] The distinction is made according to the extent to which the various approaches challenge deeply entrenched conceptions about nature, society and scientific knowledge. The adherents of the Strong Programme (Barnes, Bloor) and of the Empirical Programme of Relativism (Collins, Pinch) are moderate constructivists. They take a relativist stance with regard to scientific knowledge: variations and alterations in knowledge are explained by relating them to differences and changes in social structures and processes. The independent existence of the latter is

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12 This is an instance of the 'dialogue' between the present and the past as highlighted in Gadamer's philosophy. See H.-G. Gadamer, *Wahrheit und Methode: Grundzüge einer philosophischen Hermeneutik*, Tübingen (J.C.B. Mohr), 1965 (2nd edition).

13 This is also recognized by the hermeneutical phenomenologist Patrick Heelan, when he writes about his own account of Fleck's work: "The summary given above of Fleck's epistemology was guided by my own set of philosophical interests [..]. Any translation of a work like Fleck's will reflect the dominant interests [..] of the translator, and any philosophical critical paraphrase will likewise do the same". See P. Heelan, 'Fleck's Contribution to Epistemology', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Reidel (Dordrecht), 1986, pp. 287-307, on p. 294.

14 R. Hagendijk, *Wetenschap, Constructivisme en Cultuur*, Amsterdam (Thesis), 1996.

presumed and not called into question. The proponents of the Strong Programme even profess to be ontological realists with regard to natural reality, although they do not allow verbal accounts of that reality to figure directly in their explanatory schemes. Radical constructivists, on the other hand, try to define their position in such a way as to circumvent or bypass the epistemological debates between relativism and realism. In contrast to moderate constructivists, they do not think it legitimate to assume the existence of pre-given social structures which can be used to account for the content of knowledge. Rather, both 'nature' and 'society' are seen as being 'co-produced' by science, which is conceived of as a set of constructive practices that create order out of disorder. Radical constructivists reject *a priori* distinctions between 'social' and 'cognitive', 'subject' and 'object', 'nature' and 'society' or 'nature' and 'culture'. Instead of considering such distinctions as explanatory resources available to the analyst, they hold that those very distinctions should themselves be treated as the outcomes of construction processes. Hagendijk mentions as approaches falling under the label of radical constructivism: Knorr-Cetina's ethnographic laboratory studies, Callon and Latour's so-called actor-network theory, and the reflexive programme elaborated by Woolgar and Ashmore. To these can be added Andrew Pickering's 'science-as-practice' approach and the Heideggerian-inspired 'practical hermeneutics' developed by Joseph Rouse. Fleck's work cannot be unambiguously assigned to either the moderate or radical variety of constructivism; certain features of it are in agreement with the former, whereas other aspects exhibit more affinity with the latter variety. Modern forms of constructivism can therefore be used as a basis of comparison to sort out the different tenets and strands in Fleck's work. A detailed discussion of the problems raised by his work may also clarify the issues that divide contemporary constructivists.

In comparison to Fleck's original monograph, the present study has also widened its empirical scope by including two additional episodes from the history of syphilology. Fleck's book, it will be recalled, deals with the genesis of the modern concept of syphilis and in particular with the formation of the Wassermann reaction. These subjects (with suitable extensions) are reconsidered in Chapters III, V and VI of this dissertation. However, I have also included chapters on the discovery in 1905 of the causative agent of syphilis by Schaudinn and Hoffmann (Chapter IV) and on the development of an effective medicine against the disease in 1909-1910 by Paul Ehrlich and co-workers (Chapter VII). These two discoveries (or inventions) fall within the same time period as the development of a serological test by August Wassermann and his collaborators, which constitutes the main subject of Fleck's essay and is discussed in Chapters V and VI. It so happened that the first 10 years or so of the 20th century were an exceptionally productive decade in the whole history of syphilology.[15] The discoveries responsible for this rapid progress were intimately

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[15] According to Crissey and Parish, this decade was "far and away the most fruitful in the 500-year history of the disease". See J.T. Crissey and L.C. Parish, *The Dermatology and Syphilology of the Nineteenth Century*, New York (Praeger), 1981, p. 394.

related to each other. It would be wrong to consider them as just so many isolated 'point events'; they should rather be seen as nodes of an interconnected and expanding conceptual *network* of medical knowledge.[16] This consideration constitutes the main reason for including the above-mentioned two additional episodes in the history of syphilology, which are not extensively dealt with in Fleck's book (apart from occasional asides), into the compass of this investigation. Adding these two case studies has the further advantage of creating extra opportunities for an empirical discussion of the several issues that are raised by Fleck's work and modern varieties of constructivism.

Central issues

An issue that must certainly be addressed is generated by the central constructivist claim that facts are not simply 'found' but are actively 'constructed' (or 'socially constructed'). How exactly is this thesis to be interpreted, what are its implications, and can it be succesfully defended against criticism? Although Fleck does not employ the by now rather overused terminology of 'social construction', his view on the formation of scientific facts essentially agrees with modern constructivist versions. As he put it, facts are not states of affairs which can be directly ascertained by properly passive observation of natural reality; they are not so much discovered as *invented* in a prolonged social process of gaining collective experience. He supported this view by a sustained and impressive criticism of the possibility of 'pure observation' or observation without presupposition, using vivid illustrations from his own field of bacteriology to great effect (87-95/115-24). To most of his contemporaries, however, the very title of Fleck's monograph expressed an unfathomable paradox. This is nicely brought out by an anecdote recounted by Thomas Kuhn, who had shown a copy of Fleck's monograph to his Harvard mentor James Bryant Conant. When a few years later Conant became US High Commissioner for Germany, he mentioned the title of the book to one of his German associates. The latter's reaction was one of perplexity and disbelief: "How can such a book be? A fact is a fact. It has neither genesis nor development".[17] Today the constructivist claim that scientific facts are (socially) constructed has become rather commonplace, but it is still sufficiently offensive to arouse outcries of indignation among philosophers of a rationalist and realist persuasion. The Canadian philosopher Mario Bunge, for instance, holds that "in matters of knowledge the only genuine social constructions are

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[16] The term 'network' used here alludes to the notion of a Hesse net or Hesse network (after the British philosopher of science, Mary Hesse). See M. Hesse, *The Structure of Scientific Inference*, Berkeley and Los Angeles (University of California Press), 1974. See also Chapter II for further explanation. Fleck himself conjured up the image of a "network in continuous fluctuation" (79/105) to explain the development of the Wassermann reaction as a result of a junction of various lines of thought.

[17] T. Kuhn, 'Foreword' to L. Fleck, op. cit. (note 4), p. VIII.

the scientific forgeries committed by two or more people"[18], his prime example being the notorious Piltdown fossil skull. To assert that scientific facts are socially constructed is often held to detract from the authority and credibility of science.[19] Modern constructivists are thus accused of making it effectively impossible to draw a distinction between facts and artefacts.[20] It is significant that a similar charge had already been brought against Fleck in an early review of his work.[21] I think this particular criticism deserves careful consideration. The objection can be answered in two ways. A philosophical reply will be given in the final chapter of this book. A more empirically oriented response, however, is also possible. In fact, in their daily practice natural scientists are frequently confronted with the problem of whether or not they are dealing with an artefact in their observations and measurements. It would seem that the way in which they deal with such a recurrent problem is itself amenable to empirical inquiry. This problem figures prominently in the historical case-study on the discovery of the aetiological agent of syphilis that will be discussed in Chapter IV. There I attempt to show that the social construction of 'facts' and the social (de-)construction of 'artefacts' can be handled simultaneously using a single analytical framework.

The debate turns not only on the noun 'construction' but also on the precise meaning of the adjective 'social'.[22] "Cognition", Fleck proclaims, "is the most socially-conditioned activity of man, and knowledge is the paramount social creation" (42/58). Knowledge is social in the fundamental sense that it is always an outcome of intense 'intellectual interaction' between many individuals. There can be no strictly private knowledge, just as there can be no private language. Fleck rejects traditional epistemology which considers cognition as a two-way affair between subject and object. He adds a third component, the *thought collective*, defined as a community of persons maintaining intellectual interaction, which he sees as the social bearer of a certain *thought style*, (loosely) defined as a disposition

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18 M. Bunge, 'A Critical Examination of the New Sociology of Science Part 2', *Philosophy of the Social Sciences* 22 (1992): 46-76, on pp. 66-67.

19 P.R. Gross and N. Levitt, *Higher Superstition: The Academic Left and Its Quarrels with Science*, Baltimore MD (Johns Hopkins University Press), 1994.

20 R. Nola, 'There are More Things in Heaven and Earth, Horatio, Than are Dreamt of in Your Philosophy: A Dialogue on Realism and Constructivism', *Studies in History and Philosophy of Science* 25 (1994): 689-727. To avoid misunderstanding: 'artefact' is taken here in the sense of a spurious fact or spurious phenomenon produced by the investigation itself, not in the sense of a human-made useful object like a tool or some other device.

21 H. Petersen, 'Ludwig Fleck's Lehre vom Denkstil und dem Denkkollektiv', *Klinische Wochenschrift* 15 (1936): 239-42.

22 For a critical review of several forms of (social) constructivism which focuses on the different meanings of 'construction', see S. Sismondo, 'Some Social Constructions', *Social Studies of Science* 23 (1993): 515-53; for a philosophical analysis of the social nature of science, see S.H. Downes, 'Socializing Naturalized Philosophy of Science', *Philosophy of Science* 60 (1993): 452-68.

for selective perception and thinking. A fully socialized member of a thought collective, Fleck holds, looks through his own eyes but sees with the eyes of the collective.[23] Fleck's way of conceiving of the social is thus rather 'collectivistic', giving primacy to the collective as a whole over its individual members. This collectivistic tendency is echoed in the strong approval which he bestows on the following statement by Ludwig Gumplowicz: "The greatest error of individualistic psychology is the assumption that a *person* thinks. [..] What actually thinks within a person is not the individual himself but his social community" (46-47/63). Fleck also denies the reputed 'intellectual fathers' of the discoveries of the aetiological agent of syphilis and of the serodiagnostic test for detecting the disease - Fritz Schaudinn and August Wassermann, respectively - their status as 'discoverers': "Wassermann, like Schaudinn, is rather a standard-bearer in discovery than its sole agent" (42/57). Several non-constructivist commentators have criticized Fleck's collectivistic or "extremely anti-individualistic standpoint".[24] Foremost among the critics of this standpoint is Thomas Kuhn, who on his first acquaintance with Fleck's book already felt repelled by a "vaguely repulsive perspective of a sociology of the collective mind" and still rejects this particular aspect of his work.[25] Presumably, the famous (non-constructivist) sociologist of science Robert Merton, co-editor of the English translation of Fleck's monograph, would also take exception to Fleck's collectivism, because in his work he has always tried to steer a careful middle course between the individualistic and collectivistic views of discovery and invention.[26] The anthropologist Mary Douglas, by contrast, has expressed her full approval of this feature of Fleck's approach.[27]

Some, but by no means all, modern constructivists would agree with Fleck's emphasis on the essentially social character of cognition. Harry Collins, for example, spells out several ways in which scientific activity is social, summarizing his analysis as follows: "Thus, learning scientific knowledge, changing scientific knowledge, establishing scientific knowledge, and maintaining scientific knowledge are all irremediably shot through with the social. They simply *are* social activities".[28] He even comes close to Gumplowicz's dictum:

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[23] L. Fleck, 'To Look, to See, to Know [1947]', in Cohen and Schnelle, op. cit. (note 13), pp. 129-152, on p. 134.

[24] R.S. Cohen and T. Schnelle, 'Introduction' to Cohen and Schnelle, op. cit. (note 13), p. XI.

[25] See Kuhn's foreword to Fleck, 1979, op. cit. (note 4), on pp. IX-X.

[26] R.K. Merton, *The Sociology of Science* (edited and introduced by N.W. Storer), Chicago (The University of Chicago Press), 1973.

[27] M. Douglas, *How Institutions Think*, London (Routledge & Kegan Paul), 1987, especially pp. 12-19.

[28] H.M. Collins, *Artificial Experts: Social Knowledge and Intelligent Machines*, Cambridge MA and London (MIT Press), 1990, p. 5.

"To put the issue in the starkest form, the locus of knowledge appears to be not the individual but the social group; what we are as individuals is but a symptom of the groups in which the irreducible quantum of knowledge is located. Contrary to the usual reductionist model of the social sciences, it is the individual who is made of social groups".[29] Similar declarations on the primacy of the social can be found with other proponents of the sociology of scientific knowledge (SSK), especially with the adherents of the Strong Programme like David Bloor and Barry Barnes. Other constructivists, however, have meanwhile become rather disenchanted with the term 'social', if not with its content. Symptomatic for this tendency is the decision of Bruno Latour and Steve Woolgar to drop the term 'social' from the subtitle of the second (1986) edition of their famous book *Laboratory Life*: now it has simply become 'the construction of scientific facts'.[30] The alleged reason for this remarkable deletion, as they state it in the postcript to the second edition, is that "[b]y demonstrating its pervasive applicability, the social study of science has rendered 'social' devoid of any meaning".[31] Actually, the changed terminology is indicative of the fact that both Latour and Woolgar have completely abandoned the original explanatory ideals of the sociology of scientific knowledge. Woolgar disputes the adequacy of so-called 'interest explanations' in the style of the Strong Programme (i.e. the attempt to explain the esoteric content of scientific knowledge from social interests).[32] Latour, for his part, holds that facts and artefacts as well as new social structures are 'co-produced' in the constructive practices of techno-scientists. It is therefore not acceptable, in his view, to adduce social factors when explaining the constitution of facts and artefacts, because those factors are themselves in need of explanation. More particularly, he maintains that interests, far from being given, are malleable and can be modified and manipulated by the deliberate efforts of techno-scientists. To get their projects off the ground, the latter have to 'enroll' other parties by 'capturing' and 'translating' their interests.[33] It remains to be seen, however, whether this is so fundamentally at odds with the explanatory tenets of the Strong Programme as Latour himself suggests. Anyway, as he himself declares, Latour is not in the business of offering social explanations. Nor are such explanations very popular among representatives

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[29] Ibid., p. 6.

[30] B. Latour and S. Woolgar, *Laboratory Life: The Construction of Scientific Facts*, Princeton NJ (Princeton University Press), 1986. The first edition was entitled *Laboratory Life: The Social Construction of Scientific Facts*, Beverly Hills (Sage), 1979.

[31] Ibid. (Latour and Woolgar, 1986), p. 281.

[32] S. Woolgar, 'Interests and Explanation in the Social Study of Science', *Social Studies of Science* 11 (1981): 365-94.

[33] B. Latour, *Science in Action: How to Follow Scientists and Engineers through Society*, Milton Keynes (Open University Press), 1987. 'Artefacts' is taken here in the sense of human-made useful objects such as tools, machines, and other devices.

of other constructivist schools like ethnomethodology (Lynch), ethnographic lab studies (Knorr-Cetina), discourse analysis (Mulkay), 'practical hermeneutics' (Rouse) and the more recent study of 'science-as-practice' (Pickering). In view of such widespread skepticism, I consider it one of the tasks of the present thesis to re-examine and elucidate the possible role of social explanations in general and of interest explanations in particular within science studies.

It is perhaps no coincidence that Latour, Knorr-Cetina, and Lynch - all three of them prominent critics of social explanations, albeit in different ways - originally entered the social studies of science field through participant observation in scientific laboratories undertaken during the 1970s.[34] For Knorr-Cetina it is the notion of a 'laboratory' itself, suitably redefined in analytical terms, which promises to provide an alternative theoretical framework for science studies. Adherents of the Strong Programme will certainly object that this still leaves the macrosocial determination of laboratory work out of account. Responding to the macrosociological challenge of (microsociological) laboratory studies - namely, how to detect the influence of societal relations of power -, Latour has found a felicitous way of posing the problem, or rather of rhetorically reversing it and reformulating it in terms congenial to his own approach: How to explain the power of the laboratory over the surrounding society (its leverage effect), or in other words, how does the science get out of the laboratory? Latour's own answer to this question is that, as a matter of fact, the science never really gets out of the laboratory. Scientific results can be made applicable to society at large only by extending, as far as possible, the laboratory conditions which gave rise to their production in the first place. In other words, the 'outside' world has to be reshaped in the image of the laboratory.[35] Whatever the merits of Latour's answer (and I will certainly have to consider them), there is no doubt that the question he raised is a fruitful one. It will be a leading question in the two case-studies reported in Chapters V and VII. Indeed, my main criticism of Fleck's historical study of the Wassermann test for syphilis is precisely that his account stays too much within the boundaries of the serological laboratory.

A recent trend in constructivism is the movement away from a primary concern with 'science-as-knowledge' toward an explicit interest in 'science-as-practice'.[36] In the opinion

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[34] B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, Beverly Hills (Sage), 1979; K. Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Oxford and New York (Pergamon), 1981; M. Lynch, *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory*, London (Routledge and Kegan Paul), 1985.

[35] B. Latour, 'Give Me a Laboratory and I will Raise the World', in K.D. Knorr-Cetina and M. Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*, London/Beverly Hills/Delhi (Sage), 1983, pp. 141-70.

[36] D. Gooding, T. Pinch and S. Schaffer (eds.), *The Uses of Experiment*, Cambridge (Cambridge University Press), 1989; A. Pickering (ed.), *Science as Practice and Culture*, Chicago and London (University of Chicago Press), 1992; J.Z. Buchwald (ed.), *Scientific Practice: Theories and Stories*

of the protagonists of this 'turn to practice', the proponents of the older sociology of scientific *knowledge* (SSK) are still too preoccupied with the cognitive aspects of science. Among the newer constructivists the focus is more on the constructive engagement of scientists with the material world as it is conducted under definite (though adaptable) material and social conditions. Doing science is not just testing theories against the data provided by experiments; it also involves assembling, deploying and modifying *resources* (both material and conceptual) for the *production* of knowledge. In the new approach it is, to use Ian Hacking's distinction, the 'intervening' rather than the 'representing' that is put into the foreground.[37] The recent focus on science-as-practice has also infused a whiff of 'pragmatic realism' into constructivism, although it is still a matter of debate how much and what kind of realism can be allowed without subverting the aims of constructivist science studies. Some (e.g. Peter Galison) would admit that reality imposes 'constraints' in one way or another on the constructions of scientists, while for others (in particular Andrew Pickering) all talk about 'constraints' is anathema. This dispute may also be relevant to the interpretation of Fleck's work in view of the ambiguities related to certain submerged realist tendencies in it. Ironically, Fleck's monograph is not only 'appropriated' by the adherents of the Strong Programme as a pioneering example of the sociology of scientific knowledge, it is also 'claimed' by the newer constructivists of 'practical hermeneutics' and the science-as-practice approach in virtue of its exemplary analysis of the practical work performed by Wassermann and his collaborators in developing a serological test for syphilis.[38] Pickering points out that Fleck uses the significant metaphor of 'tuning' in his analysis of the Wassermann reaction: "[Wassermann] and his co-workers [..] 'tuned' their sets until these became selective" (86/113; Fleck refers in this connection to the efforts to enhance the success rate of the serological reaction by manipulating the quantities of each of the five required reagents.) The term 'tuning' is also used by Pickering himself to designate the so-called 'mangle of practice', or the dialectic between resistance and accommodation, as the most general pattern discernable in the pursuits of scientists. When scientists come up against 'resistances' or unexpected problems, they can of course 'accommodate' by revising their theoretical models of the phenomenon under investigation, or their interpretations of how the apparatus works, but it is just as likely that they will try to adjust their material procedures

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of Doing Physics, Chicago and London (University of Chicago Press), 1995; A. Pickering, *The Mangle of Practice: Time, Agency & Science*, Chicago and London (University of Chicago Press), 1995.

37 I. Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge (Cambridge University Press), 1983.

38 J. Golinski, 'The Theory of Practice and the Practice of Theory: Sociological Approaches in the History of Science', *Isis* 81 (1990): 492-505; J. Rouse, *Knowledge and Power*, Ithaca and London (Cornell University Press), 1987; J. Rouse, *Engaging Science: How to Understand its Practices*, Ithaca and London (Cornell University Press), 1996.

and tinker with their instruments. In general, scientists seek to achieve coherence between all three elements. Pickering stretches the meaning of the term 'tuning' still further by extending it to the revision of theories ('conceptual tuning') and the reconfiguring of social relations ('social tuning'). Thus even the social relations (work styles, institutional structures etcetera) under which scientific work is conducted are subject to the 'mangle of practice'. It will by now be clear that the science-as-practice approach offers a broad agenda of inquiry. In empirical studies it encourages us to pay attention to such issues as the acquisition of resources, the refractoriness of raw materials (including test animals!), and the coordination and organization of scientific work.[39]

Latour's question, how the science gets out of the laboratory, can also be fruitfully taken up within this framework as it is able to develop a deeper understanding of the problems that must be confronted when laboratory findings or products are to be transferred to the outside world. As I already said, I will have occasion to examine this question in the two empirical case-studies reported in Chapters V and VII. Especially in the latter chapter, dealing with the development and clinical introduction of an effective medicine against syphilis by Paul Ehrlich and his team, the empirical potential of the science-as-practice approach will be explored.

Up till now not much has been said about a concept that plays a central role in Fleck's theory, the concept of *thought style*. That is no mere coincidence. Contemporary constructivists are less than enthusiastic about this concept, at least about the explanatory role it is given in Fleck's work. In particular they object to Fleck's ascription of "an absolutely compulsive force" to thought styles (41/57). The historian of science Jonathan Harwood, intellectually close to the Strong Programme, accuses Fleck of abandoning his sociological approach and falling back into an intellectualist and idealist way of thinking. Thought styles emerge in and through the actions of a scientific community. By imputing to such styles the power to constrain thought and action, Harwood maintains, Fleck reverses the causal arrow and depicts human actors as being at the mercy of their cognitive creations.[40] (In Chapter II I will discuss the so-called finitist theory of meaning and concept application which provides the underlying philosophical rationale to such objections.)

Modern constructivist strictures against the concept of thought style do not directly affect its use in a *descriptive* context. However, Fleck's descriptive use of this notion also deserves critical attention. He gives a characterization of the so-called 'serological thought style' (or

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39 In many respects the position reached by Karin Knorr-Cetina in the tradition of ethnographic laboratory studies is quite similar to Pickering's science-as-practice approach as described above. See K. Knorr-Cetina, 'Laboratory Studies: The Cultural Approach to the Study of Science', in S. Jasanoff et. al. (eds.), *Handbook of Science and Technology Studies*, Los Angeles (Sage), 1994, pp. 140-66.

40 J. Harwood, 'Ludwik Fleck and the Sociology of Knowledge', *Social Studies of Science* 16 (1986): 173-87, on pp. 182-83. See also D. Bloor, 'Some Determinants of Cognitive Style in Science', in Cohen and Schnelle, op. cit. (note 13): 387-97.

the thought style of the 'serologists' collective') by enumerating a number of elements that supposedly make up this particular style (59-64/79-84). But this raises the question why these elements actually constitute a thought style, or more generally: when do different elements add up to a style; what makes a collection of elements a stylistic whole? A further question is whether such stylistic units necessarily have to coincide with the boundaries of a particular scientific discipline. Or to press the issue: *Can* a thought style be limited to a single discipline? Harwood argues strongly and convincingly for the *comparative* use of styles as a descriptive category: "Notice that unless one identifies recurring elements in *several* cultural sectors, there is little point in using the term *style*".[41] This would mean that (some of) the 'recurring elements' that Fleck identified as constituting the (presumed) style of the serologists' collective must also be found in other disciplinary (or cultural) sectors, and that such a collection of recurring elements must be matched with at least one contrastive set of elements covering a comparable range of sectors. The implication is that the questions raised by Fleck's idea of thought styles cannot be adequately dealt with as long as the analysis is restricted to the discipline of serology or the development of the Wassermann reaction. My decision to include additional episodes from the history of syphilology into the compass of this thesis, thus allowing consideration of several 'sectors', will therefore help fulfil one of the requirements Harwood deems essential for a proper stylistic analysis. Happily, with regard to the other requirement of finding a *contrasting* style, some of the preparatory work has already been done by the historian Pauline Mazumdar. In her book *Species and Specificity* she interprets the history of immunology (encompassing serology) as a continuous opposition between the protagonists of 'pluralist' and 'unitarian' views or styles.[42] Fleck's so-called 'serological' thought style can thus both be extended to new disciplinary sectors and re-characterized as an instantiation of Mazumdar's 'pluralist' (or specificity) style as opposed to the 'unitarian' style. After this redescription we can also more profitably re-examine the riddle of the apparent coercive force of the pluralist thought style, or the 'power' of the idea of specificity, within microbiology and immunology.

Synoptic preview

The theoretical and empirical scope of this study thus clarified, an outline of the chapters which follow can now be presented.

In Chapter II I shall systematically compare Fleck's theories with the approaches adopted by contemporary constructivists. My strategy is partly to use modern forms of constructivism as a foil for extracting relevant and valuable insights from the richness of Fleck's elaborations, partly to identify theoretical and conceptual issues that can possibly be

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41 J. Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933*, Chicago and London (The University of Chicago Press), 1993, p. 9.

42 P.M.H. Mazumdar, *Species and Specificity: An Interpretation of the History of Immunology*, Cambridge (Cambridge University Press), 1995.

clarified through an empirical 'replication' of Fleck's work. In the following chapters I will therefore deliberately put on spectacles grinded according to different constructivist recipes in order to illuminate various aspects of the concrete episodes under study (always allowing for a comparison with Fleck's empirical analyses) and to elucidate, as far as possible, the theoretical issues involved. Starting out from moderate constructivist approaches (Chapters III and IV) I will move on to more radical forms of constructivism (Chapters V and VII). In the concluding chapter the spectacles themselves will be the object of examination.

Chapter III reconsiders the historical genesis of the modern concept of syphilis, which is also the main subject of the first part of Fleck's monograph. Its purpose is to demonstrate the *prima facie* legitimacy and fruitfulness of a broadly constructivist approach towards the historical genesis of disease concepts. The aim of the chapter is therefore less to criticize than to consolidate and extend Fleck's insights. I do so by following the example of many modern constructivists in adopting Mary Hesse's finitist theory (or 'network theory') of meaning as my starting-point. The results of my explorations vindicate Fleck's view of the (socially) constructed and 'culture-laden' character of the modern concept of syphilis. I also follow up one of Fleck's more specific suggestive remarks, to the effect that moral considerations in particular entered into the construction of concepts of syphilis. To substantiate this suggestion, I pay special attention to the (formerly presumed) 'hereditary' and (still uncontested) venereal character of the disease. By performing a cross-cultural comparison with some non-venereal tropical and subtropical diseases closely related to syphilis, I hope to loosen the hold on our minds of the 'venereal fixation' characteristic of the modern concept of syphilis. Some of the findings discussed in this chapter are calculated to unsettle the tranquillity of mind of hard-headed anti-constructivists and thus, by the same token, to earn credibility for a broadly constructivist approach.

Chapter IV describes and analyzes the discovery of the causative agent of syphilis, a subject to which Fleck devotes some brief but interesting passages in his monograph. In this chapter I shall put on the type of spectacles that belong to the special brand of (social) constructivism represented by Harry Collins.[43] Characteristic of this approach, which preferably focuses on the study of scientific controversies, is that the analyst takes a strictly agnostic stance as to the reality or otherwise of the (purported) natural phenomenon under dispute and treats the arguments and actions of the conflicting parties in a symmetrical and impartial way. The empirical subject of this chapter provides a favourable occasion to follow these precepts. During the years 1905-1907 two different microorganisms, *Spirochaeta pallida* and *Cytorrhyctes luis*, were in fact proposed and defended as the looked-for aetiological agent of syphilis. Such a situation would seem to be a pre-eminent case calling for a symmetrical treatment in the modern constructivist sense. Indeed, Fleck himself already

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[43] H. Collins, *Changing Order: Replication and Induction in Scientific Practice*, Beverly Hills (Sage), 1985; see also H. Collins and T. Pinch, *The Golem: What Everyone Should Know About Science*, Cambridge (Cambridge University Press), 1993.

presented a symmetrical analysis of this episode in his monograph, but - as I will argue - the particular account he offered lacks plausibility. I will undertake a new effort, more sustained and hopefully more thorough-going than Fleck's failed attempt. In the debate on the aetiology of syphilis several issues were raised that are highly relevant for a constructivist analysis, e.g. about the reliability of (microscopic) observation and the possibility of creating 'artefacts' by staining tissue preparations. Fleck's general views on the role of perception and observation, expounded in his monograph and other writings, prove to be useful and pertinent. The case also illustrates the general constructivist insight that appeal to formal methodological rules and criteria is unable to resolve controversies. Finally, the chapter will present a conceptual analysis of the notion of 'discovery' in line with the findings of the historical case-study. A consistently sustained constructivist approach leads to a major rethinking of this notion, taking up but going beyond Thomas Kuhn's views on the matter. To put it briefly and somewhat paradoxically, using the familiar distinction of traditional philosophy of science, the constructivist proposal is to transfer the category of discovery from the 'context of discovery' to the 'context of justification', or rather, to the social context of validation and acceptance.

Chapter V deals with the genesis and development of the Wassermann reaction as a clinically usable serological test for detecting syphilis. This is also the main subject of Fleck's monograph. The 'scientific fact' to which the title of his book refers is "the fact that the so-called Wassermann reaction is related to syphilis", which was, according to Fleck, "one of the best established medical facts". The establishment of this fact is seen as the result of a cooperative effort by the so-called 'serological thought collective' led by August Wassermann, which under the influence of the social urgency of the syphilis problem and ancient ideas about syphilitic blood worked unceasingly to improve and perfect the test until a practically usable diagnostic instrument was finally obtained.

In this chapter I examine the empirical and theoretical adequacy of Fleck's analysis. I take issue with several elements of his account, but the main objection is that he simply ignores the 'clinical connection' and depicts the development of the Wassermann reaction as if it occurred exclusively within the four walls of the laboratory, with serologists busily 'tuning their sets'. In my alternative account of the whole episode, the interaction between serologists and clinicians figures much more prominently. This account is loosely inspired by Bruno Latour's ideas on 'enrollment' and 'translation of interests'.[44] To convince clinicians of the value and reliability of the Wassermann reaction, serologists were initially caught in a 'dilemma of application'[45]: if the outcome of the test agreed with the clinicians' own judgement, it would tell them nothing new; if it disagreed with their judgement, they

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44 Latour (note 33).

45 This notion is derived from M. Mulkay, T. Pinch and M. Ashmore, 'Colonizing the Mind: Dilemmas in the Application of Social Science', *Social Studies of Science* 17 (1987): 231-56, on p. 233; these authors elaborate on Bruno Latour's notions of enrollment and translation of interests.

would doubt its validity and reliability. Through active involvement of clinicians ('enrollment'), the dilemma could be overcome by, on the one hand, changing the technical execution and clinical meaning of the Wassermann reaction, and, on the other, redefining the diagnostic and therapeutic interests of clinicians to which the test would attend. Ultimately, a practically usable serological test for syphilis was achieved by the joint efforts of serologists and clinicians. This analysis also elucidates some riddles that were left unresolved within Fleck's account. However, I intend to do more than rectifying the shortcomings of Fleck's account. I also want to venture, albeit rather cautiously, some radical-constructivist exercises. In addition to the Latourian notions of enrollment and translating interests already mentioned, I have taken into the account the views of Pickering and Rouse on the character of scientific activity as a practice, in particular with regard to the realization of experimental systems and the practical engagement with raw materials, test animals and 'patient material'. In contrast to these radical constructivists, however, I see no reason to reject the notion of interests as used by the moderate constructivists on grounds of principle. In my judgement, a modest role even accrues to the professional interests of serologists and clinicians to understand and explain the development of the Wassermann reaction. Interest explanations, in my view, are perfectly compatible with the phenomenon of interest translation highlighted by Latour.

Chapter VI is devoted to an analysis of the dispute over the intellectual ownership of the Wassermann reaction, which erupted in the aftermath of the development of this serological test - as a bitter epilogue, so to speak. It was in 1921 that August Wassermann got embroiled in a lively polemics with, among others, his former collaborator Carl Bruck and his former critic Eduard Weil over the question of who could call himself the legitimate intellectual father of the Wassermann reaction. The reason for devoting a separate chapter to what was considered at the time a rather 'unsavoury' dispute, is that it offers us a unique possibility to critically examine on the basis of historical material the much-criticized 'collectivistic' or 'anti-individualistic' stance characteristic of Fleck's approach. The conclusion of my analysis is that this 'collectivistic' feature of Fleck's sociology of knowledge made him, indeed, ill-equipped to adequately deal with the struggle over the intellectual ownership of the Wassermann reaction. He uncritically takes the assertions made by the protagonists during the course of this struggle as simply reflecting their views on the development of the Wassermann reaction, without taking account of the fact that these utterances were made for strategic reasons to bolster up their respective claims to the intellectual property of the serological test or to defeat the claims of others. My own account of this 'unsavoury' episode is inspired by Robert Merton's sociology of science which takes a more balanced stance on the relationship between individual and collective.[46] For the Mertonian sociology of science, the struggle over intellectual property between (former) team members is a still unexplored theme (it has mostly concentrated on priority disputes between independent

[46] Merton, op. cit. (note 26).

scientists). I also relax Merton's restriction on analyzing the content of scientific knowledge: the question of *who* has had a creative part in the making of a discovery is indissolubly bound up with the question of *what* exactly has been discovered. In the struggle over the Wassermann reaction participants could argue their case only by taking a stand on both questions. In this way I attempt to integrate Mertonian insights within a broadly constructivist approach. In view of the fact that Merton's sociology of science has received a barrage of criticism from constructivist quarters, the attempt may be interpreted as a plea for rehabilitation.

Chapter VII deals with the development of an effective chemotherapeutic medicine against syphilis by Paul Ehrlich and his co-workers. Fleck's monograph contains only a few passing remarks on this development. The reason for including a chapter on this subject, apart from the fact that it constitutes an important node in the expanding conceptual network of syphilology, is that Ehrlich's work appears to be an excellent case on which to perform the kind of analysis that has become customary in more recent forms of (radical) constructivism, viz. those connected with the movement away from 'science-as-knowledge' toward 'science-as-practice'. Andrew Pickering is the most outspoken exponent of this tendency, but it is also manifest in Joseph Rouse's work and in Karin Knorr-Cetina's earlier contributions. Some aspects of Latour's work too can be brought under this heading. The crucial question of how laboratory results can be applicable, or be made applicable, to the world outside the laboratory (in other words, how the 'science' gets out of the laboratory), which he has raised to such prominence, can be fruitfully taken up in an analysis of 'science-as-practice'. Such an analysis had already been partially attempted in Chapter V, but is conducted in a more sustained and systematic way in this chapter. The object of analysis is Paul Ehrlich's practice of 'experimental therapeutics' (or 'chemotherapy'), which modern pharmacologists often consider to be the beginnings of rational drug design. To inaugurate this practice he built up a vast 'construction machinery' (Knorr-Cetina) by acquiring the necessary funds and material and human resources through an intimate symbiosis with the German chemical (synthetic dye) industry. In order to put these resources to productive work, he borrowed from this same industry a model of research management and division of scientific labour, which he tailored to his own needs by combining chemical work with the large-scale testing of chemical preparations on experimental animals. The secret of Ehrlich's success was in fact the combination of 'chemical mass-labour' with 'biological mass-labour' and the creation of 'experimental systems' (Rouse) through the suitable selection of test animals. Of course, Ehrlich and his collaborators had to overcome many constraints and limitations of the raw materials and test animals. The entire venture was not oriented to finding a remedy against syphilis immediately from the start; it was only during the course of the programme and through 'opportunism in context' (Pickering) that the turn to this disease occurred. Initially, Ehrlich had boasted that through his approach, using animal experiments on an extensive scale, the most 'optimal' drugs could be developed and selected so as to make the final test on man no more, as it were, than proving the sum. Things would dramatically turn out otherwise. After an effective substance had been found against syphilitically infected rabbits, the gap separating laboratory and outside world had to be bridged and this proved to be an even more exacting task than developing the medicine in the first place. A constructivist analysis inspired by the science-as-practice approach highlights the difficulties that have to be confronted when laboratory products are to find their way into the 'wider' society. In this particular case, the insufficiency of Latour's own answer to the question he raised is clearly revealed: the clinical introduction of Salvarsan involved much more than simply transplanting laboratory conditions to the outside world, it also involved the 'normalization of the object' (Knorr-Cetina), legal, social and political intervention, and continued experimentation with the medicine after its commercial introduction (the thesis of 'society as laboratory').

Chapter VIII does not deal with a particular episode in the history of syphilology, but offers a reconstruction of the so-called 'serological thought style' which according to Fleck determined the way of thinking and acting of the serologists' collective led by Wassermann. In contrast to Fleck, contemporary constructivists generally reject, on the basis of finitist arguments, such an explanatory use of the notion of thought style. This still leaves the possibility that the term refers to an interesting phenomenon worthy of investigation as an explanandum. Fleck's descriptive characterization of the serological thought style, however, also raises questions. Following Harwood I argue that this concept can be meaningfully employed only in a comparative way. It does not make sense to speak of the thought style of a serologists' collective, if this same style cannot be recognized in other sectors than serology and if it cannot be contrasted with different styles. To carry out this comparative investigation I will draw upon the various episodes in the history of syphilology as discussed in the preceding chapters, which together cover several sectors of medical science (nosology, aetiology, serology, and therapy). The unity of Fleck's 'serological' thought style will be found in the basic idea of specificity. Viewed in this way it represents the so-called 'pluralist' style, previously analyzed by Pauline Mazumdar, which can be contrasted with the so-called 'unitarian' style. Harwood's requirements for the meaningful employment of style concepts can thus be met. Finally I show that the 'power' of the pluralist style (and of the basic idea of specificity) can be partially explained from the unprecedented power structure which the Koch-Ehrlich group had built up in German medical science in the years around 1900.

In Chapter IV, finally, I have attempted to develop, by building on the results of the preceding chapters, a reasoned and well-considered position vis-à-vis the two big fundamental problems which continue to haunt constructivist studies of science and technology, to wit, the problem of realism and the question of how to conceive of 'the social' and the relationship between the individual and the collective. The latter problem includes the question of how to adequately conceptualize the notion of 'social practices' in general and of 'scientific practices' in particular. Moderate and radical constructivism take very different positions with regard to both fundamental problems. Radical constructivists push the construction metaphor to such extent that in their view not just plastics or genetically modified organisms but also microbes, electrons and quarks are held to be constructed by science. Latour and Woolgar's 'splitting-and-inversion' model about the genesis of facts often lies hidden behind this view. It is because of such views that radical constructivism clashes with current realist conceptions, despite the fact that radical constructivists themselves believe to have transcended the entire debate between realism and anti-realism. Among the moderate constructivists, the term 'construction' refers exclusively to the formation of knowledge about natural reality, not to that reality itself or its constituent objects. The adherents of the Strong Programme even take a stance as common-sense realists vis-à-vis reality. I think such a position is excellently defensible. It is preferable, in my opinion, to so-called 'scientific realism', which is too strongly committed to the existence of those theoretical entities which are currently accepted in science. Moreover, this variety of realism also argues rather problematically from the practical success of applications of science to the truth of the underlying theories and fails to appreciate the open-ended character of concept application as emphasized in the finitist theory.

As regards the second fundamental problem I should declare that I do not share the hypercritical skepticism which radical constructivists display vis-à-vis 'the social'. I have attempted to rebut each of several objections which they have adduced against 'social' explanations of the content of scientific knowledge. Latour's argument, for example, to the effect that society does not provide a solid basis for such explanations because technoscientists themselves act as 'society builders', is only a half-truth, for even when acting in that capacity technoscientists act under definite, not freely chosen or fully controllable societal relationships. I also confront Rouse's criticism that moderate constructivists tend to treat the validation of knowledge-claims within self-enclosed scientific communities and the position defended by Knorr-Cetina that the validation of such claims has no need for a social locus beyond the laboratory itself. I further reject the criticism expressed by Woolgar that any attempt to demonstrate the social determination of scientific knowledge invariably involves portraying competent and knowledgeable actors as mere puppets or 'cultural dopes'. I admit, however, that it is very difficult to strike a proper balance between the spontaneity and agency of individual actors, on the one hand, and the effects of social structures ('constraints'), on the other hand. It would therefore be very welcome to have a theory which is able to do justice to both aspects. That is why I finally examine Anthony Giddens's 'structuration theory' to see whether it fulfils these desiderata. In the end it appears that the modification of Giddens's theory proposed by William Sewell may be reasonably satisfactory. Whereas Giddens views social structure as a virtual order consisting of a set of rules and resources that is reproduced in concrete practices, Sewell reformulates the duality of structure as a duality of virtual elements, namely rules or cultural schemas, and actual elements, namely resources. The deployment of (material and human) resources is informed by cultural schemas; conversely, in order to be reproduced the latter must actually be used in the accumulation of resources. Within this framework the 'agency' of individuals is conceived as empowerment through access to resources and the competence to apply existing cultural schemas to new contexts. Sewell's emphasis on the transposability of cultural schemas to new situations exhibits similarity to the finitist view. He also conceives of 'agency' as thoroughly social. Finally I argue that his conceptualization of the notion of social practices is able to incorporate the valuable elements in Pickering's 'science-as-practice' approach and Rouse's 'practical hermeneutics' without taking the dubious 'posthumanist' and 'anti-social' tenor of the latter approaches also on board.

CHAPTER II

LUDWIK FLECK AND MODERN CONSTRUCTIVISM

1. Introduction

Historically, Ludwik Fleck's doctrine of thought styles and thought collectives represents a 'missing link' connecting the older Durkheimian tradition in the sociology of knowledge and modern forms of constructivism. When he launched his bold programme for a sociologically-informed, comparative epistemology in the 1930s, logical positivism was one of his main targets. Precisely because of the dominant position of this school of thought in the philosophy of science, however, Fleck's work found little resonance with professional philosophers among his contemporaries, as it was seen to deviate too much from the accepted standards of logical analysis and rational reconstruction. Fleck's book *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* largely went unnoticed when it was published in 1935. It received reviews in some German medical weeklies but did not draw the attention of philosophers and sociologists, despite the fact that Fleck wrote some additional articles in both German and Polish to popularize his views.[1]

Of course, the political circumstances of the times were not auspicious for a Polish Jew who published (also) in German.[2] The German occupation of Poland some years later brought disaster to Fleck and his family. In 1943 he was sent to Auschwitz and from there he was deported in 1944 to a vaccine production unit in the Buchenwald concentration camp. After the war Fleck wrote two or three more articles on epistemology and the sociology of knowledge (one of these reflecting on a curious experience in Buchenwald)[3], but he concen-

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1 The following English versions are available:
- 'Scientific Observation and Perception in General [1935]', in R.S. Cohen and T. Schnelle (eds), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, pp. 59-78;
- 'The Problem of Epistemology [1936]', ibid., pp. 79-112;
- 'On the Question of the Foundations of Medical Knowledge [1935]', *Journal of Medicine and Philosophy* 6 (1981): 237-56;
- 'Some Specific Features of the Serological Way of Thinking [1937]', *Science in Context* 2 (19-88): 343-44.

2 Biographical details about Fleck's life and work can be found in T. Schnelle, *Ludwik Fleck: Leben und Denken*, Freiburg (Hochschulverlag), 1982.

3 These are available in English:
- 'Problems of the Science of Science [1946]', in Cohen and Schnelle, op. cit. (note 1), pp. 113-27;
- 'To Look, to See, to Know [1947]', in ibid., pp. 129-51;
- 'Crisis in Science [1960]', in ibid., pp. 153-58 (this article was not published during Fleck's lifetime).

trated his energies on a successful career in Polish medical science. Toward the end of his life he and his wife got permission to emigrate to Israel and join their son living there.

For Fleck, the belated recognition that his 1935 monograph received in 1962 by being mentioned in the Preface of Thomas Kuhn's epoch-making *The Structure of Scientific Revolutions* as an essay that anticipated many of the latter's ideas, was just too late. One year before he had died in Israel. When an English translation and a German re-edition finally made Fleck's monograph accessible to a wider audience in 1979 and 1980, respectively, a vigorous constructivist tendency had meanwhile emerged in the philosophy, history and (particularly) sociology of science which this time was to secure a much stronger resonance to his ideas. Indeed, Fleck became posthumously celebrated as a precursor and pioneer of modern constructivism.

In retrospect many have wondered how a rather unknown medical microbiologist, and an amateur in philosophy and sociology to boot, living in the relatively provincial town of Lwów in pre-war Poland, could develop the highly original constructivist approach to the philosophy and sociology of science that we now recognize as a classic contribution. Fleck's own doctrine would rule out attributing such originality to an accidental outburst of purely individual creativity. Different authors have come up with a number of intellectual influences which may have made a decisive contribution to the formation of his thought. Ilana Löwy has argued that Fleck's professional experience in microbiology and immunology (in which he defended rather heterodox positions challenging mainstream views) constituted a major source of inspiration for his constructivist epistemology.[4] She has also drawn attention to the so-called 'Polish School of Philosophy of Medicine' - a tradition of philosophical reflection on the foundations of medicine by physicians-cum-philosophers like Titus Chalubinski (1820-1889) and Zygmunt Kramsztyk (1840-1920) - as an important background to Fleck's thought.[5] Other authors like Thomas Schnelle and Jerzy Giedymin have explored the possible influences of the more well-known analytically oriented 'Philosophy of Lwów' (Twardowski, Ajdukiewicz, Chwistek) on Fleck's epistemological ideas.[6] Further influences include Gestalt psychology, Niels Bohr's interpretation of quantum physics, and the Durkheimian school in the sociology of knowledge. Without denying the importance of the other intellectual influences on Fleck's thought, I will be primarily concerned in section 2 with the significance of the Durkheimian tradition for the genesis of his approach. Linking his thought to

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4 I. Löwy, 'The Epistemology of the Science of an Epistemologist of the Sciences: Ludwik Fleck's Professional Outlook and its Relationship to his Philosophical Works', in Cohen and Schnelle, op. cit. (note 1), pp. 421-42.

5 I. Löwy, *The Polish School of Philosophy of Medicine: From Tytus Chalubinski (1820-1889) to Ludwik Fleck (1896-1961)*, Dordrecht (Kluwer), 1990.

6 T. Schnelle, 'Ludwik Fleck and the Influence of the Philosophy of Lwów', in Cohen and Schnelle, op. cit. (note 1), pp. 231-65; J. Giedymin, 'Polish Philosophy in the Inter-War Period and Ludwik Fleck's Theory of Thought-Styles and Thought-Collectives', in ibidem, pp. 179-215.

this tradition will prepare the way for a more penetrating discussion of Fleck's doctrine of thought styles and thought collectives in comparison to modern forms of constructivism in section 3.

2. The path to a comparative epistemology

The lure of the sociology of knowledge

Fleck's first epistemological essay dates from 1927.[7] Its title shows that the characteristics of his own field of activity, medicine, constituted the object of his reflections. Fleck defended the view that diseases or 'morbid units' can in no way be considered as objectively given entities, but have to be seen as produced by the medical way of thinking, through selective observation and deliberate abstraction. Fleck noted that medical terminology itself testifies to the historical variability of disease classifications: witness the ubiquitous use of prefixes like 'para' and 'pseudo' (e.g. typhoid and para-typhoid, anaemia and pseudo-anaemia).[8] Although, as Löwy points out, Fleck's view with regard to the constructed character of disease concepts was by no means exceptional within the Polish School of Philosophy of Medicine[9], he soon generalized this constructivist approach. What holds for diseases, Fleck realized, also holds for their aetiological agents. Hence bacterial species too must be considered as at least partially constructed concepts, as he argued in his next epistemological article.[10] He now extended his constructivist views beyond the domain of medical science to include other forms of scientific knowledge.

Following recent philosophical interpretations of the new quantum physics (in particular those of Niels Bohr), Fleck denied the existence of an objective, absolute reality independent of human observation and cognition. "For cognition", he wrote, "is neither passive contemplation nor acquisition of the only possible insight into something given. It is an active, live relationship, a reshaping and being reshaped, in short, an act of creation. Neither the 'subject' nor the 'object' receive a reality of their own; all existence is based upon interaction and

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7 L. Fleck, 'Some Specific Features of the Medical Way of Thinking [1927]', in Cohen and Schnelle, op. cit. (note 1), pp. 39-46.

8 Ralph Gräsbeck provides a striking example of this terminological tendency: "There is a disease called pseudopseudohypoparathyroidism. Such a monstrous name tells you at once that the diagnosis of hypoparathyroidism has been subdivided into more and more entities. [What about the name 'hypoparathyroidism' itself? It suggests a prior history of subdivisions - HvdB]". See R. Gräsbeck, 'Health and Disease from the Point of View of the Clinical Laboratory', in L. Nordenfelt and B.I.R. Lindahl (eds.), *Health, Disease, and Causal Explanations in Medicine*, Dordrecht (Reidel), 1984, pp. 47-60, on p. 56.

9 Löwy, op. cit. (note 5), p. 220.

10 L. Fleck, 'On the Crisis of "Reality" [1929]', in Cohen and Schnelle, op. cit. (note 1), pp. 47-57, on p. 52.

is relative".[11] For Fleck, observation was not just 'theory-laden', as we would call it today, but also '*culture*-laden' and socially conditioned. Any new epistemology, therefore, had to be "brought into a social and cultural-historical context".[12] At this stage Fleck was clearly receptive to a *sociological* input into his epistemological project.

Around 1929 Fleck must have become acquainted with some of the leading ideas and insights of the Durkheimian sociology of knowledge, especially through the German translation of *Les fonctions mentales dans les sociétés inférieures*, written by Lucien Lévy-Bruhl, a follower of Durkheim.[13] In the introduction to the German edition, the Viennese philosopher Wilhelm Jerusalem also expounded Durkheim's views on the sociology of knowledge.[14] Fleck was also familiar with Jerusalem's contribution to a volume on the sociology of knowledge edited by Max Scheler.[15] Karl Mannheim's works were apparently unknown to him (curiously enough, Mannheim's notion of 'thought style' would also become a key-concept in Fleck's theory!). So Fleck's familiarity with the classical sociology of knowledge was rather narrow and superficial. Only Lévy-Bruhl's and Jerusalem's ideas did he know from first hand; Durkheim's ideas were known to him through the intermediary of these two writers.

We may nonetheless presume that Fleck got excited by the enthralling prospect of a new and positive theory of knowledge, based on the comparative method, that was set out in the introduction to Lévy-Bruhl's book:

> "To be able to understand the processes by which institutions have been established (especially among undeveloped peoples), we must first rid our minds of the prejudice which consists in believing that collective representations in general, and those of inferior races in particular, obey the laws of a psychology based upon the analysis of the individual subject. Collective representations have their own laws, and these (at any rate in dealing with primitives) cannot be discovered by studying the 'adult, civilized, white man'. On the contrary, it is undoubtedly the study of the collective representations and their connections in uncivilized peoples that can throw some light upon the genesis of our categories and our logical principles. Durkheim and his collaborators have already given examples of what may be obtained by following this course, and it will doubtless lead to a theory of knowledge, both new and positive, founded upon the

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11 Ibid., p. 49.

12 Ibid., p. 48.

13 L. Lévy-Bruhl, *Das Denken der Naturvölker*, Vienna (Braumüller), 1921; originally *Les Fonctions Mentales dans les Sociétés Inférieures*, Paris (Alcan), 1910. English translation: L. Lévy-Bruhl, *How Natives Think*, New York (Washington Square Press), 1966.

14 W. Jerusalem, 'Vorbemerkungen des Herausgebers', in L. Lévy-Bruhl, *Das Denken der Naturvölker* (translated by Paul Friedländer), Vienna (Braumüller), 1921, pp. V - XVII.

15 W. Jerusalem, 'Die soziologische Bedingtheit des Denkens und der Denkformen', in M. Scheler (ed.), *Versuche zu einer Soziologie des Wissens*, Munich (Duncker und Humblot), 1924, pp. 182-207; reprinted in V. Meja and N. Stehr (eds.), *Der Streit um die Wissenssoziologie*, Frankfurt am Main (Suhrkamp), 1982, Vol. I, pp. 27-56.

comparative method".[16] (On the next page, Lévy-Bruhl also spoke about "the comparative, that is, the sociological method".)

However path-breaking and inspiring this formulation of a sociologically-based comparative epistemology may have been in Fleck's judgement, he also censured Lévy-Bruhl severely for not delivering upon this promise.[17] Nor did he see the programme of a comparative epistemology actually implemented in the works of Durkheim and Jerusalem. Although Fleck derived many stimulating ideas from Durkheim, Lévy-Bruhl and Jerusalem, he found that their approaches were still embryonic and represented only the first steps toward a mature sociological theory of cognition. Their major shortcoming, in Fleck's opinion, was that they applied the principle of the 'social conditioning of thinking' to the thought of primitive peoples only, not to modern scientific thought:

"However, these embryos [the theories of Durkheim, Lévy-Bruhl, and Jerusalem] lack consistency, because they did not succeed in extricating themselves from the prejudice that modern scientific thinking presents a basic exception, being 'objective' and not subjected to the principle of social conditioning".[18]

Both Lévy-Bruhl and Jerusalem held that the liberation of the individual from the social bondage of the tribe or other closely-knit community and thereby from the concomitant grip of collective representations on his consciousness was sufficient to create a situation in which objective perception and purely logical reasoning were possible. Said Lévy-Bruhl:

"The attributes we term objective, by which we define and classify entities of all kinds, are to the primitive enveloped in a complex of other elements much more important, elements exacting almost exclusive attention, at any rate to the extent allowed by the necessities of life. But if this complex becomes simpler and the mystic elements lose their predominance, the objective attributes *ipso facto* readily attract and retain the attention. The part played by perception proper is increased to the extent in which that of the mystic collective representations diminishes".[19]

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16 Lévy-Bruhl, *How Natives Think*, op. cit. (note 13), pp. 3-4. Part of the same passage, in slightly different translation, is quoted in Fleck (46/62-63). Of course, designations like 'inferior races' or 'uncivilized peoples' or even 'primitives' will no longer be considered acceptable today.

17 See the passage in 'To Look, to See, to Know', op. cit. (note 3), p. 140 where Fleck accuses Lévy-Bruhl of "depart[ing] from his own theory".

18 Fleck, 'The Problem of Epistemology [1936]', op. cit. (note 1), p. 80.

19 Lévy-Bruhl, *How Natives Think*, op. cit. (note 13), p. 336. Compare the quoted passage in slightly different translation in Fleck (47-48/65).

Similarly Jerusalem: "Only the strengthened individual acquires the ability to state facts purely objectively [..]".[20] This "strengthened individual" (or "the independent and self-reliant personality") was seen as the product of a historical process of social differentiation and increasing division of labour. The suggestion is that as soon as the old social bonds have been loosened sufficiently, modern science can be started immediately by the unaided efforts of the emancipated individual who alone is capable of objective perception and logical reasoning. Fleck objected strongly to such a fairy-tale. He went at great length to argue that there is no such thing as 'objective perception' which would automatically ensue once one rids oneself of social prejudices. The perception of scientifically accepted properties, Fleck argued - assuming that Lévy-Bruhl and Jerusalem would accept such properties to be 'objective' -, must first be learned. It requires training and participation in a scientific group. In other words, scientific observation is itself socially conditioned.

Although he found the sketch of a programme for a sociologically-based comparative epistemology in the writings of Lévy-Bruhl and Jerusalem, it was thus left to Fleck himself to further develop and consistently implement this programme. In arguing for the desirability and necessity of a comparative epistemology, he vehemently opposed the existing dominant version of the theory of knowledge, which he called 'speculative epistemology' or *epistemologia imaginabilis* (the latter designation was chosen in analogy to the speculative *anatomia imaginabilis* of the late Middle Ages). According to Fleck, this form of epistemology is too much concerned about "what cognitive thinking *ought* to be like, and too little about what it really does look like".[21] Its inquiries "are almost always limited to a few symbolic examples and logical connections, preferred over and above all other connections, between the objects of investigation" (173 note 17/50 note 17). In criticizing 'speculative epistemology' Fleck was clearly aiming at the standard analyses of the logical positivists (and of the representatives of the 'Philosophy of Lwów') who considered it their task to provide rational reconstructions and justifications of scientific knowledge.

In Fleck's view, a sociologically-oriented comparative epistemology should be less one-sided and much more concerned with what cognitive thinking really looks like. In such an approach, modern science would lose the privileged epistemological status that had been tacitly granted to it and that served to exempt it from the meddlings of the sociology of knowledge. A comparative epistemology would not hesitate to study and compare different types of thinking, e.g. primitive, archaic, infantile, psychotic and also scientific thought-

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20 W. Jerusalem, in Meja and Stehr, op. cit. (note 15), p. 39. On p. 38 of this same article, Jerusalem sounds much more 'symmetric' with regard to primitive and modern scientific thought when he states that the mechanism of mutual reinforcement or 'social consolidation' [*soziale Verdichtung*], which gives such figments of the primitive imagination like spirits and demons "some degree of reality and stability", is also active and necessary within science. This latter insight was valuable to Fleck, who pointed out the apparent contradiction in Jerusalem's article.

21 Fleck, 'The Problem of Epistemology [1936]', op. cit. (note 1), p. 80.

forms from a uniform point of view (cf. 51/70). Perhaps modern science may legitimately lay claim to certain special virtues, but these can be determined only at the end of our investigations: "It is only by way of a comparative method, in the framework of general sociology of thinking that we can get acquainted with the features of scientific thinking".[22]

To those who hold that epistemology is concerned first of all with how scientific knowledge is logically constructed and systematically legitimized (what Fleck calls its *Legitimierung*) and not with its historical genesis - or, to translate this in Reichenbach's well-known distinction, that its business resides in the *context of justification* and not in the *context of discovery* -, Fleck has a ready-made answer:

> "Whatever is known has always seemed systematic, proven, applicable, and evident to the knower. Every alien system of knowledge has likewise seemed contradictory, unproven, inapplicable, fanciful, or mystical. May not the time have come to assume a less egocentric, more general point of view and to speak of comparative epistemology?" (22/34).

Fleck's advocacy of a comparative, less 'egocentric' epistemology exhibits a striking similarity to the considerations which led David Bloor and Barry Barnes, in the 1970s, to the formulation of the so-called Strong Programme in the sociology of science. Just like Fleck they took inspiration from the investigations of anthropologists into the thought of primitive peoples (Lévy-Bruhl in Fleck's case, Mary Douglas and Evans-Pritchard in the case of Bloor and Barnes). Their respective criticisms of the classical sociology of knowledge (for Barnes and Bloor represented by Durkheim, but especially by Mannheim) are also quite similar. Fleck reproaches the exponents of the Durkheimian tradition of not being consistent enough to extend their sociological approach to an analysis of modern scientific knowledge. Their characteristic error, according to Fleck, was "an excessive respect, bordering on pious reverence, for scientific facts" (47/85). The content of science was held to be 'objective' and therefore not amenable to sociological analysis. The (Durkheimian) sociology of knowledge thus failed to become a sociology of *scientific* knowledge. In a similar vein, Bloor criticized the "lack of nerve and will" of the old sociologists of knowledge (including Durkheim and Mannheim) which made them reluctant to "bring science within the scope of a thoroughgoing sociological scrutiny".[23] Barnes and Edge also noted that "the sociology of knowledge failed to live up to its name: it long confined itself to the realm of error and ideology, and studiously avoided anything suspected of validity".[24] In a moment we will see that Fleck actually came very close to formulating the principles of symmetry and impartiality that were

[22] Fleck, 'Problems of the Science of Science [1946]', op. cit. (note 3), p. 127.

[23] D. Bloor, *Knowledge and Social Imagery*, second edition, Chicago (University of Chicago Press), 1991 (first edition 1976), p. 4.

[24] B. Barnes and D. Edge (eds.), *Science in Context: Readings in the Sociology of Science*, Milton Keynes (Open University Press), 1982, p. 65.

originally stated by Bloor and are held to be constitutive of all modern forms of constructivism. But first we have to discuss the relationship between Durkheim's sociology and modern constructivism.

Durkheim and constructivism: the finitist elaboration

For adherents of the Strong Programme such as Barnes and Bloor, the later work of Emile Durkheim is seminal to the sociology of scientific knowledge. Although they criticize Durkheim for not extrapolating his approach from the investigation of primitive belief systems to the study of modern science, they also indicate how the Durkheimian approach can properly be so extended. It will be useful to have a closer look at Durkheim's sociology of knowledge and the elaboration proposed by Barnes and Bloor.

The sociology of knowledge can be said to have emerged in 1903 when Emile Durkheim and his nephew Marcel Mauss published their famous essay on *Primitive Classification*[25], followed in 1912 by Durkheim's voluminous book *Les formes élémentaires de la vie religieuse*.[26] The term *Wissenssoziologie* (sociology of knowledge) may have been coined in the 1920s by the German philosopher Max Scheler, but the domain of the new specialism had already been clearly staked out by the previous explorations of Durkheim and his French school.

In their essay *Primitive Classification*, Durkheim and Mauss report a mass of ethnographic data on the cosmologies or classification systems of Australian Aborigines and North American Indians. Their main thesis is that the classification of things reproduces the classification of men[27], or in other words, that the cosmology held by a particular tribe reflects its social organization. Thus a tribe may be successively divided into moieties, marriage classes, clans, sub-clans etcetera, each of the divisions being represented by a particular totem or sub-totem. Animals, plants, minerals, rivers, stars, in fact everything under and above the sun, are also assigned to and are considered to be an integral part of these social divisions. Natural classifications are not merely anthropocentric; they are, as Durkheim and Mauss emphasize, *sociocentric*.[28] Even modern scientific classifications betray there primitive social origins, according to Durkheim and Mauss, for ultimately the hierarchical relation-

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25 E. Durkheim and M. Mauss, 'De quelques formes primitives de classification: contribution à l'étude des représentations collectives', *Année Sociologique*, vol. VI (1901-2), Paris, 1903, pp. 1-72. English translation: E. Durkheim and M. Mauss, *Primitive Classification* (translated and introduced by Rodney Needham), Chicago (The University of Chicago Press), 1963.

26 E. Durkheim, *Les Formes Elémentaires de la Vie Religieuse*, Paris (Alcan), 1912; a new English translation by Karen Fields has recently appeared as E. Durkheim, *The Elementary Forms of Religious Life*, New York (Free Press), 1995.

27 *Primitive Classification* (note 25), p. 11.

28 Ibid., p. 86.

ship between genera and species derives from the hierarchical organization of tribal social life:

> "It is because human groups fit one into another - the sub-clan into the clan, the clan into the moiety, the moiety into the tribe - that groups of things are ordered in the same way".[29]

In his later work on *The Elementary Forms of Religious Life*, Durkheim undertook to demonstrate the social genesis not just of the 'logical' function of classification but also of the other 'categories' of human thought besides 'class', like space, time and causality. He also considered it a primary task for the sociology of knowledge to offer a social explanation of the phenomenon of religion, which he defined in terms of the distinction between the sacred and the profane. Religion, according to Durkheim's theory, represents the moral authority of society over its individual members.

Barnes and Bloor discern in Durkheim's work an implicit programme for the comparative study of systems of natural classification as they are systematically related to the social structures to which they correspond. The following passage from the *Elementary Forms* looks rather promising from this point of view:

> "[..] a certain intuition of the similarities and differences created by things has played a role in creating [..] classifications.
> But a feeling of similarity is one thing; the notion of kind is another. Kind is the external framework whose content is formed, in part, by objects perceived to be like one another. The content cannot itself provide the framework in which it is placed. The content is made up of *vague and fluctuating images* [..]. By contrast, the framework is a definite form having fixed contours, but can be applied to an indefinite number of things, whether perceived or not and whether existing or possible. Indeed, the potential scope of every genus is infinitely greater than the circle of objects whose resemblance we have become aware of through direct experience. [..] The idea of genus is a tool of thought that obviously was constructed by men".[30]

It is, however, doubtful whether Durkheim himself intended to assert that each society has its own system of classification which mirrors its social structure.[31] According to Warren Schmaus, Durkheim's theory of the social genesis of the categories in primitive society "provides no basis for establishing identities between scientific classifications and social classifications in contemporary society".[32] As Schmaus succinctly summarizes Durkheim's

29 Ibid., p. 83.

30 *Elementary Forms* (note 26), pp. 147-48.

31 W. Schmaus, *Durkheim's Philosophy of Science and the Sociology of Knowledge*, Chicago and London (University of Chicago Press), 1994, p. 205.

32 Ibid., p. 257.

theory: "We all have the same idea of subsuming species under genera because we all have totemists for ancestors [..]".[33] It would indeed seem that such a theory leaves little room for studying empirical variations in the relationship between social structure and cognitive content under contemporary conditions.

Durkheim held that the very objectivity of modern science could itself be explained from the social conditions of modernity. The internationalization of social life and the increase in communication between persons of different societies are the most salient influences here:

> "If logical thought tends more and more to jettison the subjective and personal elements [..] the reason is [..] that a new kind of social life gradually developed: international life [..]. As that international life broadens, so does the collective horizon [..] As a result, things can no longer fit within the social frames where they were originally classified; they must be organized with principles of their own; logical organization thus differentiates itself from social organization and becomes autonomous. This, it seems, is how the bond that at first joined thought to defined collective entities becomes more and more detached and how, consequently, it becomes ever more impersonal and universalizes. Thought that is truly and peculiarly human is not a primitive given, therefore, but a product of history [..]".[34]

Durkheim's vision is not without subtlety. Under modern conditions scientific concepts and classifications are no longer "social frames" but become increasingly "autonomous"; this entire development is however the result of social factors. We can give it a paradoxical formulation: on Durkheim's view, social factors are ultimately responsible for the fact that social factors do not normally enter into the very content of science. That is why Durkheim's sociology of knowledge did not develop into a sociology of *scientific* knowledge.

All this is of course no conclusive argument against the attempt to develop Durkheim's embryonic views on natural classification in a direction that was not intended by himself. Indeed, it would be perfectly possible to argue with Durkheim against Durkheim. Fleck's objections to the possibility of 'objective perception' postulated by Lévy-Bruhl also apply here. It is implausible to presume that 'social frames' gradually give way to 'autonomous' classifications. How on earth could it ever be possible, one might object, for things to become "organized with principles *of their own*" (emphasis added)? After all, it is still human beings who have to sort things into classes or kinds; things do not classify themselves. Durkheim himself expressly said so in the passage quoted earlier: "The content cannot itself provide the framework in which it is placed. [..] The idea of genus is a tool of thought that obviously was constructed by men".[35]

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33 Ibid.

34 Durkheim, *Elementary Forms*, op. cit. (note 26), p. 446.

35 Fleck would also have objected to Durkheim's vision of a broadened international social life as the relevant social factor determining the general character of modern thought. To a similar view propounded by Jerusalem he replied: "The concept of a thought collective comprising the whole of *Homo sapiens* is of little use, because the intellectual interactions between different types of human

Barnes and Bloor have argued that Durkheim's insight into the 'constructed' character of natural classifications can indeed be fruitfully extended to the study of modern scientific thought.[36] They admit that Durkheim lacks a fully adequate theory of the classificatory process that would have enabled him to understand the relations between social structures and natural classifications, but they believe that the required theoretical basis has meanwhile become available in the form of the so-called *finitist* or *network theory*, developed by the British philosopher of science, Mary Hesse.[37] I will explain the central ideas of this theory here. Basically, the theory is about the application of concepts or terms (classes, kinds) to cases or instances with which people are confronted in their experience and about the ways concepts are linked to each other. It is denied that there is such a thing as an 'inherent meaning' which would determine how a given concept should be applied. In other words, there is no pre-determined domain of application or extension attached to a concept. Whether a newly appearing instance (say a certain aquatic bird) falls under concept A (say the concept of 'duck') or under concept B (say the concept of 'goose') or under any other concept is not fixed in the nature of things but is a matter for communal judgement and decision. Perhaps after some deliberation the new bird will be reckoned to be a duck because it is (considered to be) more 'like' all those other birds which have already been classed as ducks than those birds which have been classed as geese. Judgements of similarity and difference are, however, never simple and straightforward. It can be plausibly argued that any single object or instance is similar to, yet also different from, any other object or instance. (There is no *metric* for overall similarity, establishing how sameness in one respect may compensate for difference in another!) At a certain point in time the 'extension' of a given concept is no more than the finite set of things that have been brought together under that concept; those things may be held to exhibit some (culturally selected) 'similarity relation'. New members of society can be introduced to the use of a concept by teaching them to perceive the established 'similarity relation' that is exemplified by the various instances of the concept. Thus children are taught the proper use of the term 'duck' under the authoritative guidance of adults who show them both examples and counter-examples ("Yes, this is a duck and that is a duck, and that one too; no, that one over there is not a duck but a goose; and the black bird swimming here is also not a duck, it is a moorhen.") in the hope that they will somehow

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society are too weak" (174 note 41/68 note 41).

36 See in particular D. Bloor, 'Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge', *Studies in History and Philosophy of Science* 13 (1982): 267-97.

37 M. B. Hesse, *The Structure of Scientific Inference*, London (Macmillan), 1974. There are several presentations of the finitist or network theory available in the constructivist literature. A textbook-like presentation can be found in J. Law and P. Lodge, *Science for Social Scientists*, London (Macmillan), 1984. More sophisticated is the account given in B. Barnes, *T.S. Kuhn and Social Science*, London (Macmillan), 1982. A carefully balanced exposition is presented in the more recent book by B. Barnes, D. Bloor and J. Henry, *Scientific Knowledge: A Sociological Analysis*, London (Athlone), 1996.

pick up the relevant 'similarity relation'. And that is what normally happens, of course. One might object that giving explicit verbal definitions constitutes a viable alternative for teaching the use of a concept, but this only shifts the problem to the terms figuring in the definition ("So a duck is a web-footed bird, you say, but then explain the use of the term 'web-footed' to me!").

If there is no 'inherent meaning' of a concept which compels its 'proper' application in the present, then neither does the established usage of a term determine its future applications. The future applications of terms are *open-ended* in principle (and even their past applications are revisable!). There is always more than one way to proceed in extending our concepts to new instances. Such extension to new cases must be equated with the extension of an analogy. Whether the next instance falls under a given concept is a matter of collective judgement, negotiated on the basis of more or less varying individual intuitions. Often, there may be a strong and generally shared sense of how to proceed, and 'everybody' may be convinced that it is 'the only right way' to go on, but this does not the least diminish the importance of the point of principle that is raised by the finitist or network theory: every act of classification and every extension of an analogy is sociologically problematic, even if such acts as a matter of fact occur routinely and in a taken-for-granted manner thanks to a massive unreflective agreement in practice.

Until now we have largely focused on the way concepts relate to the instances which are taken to fall under their purview (it is this aspect that is highlighted in the designation *finitist* theory), but concepts are also related to each other. They are mutually connected through 'laws' or 'generalizations', thus forming conceptual networks or fabrics (this aspect explains why the designation *network* theory is also used). It is not just empty taxonomies or classification systems that are culturally transmitted; simultaneously beliefs about the world are passed on to later generations: the two components are fused together in one package. The important point to note is that all the indeterminacy and open-endedness which characterize the relationship of concepts to their instances automatically carry over to the conceptual network as a whole. It is the entire network that confronts experience. Any 'law' linking two or more concepts (e.g. "Fire is hot") can be upheld in the face of (apparently) 'recalcitrant' experience, if some repair work is done at some other place in the fabric. This, of course, is just another formulation of the well-known Duhem-Quine Thesis. If some parts of the network are held stable and intact come what may, it is by virtue of a conventional community decision to treat them in that way. Communities may have different inclinations and preferences about which parts of the network actively to protect and which parts to modify in the light of experience that impinges on the whole fabric. To capture these selective preferences, Mary Hesse introduced the notion of 'coherence conditions', i.e. requirements that indicate how to maintain the integrity of the entire network. In her view, both biological constraints of 'learning organisms' and 'culturally conditioned metaphysical principles' can in principle fulfil the role of coherence conditions. Barnes and Bloor, however, dismiss her

recourse to the realm of culture as 'idealistic'; they prefer to locate the coherence conditions in the social sphere (e.g. social interests).
The upshot of all these finitist arguments is that the accepted usage of scientific concepts, as of any other concepts, is *conventional* through and through and not dictated by the structure of objective reality. Hence the finitist or network theory provides powerful philosophical support to the constructivist undertaking in the sociology of science.[38]

Not only does the finitist theory provide a philosophical underpinning for constructivism, it can also be used to criticize some widely held views about what is involved in following norms or rules. In functionalist sociology, for example, the successful internalization of the basic social norms is considered a necessary and sufficient condition for maintaining the normative order of society. Norms are held to define unambiguously the types of action which are in conformity with them. However, what the finitist theory highlights is that in trying to act in accordance with a norm people have continuously to (re)negotiate and (re)establish what the norm implies. There is no 'inherent meaning' of the norm that would lay down in advance how it should be applied in all possible situations (cf. Wittgenstein's comments about rule-following: a rule does not contain a rule for its own application). Barnes formulates the following criticism of the functionalist view:

> "Normative functionalism mistakenly attributes the power people exercise in deciding what is involved in following norms to the norms themselves. It empowers norms, and represents people as acting under pressure from that power".[39]

In fact, according to Barnes, such 'empowerment' is a very widespread tendency in social science generally and is extended to traditions and all of the elements of which traditions consist (ideas, ideology, norms, laws, even 'technology'), which are thus represented as inherently constraining. With his colleagues Bloor and Henry, he has therefore formulated the following rule for explanation in constructivist sociology of knowledge:

> "If finitism is accepted, then verbal formulations of values, rules, aphorisms and ideas are all alike debarred as causally potent, as are the inherent persuasiveness of rhetoric, or the alleged powers of ideologies or legitimations".[40]

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[38] A finitist analysis of the history of the concept of syphilis will be given in Chapter III. I have previously used the finitist theory to elucidate the historical patent dispute about aniline red or fuchsine in France during the 1860s, in which the sameness or difference of various commercial dyestuffs was at issue. See H. van den Belt, 'Action at a Distance', in R. Smith and B. Wynne (eds.), *Expert Evidence: Interpreting Science in the Law*, London (Routledge), 1989, pp. 184-209.

[39] B. Barnes, *The Elements of Social Theory*, London (UCL Press), 1995, p. 59. Barnes also criticizes Max Weber's theory of bureaucracy on the way it treats rules as 'empowered' (see p. 201 ff.).

[40] Barnes, Bloor and Henry, op. cit. (note 37), p. 119.

We will later have to address the question whether Fleck's attribution to a thought style of the power to determine the thinking and perception of the members of the corresponding thought collective also amounts to a violation of this rule.

The case of the microbiological laymen collective in Buchenwald

In his first publication on epistemology published after the war, Fleck comes very close to formulating an equivalent of the tenets of symmetry and impartiality which can be considered the defining principles of all modern forms of constructivism. The article reports about a remarkable, though somewhat sinister episode during Fleck's stay in the Buchenwald concentration camp. (In an attempt to spiritually survive the Holocaust, Fleck had maintained as far as possible his research interests in serology and epistemology.[41]) Here, in August 1943, the SS had set up a laboratory for the study and production of typhus vaccine in Block 50 (not far from the notorious Block 46 in which experiments on prisoners were performed). In December 1943 Fleck, who was known as a leading typhus expert, was deported from Auschwitz to this laboratory in Buchenwald. What he found there was a kind of microbiological research-team or (to use his terminology) 'thought collective' consisting of 8 persons who were laymen in microbiology under the direction of a German SS physician who also lacked proper qualifications.[42] In effect, without knowing it, the SS had set up a unique and interesting experiment in the sociology of knowledge. The collective had at its disposal laboratory facilities and instruments, test animals, handbooks and other professional literature, in sum all the necessary wherewithal to do research. It was charged with isolating from the lungs of artificially infected mice and rabbits the causative agent of typhus, *Rickettsia Prowazeki*, in order to prepare typhus vaccine. On Fleck's arrival in Buchenwald it seemed as if the collective had already realized its task. Large quantities of typhus vaccine were apparently produced and sent to the Eastern front for use by the SS troops. It soon transpired to Fleck, however, that whatever it was that the collective isolated from the lungs of infected test animals, it was definitely not the agent of typhus, *Rickettsia Prowazeki*! Thereupon it was decided to keep this discovery secret from the Germans. Until the end of the war hundreds of liters of ineffective vaccine continued to find their way to the Eastern front. In addition, small quantities of effective vaccine were produced for use by prisoners who were exposed

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41 T. Schnelle, 'Microbiology and Philosophy of Science, Lwów and the German Holocaust: Stations of a Life - Ludwik Fleck 1896-1961' , in Cohen and Schnelle, op. cit. (note 1), pp. 3-36, on p. 26.

42 "The collective consisted of: (1) a young Polish physician, without any specialist preparation; he was the head of the collective [NB: Fleck is not reckoning the German SS physician among the members of the collective - HvdB]; (2) a doctor of laws and philosophy - an eminent Austrian political figure; (3) a worker from a factory making rubber articles - a German Communist activist; (4) a young Czech physician, with rudiments of bacteriological preparation; (5) a practising Czech veterinarian, without bacteriological preparation; (6) a Dutch student of biology, with his assistant, a student of the 3rd or 4th year of medical studies; and (7) a Vienna confectioner". See L. Fleck, 'Problems of the Science of Science [1946]', in Cohen and Schnelle, op. cit. (note 1), pp. 113-27, on p. 118.

to special risks and to prepare samples that had to be sent regularly to external institutes for verification purposes.

In his 1946 article Fleck attempted to draw epistemological lessons from these experiences. It was not an occasional and isolated 'mistake' that had been committed by the microbiological laymen collective, he maintained, but it was rather a "closed, harmonious system of errors" that had been gradually built up during a process of social stimulation and consolidation.[43] In the end members of the collective 'found' in their microscopic preparations, which they made by scrupulously following the handbook instructions, all the stages of the developmental cycle of *Rickettsias* in the sequence required by the handbooks: "from the dyestuff precipitates, fat globules, various bacteria and cellular remnants they managed to arrange the entire developmental cycle".[44] The spell of this vicious circle or "harmony of illusions" could be broken only by an expert outsider, Fleck himself in this particular case.[45]

The main conclusions Fleck derived from the activities of the microbiological laymen collective are the following:

> "Most important in our story is the fact that [...] *the social mechanism of the origination of an error is the same as that of the origination of true knowledge* [...]. Both in faulty and in true science it is the same collective forces that play the role of a motor, while the individual is the representative of certain social functions rather than a conscious source of action. In both false and true knowledge a view does not arise by logical calculation of some elements, but by way of a complex process of stylization. There exists no

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43 Ibid., p. 123. The notion of 'social consolidation' is derived from Wilhelm Jerusalem (see also note 20).

44 Ibid., p. 119.

45 In addition to the epistemological conclusions which Fleck explicitly draws from this experience, the case of the microbiological laymen collective in Buchenwald also illustrates the insufficiency of written instructions and hence the relevance of unarticulated and partly unarticulable skills ('tacit knowledge') for transmitting knowledge from one place to another. The theme of 'tacit knowledge' is an important subject in contemporary sociology of scientific knowledge, especially in the work of Harry Collins. See his book *Changing Order: Replication and Induction in Scientific Practice*, Beverly Hills and London (Sage), 1985, pp. 56-57 and 70-71. A very interesting application of the idea is developed in D. MacKenzie and G. Spinardi, 'Tacit Knowledge and the Uninvention of Nuclear Weapons', in D. MacKenzie, *Knowing Machines: Essays on Technical Change*, Cambridge MA (MIT Press), 1996, pp. 215-60. Fleck himself stresses the importance of *Erfahrenheit* or "being experienced" as a condition for the ability to perceive and recognize the characteristic *Gestalts* of a certain discipline (e.g. diphtheria bacilli in microscopic preparations): "One has to acquire a certain experience, a certain knack, which cannot be replaced with verbal formulae" (Fleck, 1935, op. cit. [note 1], p. 60). Written research papers are inherently incomplete; they do not explicate the underlying skills that are required to conduct experiments: "It is as if the words of a song were published without the tune" (96/126). Fleck's digressions on the role of *Erfahrenheit* are quite comparable to modern discussions of 'tacit knowledge'.

> observation that would not be forestalled by a directing and limiting readiness of thought [emphasis added]".[46]

Fleck's statements here can be compared to the principles of impartiality and symmetry which are constitutive of modern constructivism. In 1976 Bloor formulated four defining tenets or principles of the *Strong Programme* in the sociology of science. These four principles are:

1. *causality*: the programme is concerned with the conditions which bring about belief or states of knowledge;
2. *impartiality*: the programme is impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation;
3. *symmetry*: the programme would be symmetrical in its style of explanation. The same types of causes would explain, say, true and false beliefs;
4. *reflexivity*: the programme would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself.[47]

It is rather common to speak of the principle of symmetry in a new sense, in which the tenets of impartiality and symmetry distinguished by Bloor are fused together. I will also conform to this current usage. The principle of symmetry (in this new sense) is in fact a strategic counter move to the rationalist view of science to which philosophers like Lakatos, Laudan and Newton-Smith subscribe. It is the conviction of these philosophers that only 'pathological' episodes in the history of science (e.g. the Lysenko affair in Soviet Russia or the Piltdown forgery in Britain) are amenable to analysis by the sociology of knowledge. As long as scientists follow the rules of scientific rationality, there is nothing to be explained by the sociologist; they will only become an object for sociological investigation if they deviate from these rules (cf. Newton-Smith: "sociology is for deviants"). With the principle of symmetry, Bloor actually claims the whole range of scientific activity, both 'good' and 'bad' science, as a potential object for the sociology of knowledge. Although most other varieties of constructivism do not subscribe to the other two principles formulated by Bloor, the principle of symmetry is beyond dispute within constructivist circles.

By stating in the passage just quoted that "in faulty and in true science" the same social mechanisms are at play, Fleck anticipated the formulation of the defining principle of modern constructivism.

In the passage quoted we also find the ingredients that are characteristic for Fleck's comparative epistemology: the individual as a representative of social functions rather than as a conscious source of action, Fleck's rejection of 'logical calculation' and his emphasis

[46] Ibid., p. 123.

[47] D. Bloor, op. cit. (note 23), p. 7.

on 'stylization', and his notion of a 'readiness' for selective thinking and observation as typical of a thought style. Let us therefore have a closer look now on Fleck's 'doctrine of thought styles and thought collectives'.

3. Fleck's doctrine of thought styles and thought collectives

I will first examine Fleck's theory with regard to two big problems, the relationship between the individual and the collective and the question of realism. These two problems are not only relevant when considering Fleck's theory, but can also be raised vis-à-vis modern varieties of constructivism. Barnes, Bloor and Henry note that sociologists of science generally, in describing the actions of scientists, have been forced to consider what institutions, social relations, groups and collectives are: "In particular, they have had to confront the perennial problem of the relationship of the individual and individual action to the collective, whether as a group, or a social structure, or an institution, or a culture".[48] The second problem, the question of realism, is of course hardly less of a "perennial problem". Like the first problem it has a wide relevance for different fields of inquiry, but it has gained special prominence and urgency in constructivist studies of science. With regard to both problems, Fleck's views will be compared to the positions taken by modern varieties of constructivism. Subsequently, I will discuss two different issues which are more specific for Fleck's approach: the relationship between 'active linkages' and 'passive linkages' and the idea of a thought style and of a stylized thought constraint. Even in these more specific issues, however, the two "perennial problems" are also implicated.

The collective and the individual

In contradistinction to traditional epistemology, which construes cognition as a *dual* relationship between a knowing subject and the object to be known, Fleck's comparative epistemology considers this process as involving a *threefold* relationship; it introduces a third factor into the equation: the thought collective (and together with it the thought style, because for Fleck these are correlative concepts): "every cognition is a process between an individual, his thought-style which results from affiliation to a social group [a thought collective - HvdB], and the object".[49]

Fleck defines a thought collective as "a community of persons mutually exchanging ideas or maintaining intellectual interaction" (39/54). As it stands, there is nothing objectionable to this definition. It could be an element in an *interactionist* view according to which knowledge emerges from social interaction and is sustained and reinforced through continued

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[48] Barnes, Bloor and Henry, op. cit. (note 37), p. 113.

[49] Fleck, 1947, op. cit. (note 3), p. 148.

interaction.[50] What is however problematic in Fleck's theory, is that he does not consistently stick to an interactionist view but often switches over to what may be termed a *collectivist* view, i.e. when he attributes primacy and an independent existence to the thought collective as a whole over and above its interacting members.[51] Hence his view of individuals, not as "a conscious source of action", but as the passive exponents of the collectives to which they belong, or, to use the terminology of ethnomethodology, as the 'judgemental dopes' of the thought style:

> "Although the thought collective consists of individuals, it is not simply the aggregate sum of them. The individual within the collective is never, or hardly ever, conscious of the prevailing thought style, which almost always exerts an absolutely compulsive force upon his thinking and with which it is not possible to be at variance" (41/56-57).

This passage seems to echo Fleck's own brief paraphrase of Durkheim's view presented earlier: "Durkheim speaks expressly of the force exerted on the individual by social structures [..]. He also mentions the superindividual and objective character of ideas belonging to the collective [collective representations - HvdB]" (46/62).[52] Indeed, the oscillation between interactionist and collectivist views that we find in Fleck is also characteristic of Durkheim.[53]

With his 'collectivism' Fleck moves in a direction contrary to that of modern constructivism. As Barnes, Bloor and Henry note: "The trend in the sociology of science since Merton has been to emphasize more and more strongly the standing of individuals as active agents in order to make sense of their actions or explain their provenance. This is consistent with

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50 Interactionism is one of three basic forms of social theory distinguished by Barry Barnes, the other two being individualism and functionalism. In individualism (e.g. rational choice theory) human beings are presumed to be independent, rational/calculative, goal-oriented and self-regarding individuals. In interactionism, by contrast, human beings are not seen as autonomous, pre-constituted individuals who combine stategically for their own expedient reasons, but rather as individuals susceptible to each other in interaction. In functionalism entire social systems are seen as having their own functional needs and requirements, largely independently of the individuals of which they are composed. See Barnes, op. cit. (note 39).

51 In terms of Barnes' three basic forms of social theory (see previous note), what I have described as 'collectivism' would come closest to 'functionalism'. I have not used the latter designation to characterize Fleck's position because we do not find in his writings the systems vocabulary which is characteristic of the usual forms of functionalism.

52 Compare: "[Certain] types of conduct or thought are not only external to the individual but are, moreover, endowed with coercive power, by virtue of which they impose themselves upon him independent of his individual will"; E. Durkheim, *The Rules of Sociological Method*, New York (Free Press), 1964 [1895], p. 2.

53 Barnes (op. cit. [note 39], p. 95) depicts Durkheim as seminal to interactionist social theory, but this downplays the collectivistic strand and the 'empowerment' of 'collective representations' also present in his work.

an analogous shift in the social sciences generally".[54] The general trend takes two forms in the sociology of science. One way to present the individual as an active agent is to conceive of him or her as a participant in a 'way of life'. Participants share a culture or tradition. Cultural elements, however, are considered to be facilitating *resources* for, rather than *constraints* upon, individual actions. This approach is best exemplified in the work of Harry Collins. It could be termed 'interactionist' in Barnes' sense and retains a strong and 'thick' notion of 'the social'. In the second approach 'the social' (in its customary sense) is substantially thinned out by reverting to an 'economic' (individualist) conception of the individual. Barnes, Bloor and Henry see this line exemplified in Bruno Latour's actor-network theory. In agreement with the Hobbesian tenor of this theory no social bond whatsoever is presumed; actors are constantly trying to 'enroll' others or are being 'enrolled' by them. The 'associations' or 'links' that are forged last only as long as expediency permits. Actually, as a solution to this problem (?), the social is partly shifted from the realm of human actors and their relationships on to the sphere of material artefacts.[55] In other forms of radical constructivism (e.g. Pickering's science-as-practice approach or Rouse's practical hermeneutics) the same thinning out or de-centring of the social in its customary sense is taking place. In both moderate and radical constructivist approaches, the emergence of cognitive and social order becomes (more) difficult to explain as social 'constraints' may no longer be invoked.

In Fleck's theory the compulsive force of the thought style over the individual members of the thought collective is somewhat mitigated to the extent that individuals normally belong to different (scientific and non-scientific) thought collectives. Such common memberships facilitate the inter-collective communication of ideas which is seen as an important source of innovation. Within a thought collective, Fleck distinguishes between the so-called 'esoteric circle' and one or more so-called 'exoteric circles'. The 'esoteric circle' consists of 'initiates' who are directly involved in the creation of new knowledge; the 'exoteric circles' have only an indirect relation to this knowledge through trusting the initiates; the latter, however, are in their turn dependent upon 'public opinion', that is, upon the opinion of the exoteric circle(s) (105/139). Only the confidence of the exoteric circle(s) confers solidity and certainty to the knowledge produced within the esoteric circle.[56] Fleck's notion of 'esoteric circle'

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54 Barnes, Bloor and Henry, op. cit. (note 37), p. 114.

55 B. Latour, 'Une sociologie sans object? Note théorique sur l'interobjectivité', *Sociologie du travail* 36 (1994): 587-608. Latour reverses Durkheim's well-known formula: "[..] il faut traiter les choses comme des faits sociaux" (p. 606). Karin Knorr-Cetina likewise introduces the notion of 'object-centered sociality'; see her 'Epistemics in Society: On the Nesting of Knowledge Structures into Social Structures', in W. Heyman, H. Hetsen, and J. Frouws (eds.), *Rural Reconstruction in a Market Economy*, Mansholt Studies 5, Wageningen Agricultural University, 1996, pp. 55-73.

56 It is remarkable that Fleck analyzes the intellectual interaction between the esoteric circle of a collective with its exoteric circles (or the exoteric circles of other collectives?), but ignores the possibility of intense intellectual interaction taking place between the esoteric circles of two different collectives. One might imagine that this possibility becomes actual whenever a medical labora-

resembles the concept of the 'core-set' (the relatively small group of scientists who are most directly involved in negotiating the outcome of a controversy) as used by Harry Collins.[57] Collins also notes that the certainty of knowledge increases dramatically with distance (in both space and time) from the 'core-set'. He formulates a special proposition to describe this effect:

> "*Proposition Eleven*: 'Distance lends enchantment': the more distant in social space or time is the locus of creation of knowledge the more certain it is".[58]

This may be compared to the following pronouncement of Fleck:

> "The greater the distance in time or space from the esoteric circle, the longer a thought has been conveyed *within the same thought collective*, the more certain it appears" (106/140).

Likewise:

> "Social distance transforms the author from a creator to a discoverer. The developing scientific fact changes from mental composition to an object. It becomes impersonal, self-contained, it becomes a thing".[59]

The philosopher Stephen Toulmin has suggested that Fleck gave so much emphasis to the collective aspects of science because, in his time, it was desirable to counteract the excessively individualistic view of scientific thinking that was then current.[60] At any rate, Toulmin notes that Fleck's writings also contain, in addition to his 'collectivism', a more adequate and promising strand of thought which he terms 'populational'.[61] This alternative line is found in Fleck's discussion of the differences between 'journal science' and 'vademecum science'. Scientists present their ideas in scientific journals as 'personal and provisional' contributions,

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tory has to develop diagnostic methods for use in a clinical context. Fleck's own example of the development of the Wassermann reaction would be a case in point, as it involved a close cooperation between serologists and clinical venerologists. He describes this development, however, as the exclusive work of the so-called "serologists' collective" and neglects the clinical connection altogether. See chapter V.

57 H. Collins, *Changing Order: Replication and Induction in Scientific Practice*, London (Sage), 1985, pp. 142-45.

58 Ibid., p. 145.

59 Fleck, 1936, op. cit. (note 1), p. 108.

60 S. Toulmin, 'Ludwik Fleck and the Historical Interpretation of Science', in Cohen and Schnelle, op. cit. (note 1), pp. 267-85, esp. on p. 275.

61 In terms of Barnes' distinctions this would be a variety of 'interactionism'.

which will be incorporated into the common stock of knowledge ('vademecum science'), if at all, only after intense scrutiny and modification. In journal articles, Fleck observes, we find typical turns of phrases such as 'I have *tried* to prove ...', 'It *appears* possible that ...', whereas it is only in impersonal vademecum science that we find expressions such as 'This or that exists' or 'It has been firmly established that ...' (118/157). By following scientific findings on their way from 'journal science' to (possibly) their final incorporation into 'vademecum science', we may conceive of science as an interplay between (individual) *variation* and (collective) *selection*. Such ideas have been taken up and elaborated by evolutionary theories of science.[62] They clearly reconcile the 'individualistic' and the 'collectivistic' aspects of science.

In my opinion, Fleck's analysis of intellectual interaction (*Denkverkehr*) within a thought collective, with ideas passing from one person to another and thereby undergoing subtle changes and transformations (42/58), can also be usefully interpreted in 'populational' terms. Gerard de Vries has aptly characterized this view as an 'epidemiology of intellectual contact'.[63] Fleck, however, seems too eager to sacrifice such insights for a Durkheimian reification and hypostatization of the thought collective and the thought style as independent social and cognitive structures exerting a powerful influence over individuals (46/62). Fleck's image of a circulation of ideas within the thought collective is strongly reminiscent of a very similar image of Latour and Woolgar, except that the latter do not speak of the circulation of *ideas* but of *statements* (because of their self-imposed moratorium on cognitive terms) and are less inclined to give in to Durkheimian temptations.[64] Actually, Fleck's discussion of 'journal science' and 'vademecum science' can be considered as an anticipation of Latour and Woolgar's well-known view of the construction of scientific facts as effected through alteration of the *modalities* attached to statements (e.g., moving a modality from 'it is probable that A is B' to 'it is firmly established that A is B'). Fleck's assertion that "even the simple communication of an item of knowledge can by no means be compared with the translocation of a rigid body in Euclidean space" (111/145) would fit in well with Latour's criticism of the 'diffusion model' and his advocacy of a 'translation model'.[65] Likewise, Fleck's insight that every understanding between persons involves some misunderstanding[66] would be

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62 L. Boon, 'Variation and Selection: Scientific Progress without Rationality', in W. Callebaut and R. Pinxten (eds.), *Evolutionary Epistemology*, Dordrecht (Reidel), 1987, pp. 159-77. See also D. Hull, *Science as a Process*, Chicago (University of Chicago Press), 1988.

63 G. de Vries, 'De Besmettelijkheid van Intellectueel Contact', *Kennis en Methode* 5 (1981): 156-64, on p. 162.

64 B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, Beverly Hills (Sage), 1979.

65 B. Latour, *Science in Action*, Milton Keynes (Open University Press), 1987, pp. 132-36.

66 Fleck, 1936, op. cit. (note 1), p. 85.

endorsed by Latour who emphasizes that every *'traduction'* is a *'trahison'* (following the Italian saying *'traduttore traditore'*).[67]

Noting that in intellectual interaction a thought passes from one individual to another, each time a little tranformed, Fleck cannot resist to ask: "*Whose* thought is it that continues to circulate?" [emphasis added], and he feels obliged to answer: "It is one that obviously belongs not to any single individual but to the collective" (42/58). It would seem that Fleck unnecessarily switches over here from an interactionist to a collectivist approach. The strange thing is that Fleck is fully aware of the possible objection to this particular move but remains completely unimpressed by it, as appears from the following passage:

> "We could agree with anybody who calls the thought collective fictitious and the personification of a common result produced by interaction. But what is any personality if not the personification of many different momentary personalities [..]?" (44/60).

Why should a thought as "a common result produced by interaction" be assigned to the 'collective' as its presumptive 'author'? Can a collective act at all in the capacity of an 'author'? Members of a scientific thought collective show by their behaviour that they see things differently from Fleck: as Latour and Woolgar also observe, a new idea is committed to the general circulation only after having been provided, so to speak, with a personal label. Whether both, the idea and the property claim, will survive the vicissitudes of social interaction is of course another matter. The disputes over *intellectual property* which often result are the stock-in-trade of Robert Merton's sociology of science. Such a dispute was the struggle over the ownership of the Wassermann reaction, which will be discussed in chapter VI. It will present a useful opportunity to confront Fleck's analysis with a (constructivistically amended) Mertonian approach.

The question of realism

Constructivism is often opposed to realism. Such a simple opposition is rather misleading, however, because there are many different forms of constructivism as well as many varieties of realism. Things are even more complicated because there is also no generally accepted classification of the existing forms of constructivism and of realism (there are forms of 'would-be' realism - e.g. Hilary Putnam's 'internal realism' - that are not considered to be proper forms of realism by the adherents of other varieties!). Instead of a general opposition between constructivism and realism, we thus have a situation in which certain tenets of particular forms of constructivism conflict with, or are seen to conflict with, certain cheri-

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67 "La traduction est toujours par définition un mal-entendu [..]", in B. Latour, *Les Microbes: Guerre et Paix suivi de Irréductions*, Paris (A.M. Métailié), 1984, p. 73; or "To translate is to betray: ambiguity is part of translation" and "In the translation model there is *no transportation without transformation* [..]", both in B. Latour, *Aramis or the Love of Technology*, Cambridge MA (Harvard University Press), 1996, p. 48 and p. 119.

shed principles of particular forms of realism. It will be clear that this situation does not lend itself easily to adequate and comprehensive description.

A provisional but feasible way to address the issues may be to examine how the question of realism has been raised and dealt with in various forms of constructivism. For the sake of simplicity, a division can be made between 'moderate' and 'radical' forms of constructivism.[68] Among 'moderate' constructivists the debate is about what status, if any, objective reality or the natural world should be accorded in the sociological explanation of knowledge and belief. Harry Collins, who is the foremost representative of the so-called Empirical Programme of Relativism, defends the position that an *agnostic* stance with regard to objective reality is the most appropriate one to take when doing the sociology of scientific knowledge, or as he laconically states: "the natural world has a small or non-existent role in the construction of scientific knowledge" (this statement has meanwhile acquired some notoriety).[69] To take the principle of symmetry to its conclusion, Collins deems it necessary to treat all description-type language at the outset as though it did not describe anything real. Thus the manner in which generalizations come to be accepted must be understood in a similar way, whether they are about the colour of emeralds or about the number of angels dancing on the head of a pin.[70] Collins calls this a 'meta-methodological presupposition'. His colleagues from the Strong Programme, Barnes and Bloor, strongly disagree. They consider themselves to be ontological realists. The sociologist, they hold, should be willing to acknowledge the causal relevance of the physical environment when studying the formation of knowledge and beliefs. Whereas Collins claims that the prevailing sense of natural order and regularity derives entirely from culture and not at all from nature, Barnes and Bloor present a view in which cognition is seen as resting on a symbiosis of nature and culture.[71]

Among 'radical' forms of constructivism, the question of realism is posed in a quite different way. Radical constructivists often claim to be able to circumvent or bypass the epistemological debate on realism and anti-realism. This claim is however highly problematic. The reason is that they take the construction metaphor much further than their moderate

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68 In distinguishing 'moderate' from 'radical' constructivism, I follow Rob Hagendijk. See his *Wetenschap, Constructivisme en Cultuur*, Amsterdam (Thesis), 1996. In my opinion, however, Hagendijk does not satisfactorily elaborate the differences between both varieties of constructivism with regard to the question of realism, because he appears to take at face value the radical constructivist claim to have circumvented the epistemological debate between realism and anti-realism (or the debate between realism and 'relativism' as it is called rather misleadingly).

69 H.M. Collins, 'Stages in the Empirical Programme of Relativism', *Social Studies of Science* 11 (1981): 3-10, p. 3.

70 Collins, op. cit. (note 45), p. 174.

71 A recent exposition of the Strong Programme view on the role of natural reality in the formation of belief and a criticism of the position of Harry Collins can be found in Barnes, Bloor and Henry, op. cit. (note 37), esp. p. 73 ff.

colleagues. Whereas moderate constructivists almost exclusively speak about the construction of *knowledge* (sometimes also of 'facts', but these are then put on a par with knowledge), radical constructivists have no qualms to extend the metaphor and to speak also of the construction or fabrication of the objects populating our world. Thus, science is seen by Karin Knorr-Cetina as "secreting" an unending stream of "entities and relations that make up the world"[72], whereas Latour's *techno-science* is hardly less productive in its contribution to the "proliferation" of new "quasi-objects" or "hybrids".[73] Among the objects and entities (thought to be) so created are not just, say, plastics and transgenic plants, but also microbes, atoms, genes, quarks, pulsars, black holes, supernova etecetera. It is this (in my eyes rather implausible) view which brings these radical varieties of constructivism in direct conflict with most if not all forms of realism.[74]

How is the question of realism posed in Fleck's thought? Is he a moderate or rather a radical constructivist on this matter? Although his primary concern with epistemology and knowledge suggests an affinity with the views of moderate constructivism, we also find several indications in his work which point in the direction of radical constructivism.

While there are many different forms of realism, it might still be helpful to have a provisional working definition of realism as a standard of comparison for characterizing Fleck's views. Let us therefore follow David Papineau and take realism, for any body of putative knowledge, to involve "the conjunction of two theses: (1) *an independence thesis*: our judgements answer for their truth to a world which exists independently of our awareness of it; (2) *a knowledge thesis*: by and large, we can know which of these judgements are true".[75] There is no doubt that Fleck is opposed to realism so defined.

Already in his 1929 essay 'On the Crisis of "Reality"', he formulated a view which would later acquire so much fame and notoriety in the wake of debates on Thomas Kuhn's work that it was dubbed '*many-worldism*' by its realist opponents. Fleck stated that "[m]embers of different scientific communities live in their own scientific and also professional reality".[76]

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[72] K. Knorr-Cetina, 'The Ethnographic Study of Scientific Work: Towards a Constructivist Interpretation of Science', in K.D. Knorr-Cetina and M. Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*, London (Sage), 1983, pp. 115-40, on p. 135.

[73] B. Latour, *We Have Never Been Modern*, Cambridge MA (Harvard University Press), 1993.

[74] R. Nola, 'There are More Things in Heaven and Earth, Horatio, Than are Dreamt of in Your Philosophy: A Dialogue on Realism and Constructivism', *Studies in the History and Philosophy of Science* 25 (1995): 689-727.

[75] D. Papineau, 'Introduction', in D. Papineau (ed.), *The Philosophy of Science*, Oxford (Oxford University Press), 1996, p. 2.

[76] Fleck, op. cit. (note 10), p. 49.

Kuhn would later defend the, for realists outrageous and bizarre, view that when paradigms change, the world itself changes with them; thus he stated that "after Copernicus, astronomers lived in a different world", and likewise "after discovering oxygen Lavoisier worked in a different world".[77] The 'many-worldism' view was therefore not Kuhn's original invention, but neither was it Fleck's; before him, the thesis of the 'plurality of realities' had already been propounded by Leon Chwistek, a representative of the famous 'Philosophy of Lwów'.[78]

Fleck would have denied both the 'independence thesis' and the 'knowledge thesis', which Papineau takes to be, in conjunction, constitutive of realism. After having argued that the history of science cannot be construed as an approach to the ideal, 'absolute' reality, "not even asymptotically" (thus implicitly rejecting approximation versions of the 'knowledge thesis' such as defended by Richard Boyd[79]), Fleck attacks the notion of an 'independent' reality head-on:

> "Of what ought the absolute reality to be independent? If one wished it to be independent of man, one ought to consider that in this event it would also be of no use to man".[80]

The 'many-worldism' view and the rejection of the idea that science gradually approximates the true, objective picture of the world are combined in the reply that Fleck puts in the mouth of Sympatius (figuring in a constructed dialogue) to answer the challenge of Simplicius: "You will not gainsay that today's science is closer to the objective picture of the world than the science of 100 years ago?". Sympatius (who is obviously Fleck's mouthpiece) replies:

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77 T.S. Kuhn, *The Structure of Scientific Revolutions*, Chicago (University of Chicago Press), 1970 [1962], p. 117 and p. 118. Kuhn remained loyal to this view. In a more recent contribution, dating from 1989, he stated: "The heavens of the Greeks were irreducibly different from ours". See T.S. Kuhn, 'The Natural and the Human Sciences', in D.R. Hiley et al. (eds.), *The Interpretive Turn*, Ithaca and London (Cornell University Press), 1991, pp. 17-24, on p. 21.

78 Schnelle, op. cit. (note 6), pp. 254-56. It must be noted that there is also a *biological* version of 'many-worldism', as transpires from the following passage derived from Jakob von Uexküll and quoted by Fleck: "[..] the biologist claims that there are as many worlds as there are subjects, that all worlds are worlds of appearance that can be understood only in connection with the specific subject" (179 note 6/138 note 6). Uexküll's thought has been a major source of inspiration for Konrad Lorenz' ethology.

79 R. Boyd, 'On the Current Status of the Issue of Realism', *Erkenntnis* 19 (1983): 45-90.

80 Fleck, op. cit. (note 10), p. 55.

> "I do not think that today's science is closer to the objective picture of the world than the science of 100 years ago. On the other hand, I am sure that today's science is closer to our world of today, while the science of 100 years ago was closer to what was then the world of the creators of science".[81]

One might wonder what the presumed mechanism is that brings today's science closer to "our world of today". Does science somehow *create* the world, or at least 'our' world? If Fleck attributes 'world-making' powers to science, then on that account he would indeed move very close to the radical varieties of contemporary constructivism. In the following passage he seems to go all the way in that direction:

> "[..] the theory of thought-styles throws a specific light on the relation between 'reality' and 'cognition': the chasm between 'nature' and 'culture' fades away because the cognitive activity (note: the collective one, which creates a separate thought-style) is not a unilateral action as, e.g., the figurative rendering of a certain object, but consists in a bilateral interaction: The thought-style *creates* reality, not in a different way from other products of culture and, at the same time, itself undergoes certain harmonious changes".[82]

(The fading away of the chasm between 'nature' and 'culture' would of course nicely correspond with the radical constructivist rejection of any a priori distinction between the two.) Fleck charges the realist who strives to know absolute reality with a strange misunderstanding: "Is [such striving] not the same as if one wanted to open up a pristine jungle, without changing its condition?".[83] Gerard de Vries presents a further elaboration of this simile which highlights the 'world-making' quality of modern science:

> "In the eyes of the relativist [= radical constructivist - HvdB], realists are as deceived as tourists are, who travelled long to enjoy what they take to be 'pure nature', and who fail to notice that the sand-beaches are cleaned and raked every night, that the exuberant flora of their pet-island is artificially irrigated and that the 'original dances' of the natives are organized by the national airline. What tourists perceive as 'pure nature' is made possible by their pecuniary investments and the efforts of local organizers; analogously, what scientists experience as 'the natural world' is made possible by their conceptual investments and the organization of manpower and laboratory equipment".[84]

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81 Fleck, 1946, op. cit. (note 3), pp. 116-17.

82 Fleck, 1936, op. cit. (note), p. 112.

83 Fleck, op cit. (note 10), p. 55. Fleck's comparison is well-chosen. The American environmentalist and photographer Ansel Adams, who wanted to keep the wilderness of Yosemite pure in order to protect its 'spiritual potential', realized the tragic paradox involved: "unfortunately, in order to keep it pure we have to occupy it". Quoted in S. Schama, *Landscape & Memory*, London (HarperCollins), 1995, p. 9.

84 G. de Vries, 'Explaining "Truth" in a Relativist Way', in T.A.F. Kuipers (ed.), *What is Closer-to-the-Truth?*, Amsterdam (Rodopi), 1987, pp. 217-28, on p. 219.

Active and passive linkages

We have seen that Fleck is no realist in Papineau's sense. There is no human-independent world which as such is accessible to scientific investigation. As soon as the jungle is opened up, it is no longer pristine. Still, one might concede that any research requires conceptual investments and the organization of manpower and laboratory equipment as *active* contributions on the part of scientists, and yet deny that this entails that reality has no share in determining the outcome of research. In other words, one could attempt to strike a balance between what the world contributes to our knowledge and what scientists do, or between discovery and invention. Such a position could perhaps be considered a constructivist variety of realism.[85] At first sight it appears that this is just what Fleck is aiming at when he introduces the distinction between 'active linkages' and 'passive linkages' (or 'couplings') which together are held to compose all knowledge. Fleck writes: "Cognition [...] means, primarily, to ascertain those results which must follow, given certain preconditions. The preconditions correspond to active linkages and constitute that portion of cognition belonging to the collective. The constrained results correspond to passive linkages and constitute that which is experienced as objective reality. The act of ascertaining is the contribution of the individual" (40/85). There are however a lot of problems connected with this formulation. First, the chosen terminology is somewhat unfortunate. 'Active linkages' are not presuppositions that are open to free rational choice - as conventionalism, in Fleck's opinion, maintains.[86] They represent the contribution of the collective to cognition and can be explained historically from the development of science and culture. Fleck assigns the act of ascertaining the 'passive linkages' to the individual, but this is a rather empty concession, because this ascertaining should precisely amount to the elimination of the individual 'arbitrariness' or 'caprice' of thought. Fleck attempts to construe the genesis of a 'fact', or the establishment of 'passive linkages', as a process of progressive restriction on the 'freedom of thought' of the investigators: in the end the collective has to feel firm ground which resists its arbitrary will (Fleck speaks of a *Widerstandsaviso* or 'signal of resistance'): "This is how a *fact* arises. *At first*

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85 In the terminology of Frank Farrell, this position would be characterized as *parochial realism*: "Let us say that someone might be a *parochial realist* regarding a certain feature. The realism of the position consists in the claim that regarding the feature in question we are dealing with a tracking of some real aspect of the universe. Yet the position is a parochial one in making the further claim that a certain sensibility not shared by all thinkers or experiencers, or a particular way of engaging the world, is a condition for letting that feature emerge as determinate". See F.B. Farrell, *Subjectivity, Realism and Postmodernism*, Cambridge (Cambridge University Press), 1996 [1994], p. 160. Farrell mentions David Wiggins as a representative of parochial realism.

86 Jerzy Giedymin, who is a specialist on conventionalism, points out that Fleck is liable to a widely shared misunderstanding here. Like many others, Fleck mistakes "the claim that our fundamental concepts are not uniquely imposed upon us by experience or by *a priori* intuitions and in this sense are conventional (and arbitrary, free) with another claim - *never made by conventionalists* [emphasis added] - that conventions depend only on the free choice or on the whims of the individual scientist and are unaffected by experiential, socio-historical or biological circumstances" (J. Giedymin, op. cit [note 6], p. 186).

there is a signal of resistance in the chaotic initial thinking, then a definite thought constraint, and finally a form to be directly perceived. A fact always occurs in the context of the history of thought and is always the result of a definite thought style" (95/124). Fleck's reconstruction of the establishment of 'passive linkages' leaves many questions unanswered. These linkages, he says, constitute "what is experienced as objective reality", but what role exactly, if any, does he allow 'objective reality' to play? Fleck's thought seems to be oscillating here. At one place he suggests that "objective reality can be resolved into historical sequences of ideas belonging to the collective"; from the viewpoint of comparative epistemology this factor could thus be eliminated (40-41/56). Cognition would then no longer be seen as a process involving subject, thought collective, and object. Elsewhere, however, Fleck asserts that a scientific fact cannot be substantively reconstructed in toto from a cultural-historical and sociological investigation (83/110). But then, again, he suggests that the residual, "something inevitable, steadfast and inexplicable by historical development", can in turn be explained by "an intrinsic constraint imposed by thought style" (79-80/105-06). This would appear to leave no role for 'objective reality' after all.

In addition to this unclarity with regard to the role left for 'objective reality', there is another problem with Fleck's conception of 'passive linkages' as 'inevitable' results, given certain presuppositions (the 'active linkages'). This notion of inevitability and intellectual constraint runs counter to the basic tenet of the finitist theory. Barnes, Bloor and Henry oppose the widely shared view which first concedes that conventions can indeed be freely chosen, but then holds that once they have been chosen they will determine our subsequent classificatory activity. As if everything will be fixed after the 'premises' have been decided upon. This view conflicts with the open-ended character of the application of terms (and, consequently, also of conventions).[87] Fleck's conception of 'active' and 'passive' linkages seems to exemplify this way of thinking. He illustrates his distinction with the example of the atomic weights of the elements: "The origin of the number 16 for the atomic weight for oxygen is almost consciously conventional and arbitrary. But if 16 is assumed as the atomic weight for O, oxygen, of necessity the atomic weight of H, hydrogen, will inevitably be 1.008" (83/110). If we look more closely at the actual history of our changing ideas about atoms, molecules, elements, and atomic weights, we may get a clearer picture of what is wrong with Fleck's notion of 'inevitable' results.

In the first half of the 19th century and beyond there was widespread disagreement among chemists concerning the *relative* atomic weights for elements and consequently also concerning chemical formulae.[88] For oxygen and hydrogen, for example, the ratios of 16

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87 Barnes, Bloor and Henry, op. cit. (note 37), p. 55.

88 The following account is mainly based on The Open University Course Team, *The Structure of Chemistry*, Milton Keynes (Open University Press), 1976. See also J.R. Partington, *A Short History of Chemistry*, London (MacMillan), 1957 (3rd edition), pp. 256-58 and J. Hudson, *The History of Chemistry*, Houndmills etc. (MacMillan), 1992, chapter 9.

to 1 and 8 to 1 were both current. There was also a cleavage between the two main departments of chemistry: inorganic chemistry used one set of atomic weights, organic chemistry a different set. An international conference was held at Karlsruhe in 1860 to find a solution for the general state of confusion. At the conference the Italian chemist Stanislao Cannizzaro made a strong case for the use of Avogadro's hypothesis (known already for almost half a century) in the determination of (relative) atomic weights, which required a distinction to be made between 'molecules' and 'atoms'; but he was opposed by a 'big shot' like Kekulé who questioned the admissibility of physical evidence for chemical molecular weights. For oxygen and hydrogen, adoption of Cannizzaro's ideas led to the ratio of 16 : 1. The ratio mentioned by Fleck of 16 : 1.008 reflects the discovery of isotopes at the end of the century, which in turn led to a debate on the meaning of the concept of 'element'. Only after all these controversies had been settled and a system of *relative* atomic weights had become established, did a more narrowly 'conventionalist' debate on which standard to take as a basis (H=1 or O=16) get started. Some chemists argued in favour of taking H=1, mainly for pedagogic reasons (it is easier to teach), whereas others argued for taking O=16, which would be more convenient for experimental research because the atomic weights of many elements could be determined only through combination with oxygen. Around 1900 a commission installed by the German Chemical Society conducted an international survey among professional chemists to decide the issue. Because most of the chemists were in favour of oxygen, O=16 was from then on taken as the standard.[89] Fleck's assertion that, assuming O to be 16, H will "inevitably" be 1.008, glosses over this whole convoluted history. He indicates the victor after the battle-smoke has cleared. With hindsight outcomes will nearly always look 'inevitable'. Such a retrospective procedure does not do justice to the constructivist insight (in particular emphasized by Harry Collins) that research results are always in principle 'interpretively flexible' and 'socially negotiable'. It took indeed a lot of 'negotiation', at the Karlsruhe Conference and beyond, before the currently accepted set of atomic weights was in place.

Fleck's use of 'passive linkages' can be compared to the use of the notion of 'constraint' by some modern critics of constructivism. Such critics take constructivists as holding that scientists just say whatever they wish about nature, to which they then react by saying that of course this is not true and that surely there must also be 'constraints' on construction.[90] Andrew Pickering in his turn has passionately opposed all 'constraint talk' as essentially

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89 The latter part of the story has been told by the German history of science student, Britta Goers, as reported in 'An Atomic Weight Controversy in Nineteenth-Century Germany', *Chemical Heritage*, Volume 14, Nr. 1, Winter 1996-97, p. 29.

90 An example is Y. Gingras and S.S. Schweber, 'Constraints on Construction', *Social Studies of Science* 16 (1986): 372-83.

begging the question.[91] He conceives of scientific practice as an open-ended process of 'modelling' with no pre-determined destination. Every cultural element involved in this 'modelling' - the material procedure, the interpretive model (the understanding of how the apparatus works) and the phenomenal model (the theoretical understanding of the phenomenon under investigation) - is a flexible resource susceptible to endless tinkering and tuning. Still, this does not imply that scientific work should be easy or that scientists can 'construct' anything they like. The point of experimental practice is to achieve coherence between all three elements and that is not something easily attained. In the course of their experimental work, Pickering notes, scientists are repeatedly confronted with 'resistances'. Unlike 'constraints' - which are held to obtain from the outset -, 'resistances' only *emerge* in the real time of practice. They are just as 'emergent' and 'contingent' as the accommodations to which they may give rise. In Pickering's view, it is therefore possible, indeed mandatory, to eschew the notion of 'constraint' without giving up the conviction that science is really hard work.

It will be clear by now that modern constructivism has little use for Fleck's "intrinsic constraint imposed by thought style" (80/106). In his comments on Fleck, Bloor rejects the assumption that a style unfolds itself: "It contains no inherent implications that 'determine our Reason' or guide our understanding".[92] Fleck himself maintains that the thought collective is always looking for the *unique* solution to a given problem that is in conformity with the thought style. As an example he refers to the acceptance by the medical community of Fritz Schaudinn's *Spirochaeta pallida* and its rejection of John Siegel's *Cytorrhyctes luis* as the aetiological agent of syphilis (100/131). I will show in Chapter IV that this particular example does not support Fleck's thesis. Nor do I see any *a priori* reason why there should be "always only one [...] stylized solution" to any given problem (100/131).

The concept of style and its grammar

It is not surprising that modern constructivists have little sympathy for Fleck's view that a style exerts a constraining force over the thought and action of the members of the corresponding collective. After all, they find it unacceptable to treat any element of a cultural tradition as inherently constraining (Barnes et al.) or reject the invocation of constraint altogether (Pickering). In their judgement, Fleck illegitimately 'empowers' thought styles and represents people as acting under pressure from those powers. This violates the tenets of finitism and goes against the precept to analyze scientific practice as occurring in real time.

If the notion of a 'stylized thought constraint' (*stilgemässer Denkzwang*) must be rejected, this will greatly reduce the *explanatory* significance of the concept of thought style. It

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[91] A. Pickering, 'Beyond Constraint: The Temporality of Practice and the Historicity of Knowledge', in J.Z. Buchwald (ed.), *Scientific Practice: Theories and Stories of Doing Physics*, Chicago (University of Chicago Press), 1995, pp. 42-55.

[92] D. Bloor, 'Some Determinants of Cognitive Style in Science', in Cohen and Schnelle, op. cit. (note 1), pp. 387-97, on p. 393.

does not, however, exclude the possibility that the concept refers to an important kind of phenomenon that in its own right is worthy of systematic inquiry and explanation. In other words, a thought style may be an *explanandum* rather than an *explanans*. To decide whether it should be granted this status, it would be very helpful if some instructions could be given as to how thought styles are to be recognized and identified. So what makes up a thought style, what are its defining traits?

Thought styles would hardly merit systematic attention if they were no more than what Fleck's commentators Cohen and Schnelle make them out to be:

> "What the thought-style dictates is accepted as a matter of course by members of the collective. But in fact it is never more than a conglomerate of shared *convictions*, on the basis of which views are developed which conform to them, i.e. 'stylized' views, which when pursued far enough take the shape of an internally consistent 'system of opinion'".[93]

If a thought style were just a "*conglomerate* of shared convictions", it would lack the unity and internal harmony that would make it an interesting phenomenon in the first place. Indeed, it would hardly be justified to call such a conglomerate a style at all.

It must be admitted that the explication given by Cohen and Schnelle captures Fleck's use of the term 'thought style' reasonably well. Already in the 1930s the medical historian and Fleck's compatriot, Tadeusz Bilikiewicz, commented on this use:

> "This term [the term 'style'] is undoubtedly not applied in its usual meaning. Usually we interpret 'style' as a form that is a manifestation of some kind of creativity. In Fleck's formulation the term means a set of sociological conditions on which depend not only the *form* but the *substance* of creation, too".[94]

Although Bilikiewics' comment partly reflects the typical humanist's aversion to a sociological approach, he is certainly on target with his charge that in Fleck's formulation a style also pertains to the *substantive* (and not exclusively to the *formal*) aspects of intellectual creations. Fleck's use of the style concept thus lacks the clarity and coherence that characterizes its dominant use in art history as popularized by Heinrich Wölfflin.[95] This approach analyzes the Classical (Renaissance) and Baroque styles of European art history in a rigorously comparative way by describing works of art belonging to the domains of architecture, painting and sculpture in terms of purely 'formal' oppositions, e.g. 'linear' versus 'pictorial', 'two-dimensional' versus 'spatial', 'closed form' versus 'open form'. Fleck's use of the style concept

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93 R.S. Cohen and T. Schnelle, 'Introduction', in Cohen and Schnelle, op. cit. (note 1), pp. IX-XXXIII, on p. XX.

94 Bilikiewicz, 'Comments on Ludwik Fleck's "Science and Social Context"', in Löwy, op. cit. (note 5), pp. 257-66, on p. 258.

95 H. Wölfflin, *Kunstgeschichtliche Grundbegriffe*, Basel (Benno Schwabe & Co), 1915.

falls far short of the systematic rigour achieved by Wölfflin's approach. His characterization of the so-called 'serological thought style', for example, is no more than a simple enumeration of a number of substantive elements that supposedly make up this particular style (59-64/79-84). These elements include such items as serological specificity, the distinction between cellular and humoral immunity, the 'chemical delusion', the battle metaphor for infectious diseases, the emphasis on systematic observation as the basis of diagnosis and the methodical necessity of applying 'controls'. He does not indicate of what the unity of this style consists, nor does he explicitly state whether there is such a unity at all.

Fleck was not opposed to drawing analogies between science and art. In this respect he shows a striking difference with the sociologist Karl Mannheim, who was the first to introduce, in 1925, the term *Denkstil* into the sociology of knowledge, "in the absence of a better expression".[96] We have already mentioned that Fleck was not acquainted with any of Mannheim's writings. Whereas Mannheim emphatically declares that he by no means intends to 'analogize' between thinking and artistic creation[97], Fleck exhibits no inhibitions on this score: "In science, just as in art and in life, only that which is true to culture is true to nature" (35/48). Fleck shows a great sensitivity to the role of anatomical illustrations and of images in general in scientific cognition.[98]

For Fleck, the main thrust of his use of stylistic categories is against the kind of logical analysis of systems of scientific statements that was practised by the members of the Vienna Circle. The following passage elaborates this contrast and delineates the tasks of 'comparative epistemology' in understanding the emergence and transformation of different styles:

> "In the history of scientific knowledge, no formal relation of logic exists between conceptions and evidence. Evidence conforms to conceptions just as often as conceptions conform to evidence. After all, conceptions are not logical systems [..] They are stylized units which either develop or atrophy just as they are or merge with their proofs into others. [..] It is one of the most important tasks in comparative epistemology to find out how conceptions and hazy ideas pass from one thought style to another, how they emerge as spontaneously generated pre-ideas, and how they are preserved as enduring, rigid structures [*Gebilde*] owing to a kind of harmony of illusions" (27-28/40-41).

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96 K. Mannheim, *Konservatismus: Ein Beitrag zur Soziologie des Wissens*, Frankfurt am Main (Suhrkamp), 1984 [1925], p. 51 and p. 227, note 5.

97 Ibid., p. 227-25, note 5.

98 A comparable interest in the role of pictorial representation is also characteristic of modern constructivism. See M. Lynch and S. Woolgar (eds.), *Representation in Scientific Practice*, Cambridge MA (MIT Press), 1990. See further the special issue of *Biology & Philosophy*, Vol. 6, No. 2 (April 1991), on pictorial representation in biology.

Strangely enough, for all his emphasis that "epistemology [..] is a science of thought-styles"[99] and that epistemology should be conducted on a comparative basis, Fleck never draws what would appear a rather obvious conclusion, viz. that thought styles themselves should be preferably conceived in comparative terms. Precisely the latter point of view has been persuasively defended by the British historian of science, Jonathan Harwood.[100] I think that we can extract from his digressions some minimal requirements for the meaningful use of the term 'style', or the rules governing its grammar, so to speak.

Harwood argues that styles can be said to exist when various cultural sectors embody particular recurring elements *and* when those elements are distinctive, i.e., when they differ from one situation (society) to another. Typographically, the point can be brought home as follows:

Society x	*Society y*
ARCHITECTURE	architecture
SCULPTURE	sculpture
PAINTING	painting

It makes no sense to use the term *style*, Harwood insists, unless recurring elements are identified in *several* cultural sectors. Otherwise one could say just as well, or indeed with more justification, that the architecture of society x and that of society y were simply different. The concept of style is intended to draw attention to the fact that the cultural differences between societies are *patterned*. Thus when styles are involved one may say, for instance, that the architecture of society x differs from that of society y *in the same way* (where the meaning of 'same' has to be further specified) as the sculpture of x differs from the sculpture of y. The existence of such patterned differences enables us to distinguish between 'form' and 'content'. Harwood's considerations may appear rather simple and elementary, but they are very useful for critically scrutinizing the extensive literature on (concepts of) style. It is striking how infrequently current uses of the term 'style' satisfy the seemingly minimal requirements laid down by him.[101] Wölfflin's employment of the style concept in art history is a positive exception to this rule. I have already intimated, however, that Fleck's use of the concept of 'thought style' is also unsatisfactory in this regard.

However serious the shortcomings of Fleck's concept of style may seem to be, on the level of empirical description they are not beyond repair. Harwood's requirements imply that (some of) the elements that Fleck identified as constituting the (presumed) style of the serolo-

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99 Fleck, 'The Problem of Epistemology', op cit. (note 1), p. 98.

100 J. Harwood, *Styles of Scientific Thought: The German Genetics Community 1900-1933*, Chicago and London (The University of Chicago Press), 1993.

101 See, for example, how the concept of 'life-style' is used in the sociology of Anthony Giddens.

gists' collective must also be found in other cultural (or, in this case, disciplinary) sectors, and that such a collection of 'recurring elements' must be matched with at least one contrasting set of elements covering a comparable range of sectors. It so happens that both conditions can in fact be fulfilled. We can recognize similar elements as identified by Fleck for the sector of serology in other sectors of medical science (nosology, aetiology, and therapy) and, happily enough, it proves also possible to find a *contrasting* style spanning the same range of sectors. A detailed demonstration in support of this assertion builds on a more intimate knowledge of and familiarity with the various fields of medical science that will be discussed in the next historical chapters, so this must be postponed till Chapter VIII.

CHAPTER III:

CULTURE, HEREDITY, AND THE CONCEPT OF SYPHILIS

1. Introduction

In the previous chapter we have seen that the medical scientist Ludwik Fleck was led to a constructivist view of scientific knowledge through a critical reflection on concepts of disease that were current in his own discipline. In the first part of his monograph *Genesis and Development of a Scientific Fact* he offered a more detailed analysis along constructivist lines of the historical emergence of the modern concept of syphilis. Like other diseases this particular disease unit was not an objectively given entitity, Fleck argued; he attempted to show that it was rather the product of a long process of *Herausmeisselung* (chiseling out). Fleck considered it utterly naive, however, to assume that this long process of conceptual development had finally reached completion in our own century. His constructivist conception of disease entities was thus diametrically opposed to any essentialism or conceptual realism which views diseases as specific natural kinds possessing essential properties of their own and waiting to be properly delimited by medical science.

In this chapter I will elaborate and extend Fleck's analysis by drawing on more recent contributions to the history of syphilis and also, on occasion, by adducing new historical material. Although I will have to criticize Fleck's account on the level of historical detail, my primary aim here is to consolidate his constructivist approach towards the genesis of disease concepts. In this venture I hope to derive additional inspiration from Mary Hesse's *finitist theory* or *network theory* of meaning and concept application. As we have seen in the last chapter, this theory has been turned into a powerful support for the sociology of scientific knowledge by the two founders of the Strong Programme, Barry Barnes and David Bloor. Let me briefly recapitulate the finitist or network theory here. The theory holds that the established meaning of a term does not determine its future applications. Concepts do not have fixed domains of application or 'reference classes' attached to them; they are open-ended in principle. Whether a new instance falls under a given concept is a matter of judgement, subject to social negotiation. Concepts do not exist in isolation, however. They are mutually connected through 'laws' or 'generalizations', thus forming a conceptual network or fabric. It is the conceptual network as a whole which confronts experience. Any conceptual relationship can be upheld in the face of (purported) 'anomalies', provided that suitable rearrangements are made elsewhere in the system (the Duhem-Quine thesis). The finitist theory therefore leads naturally to the view that the accepted usage of scientific concepts is *conventional* through and through and not dictated by the structure of objective reality. In this chapter I will attempt to show that the case of the concept of syphilis is no exception. As such this attempt is hardly original. David Bloor has already used Fleck's account of the genesis of the modern concept of syphilis to illustrate the finitist theory.[1] Nevertheless, an elaboration of the finitist account by drawing on a larger set of historical data may still be worthwhile.

A further objective of this chapter - beyond arguing for the conventional and constructed character of disease concepts - is to ascertain to what extent social and cultural factors, in particular considerations of morality, have entered into the framing of concepts of syphilis. Fleck observes that, from the Renaissance on, the "fixation upon the emotive venereal character of [the] disease entity" has imprinted a "stigma of fatefulness and sinfulness" upon syphilis (3/5). He explains this fixation on the venereal character of syphilis from the prevalent 16th-century attitude dominated by astrology and religion, but does not make clear why this fixation could maintain itself for centuries despite the withering away of this prevalent attitude. So why has syphilis, even in recent times, been constructed as an essentially venereal or sexually transmitted disease? Could it have been, a conceptual realist might be tempted to suggest, because despite all their superstition our 16th-century ancestors happened to hit upon an essential trait of the disease? Because syphilis simply *is* a venereal disease? After all, is there anybody who would deny that indeed it is? It seems as if the sheer weight of the verdict of reality prevents any sensible constructivist endeavour from getting off the ground. Fortunately, it is still possible for the constructivist analyst to get a handle on this difficult matter. During the 20th century some medical investigators who were familiar with non-venereal tropical and subtropical diseases closely related to syphilis have actually questioned prevalent disease classifications. In section 2 I will pay attention to the views of this dissident minority because they enable us to loosen the hold on our minds of the 'venereal fixation' characteristic of the modern concept of syphilis.

In pursuing the influence of moral considerations on the framing of syphilis concepts I have devoted a separate section to the vicissitudes of the curious notion of 'syphilis insontium' (or 'syphilis in the innocent') which was very popular at the end of the 19th century. Explicitly framed in moral terms, this concept was instrumental in bringing about a partial 'de-moralization' of syphilis.

Fleck spoke about the "stigma of fatefulness and sinfulness" which was imprinted upon syphilis by the fixation upon the emotive venereal nature of the disease (see above). I would like to add that this moral stigma was even more pronounced by the fact that before 1900 syphilis was not only considered a *venereal* but also a *hereditary* disease, thereby reinforcing in particular the aspect of 'fatefulness'.[2] In modern eyes it may seem rather strange to

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1 D. Bloor, *Wittgenstein: A Social Theory of Knowledge*, London and Basingstoke (Macmillan), 1983, pp. 34-37. More correctly, Bloor uses Fleck's account to defend Wittgenstein's theory of family resemblances, but the latter theory is closely connected to Mary Hesse's finitist theory.

2 For Fleck, this aspect of fatefulness was already established by astrological speculation at the turn from the 15th to the 16th century. At that time many authors considered the syphilis epidemic in Europe from 1494 onward to be caused by a particular astrological constellation occurring on 25 November 1484: a "conjunction of Saturn and Jupiter under the sign of Scorpio and the House of

consider a particular disease as being both infectious and hereditary. Before 1900, however, and even beyond, both properties were not generally regarded as mutually exclusive. Just as it may be appropriate to ask why certain diseases are constructed or framed as venereal diseases, likewise we can also inquire why they are conceived as being of a hereditary or, for that matter (keeping to the tenets of symmetry and impartiality), of a non-hereditary character.[3] I have devoted section 4 to an extensive discussion of the concept of 'hereditary syphilis' to explore how it was associated with cultural values and perceptions and how it gradually gave way to our modern concept of 'congenital syphilis'. The latter transition was part of a major transformation of the conceptual network which resulted in the attributes 'infectious' and 'hereditary' becoming mutually exclusive properties. The wide conceptual ramifications of this change underscore the appropriateness of the network metaphor.

The ramifying character of conceptual networks also poses a problem for writing this chapter. Ostensibly about the historical genesis of the modern concept of syphilis, it cannot fully avoid discussing some other nodes in the expanding conceptual network of medical knowledge to which this concept became connected (such as those of the aetiological agent or the Wassermann reaction) and which I had reserved for treatment in later chapters. I take comfort in the thought that this is not simply to be blamed on my lack of compository skills but agrees with what can be expected from the network theory. As Barnes in an exposition of this theory explains:

> "[..] there is no way of acquiring knowledge in a genuinely step-by-step manner, with each step being completely understood and justified before moving on to the next. The knowledge associated with any part of a conceptual fabric is only fully acquired when the whole fabric has been acquired. Conceptual fabrics [..] have the character of hermeneutic systems [..]".[4]

The same problem will of course recur in later chapters. Everything is connected to everything else, but one can only discuss in sufficient detail one thing at a time. In chapter VIII I will try to give an overview of 'the whole fabric'.

In the next section I start with reconstructing and commenting upon Fleck's account of the making of the modern concept of syphilis, before expounding my own views relating to certain problems in the 'aetiological' concept of syphilis and to the venereal character of the disease. Section 3 will deal with 'syphilis insontium', section 4 with 'hereditary syphilis'.

Mars [..] The sign of Scorpio, which rules the genitals, explains why the genitals were the first place to be attacked by the new disease" (Iwan Bloch, cited by Fleck 2/4).

3 See E.J. Yoxen, 'Constructing Genetic Diseases', in P. Wright and A. Treacher (eds.), *The Problem of Medical Knowledge: Examining the Social Construction of Medicine*, Edinburgh (Edinburgh University Press), pp. 144-161.

4 B. Barnes, *T.S. Kuhn and Social Science*, London and Basingstoke (Macmillan), 1982, p. 73.

2. The making of the modern concept of syphilis

In broad outline Fleck distinguishes four successive concepts of syphilis spanning the historical period from the end of the 15th to the first decade of the 20th century:

(a) syphilis as an ethical-mystical disease entity of 'carnal scourge' (*Lustseuche*);
(b) syphilis as an empirical-therapeutic disease entity responding to mercury compounds;
(c) a pathogenetic concept of syphilis as a generalized disease (related to the idea of 'foul blood');
(d) the aetiological concept of syphilis as a disease caused by a specific microbial agent.

I will follow Fleck's exposition and comment on each of the concepts distinguished by him. Most attention will be paid, however, to the more recent 'aetiological' concept of syphilis. Under this heading I will offer my own thoughts about the subject, referring to Fleck's views only when these can be usefully taken into account.

The carnal scourge and the differentiation of venereal diseases
The conception of syphilis as the carnal scourge *tout court* comprises not just what we today would call syphilis but also the other venereal diseases, viz. gonorrhoea, ulcus molle (soft chancre, chancroid) and conditions that were only later recognized as such like lymphogranuloma venereum. Slightly disagreeing with Fleck, Kenneth Flegel maintains that it was only after 1540 or 1550 that syphilis, gonorrhoea and soft chancre were generally taken as manifestations of the same 'Venus disease' or *lues venerea*; earlier writers had distinguished syphilis at least from gonorrhoea.[5] From about 1550 to about 1750 the belief in the existence of one venereal disease was unchallenged. Since the second half of the 18th century until well into the 19th century, however, fierce controversies were raging between the adherents of the so-called identity and duality theories. Flegel asserts that the confusion was finally resolved in 1838, when the famous French venereologist Philippe Ricord, on the basis of clinical evidence and experimental findings (in the previous 7 years he had performed no less than 2.500 inoculation experiments!), proclaimed to the world that gonorrhoea and syphilis were different diseases. But the process of enlightenment was not completed even with Ricord, for he apparently did not yet differentiate between syphilitic chancre and

[5] K.M. Flegel, 'Changing Concepts of the Nosology of Gonorrhea and Syphilis', *Bulletin of the History of Medicine* 48 (1974): 571-88.

chancroid.[6] This differentiation was to be the contribution of his pupil Bassereau and of the Lyonnese physician Rollet. It is important to note that the distinction between the three venereal diseases was already fairly widely accepted before the actual onset of the bacteriological era.[7] The subsequent identification of the causative agents of gonorrhoea (Neisser, 1879), ulcus molle (Ducrey, 1889) and syphilis (Schaudinn and Hoffmann, 1905) merely confirmed the already accepted separation of three different diseases.

One aspect of this historical development that deserves further comment is the role played by inoculation experiments in the eventual separation of syphilis from other venereal diseases. It is difficult not to agree with Fleck's view that one single experiment has a very limited significance compared with "the total experience consisting of experiments, observations, skills, and transformations of concepts available within a given field" (10/16). A well-known example is the notorious experiment performed by John Hunter in the second half of the 18th century. Hunter inoculated himself with the urethral discharge of a man who was alleged to have gonorrhoea and ... obtained an ulcer followed by a typical syphilis. Later writers have explained this outcome, which for Hunter proved the unity of syphilis and gonorrhoea, from the unlucky coincidence of a mixed infection. Fleck uses this example to draw an important epistemological conclusion: "Even a heroic 'crucial experiment', such as that performed by Hunter, proves nothing, for its result must now be regarded as either an accident or an error" (10/16). Contemporary philosophy of science would concur with the view that 'crucial' experiments in the literal sense of the word do not exist, due to the Duhem-Quine Thesis. Still we should not overlook the persuasive force of the very large number of inoculation experiments performed by Ricord, all pointing in the same direction. In science and in medicine, sheer numbers do count for something.

It must be admitted that the above description of the amalgamation and subsequent differentiation of the three venereal diseases has a rather 'Whiggish' savour. In such a brief compass this seems hardly avoidable. The following formulation of the problem given by Flegel also smacks of 'Whig history': "Soft chancre, gonorrhea and syphilis were considered as one. This conviction was a central tenet of venereology for three hundred years. How could maladies which had been previously well known become confounded with a striking, supposedly new, epidemic disease?".[8] Here the 'Whiggish' tendency is somewhat too strong. We would do well, I think, to avoid the patronizing suggestion that the medical

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6 Flegel, op. cit. (note 5), p. 586 footnote 83; O. Temkin, 'Therapeutic Trends and the Treatment of Syphilis before 1900', *Bulletin of the History of Medicine* 29 (1955): 309-16, p. 310.

7 Flegel observes in a footnote: "The fact is that *present-day* physicians regard gonorrhea and syphilis as different diseases, *most particularly because the causative agents are different*. However, physicians believed that they were different before the discovery of bacteria [..]"; Flegel, op. cit. (note 5), pp. 575-76, footnote 23 (emphasis added).

8 Flegel, op. cit. (note 5), p. 575.

writers of previous centuries should have known better. Bearing this in mind, however, it is still legitimate to ask why those writers recognized only one venereal disease.

Part of the explanation may undoubtedly be found, as Fleck suggests, in the early fixation on the sexual mode of transmission as a morally relevant trait of the disease, which was reinforced by religious (and astrological!) convictions. An additional explanatory factor, according to Temkin, may be found in the theory of humours which dominated medical thought during the Renaissance and afterwards: "The venereal poison, having entered the body, was compelled towards its exterior. Fortunate those whom it was discharged in form of a gonorrhea, for they might escape the true lues!".[9] The theory of humours would also offer a persuasive interpretation of the treatment of syphilis with mercury compounds which were already introduced early in the 16th century.

The 'empirical-therapeutic' concept of syphilis

Before the syphilis epidemic at the end of the 15th century, mercury was already in use as a remedy against scabies and leprosy. Because the new disease also manifested eruptions on the skin, it was understandable that the familiar metal should be tried on it too, and, apparently, with some positive effect. Fleck contrasts the 'ethical-mystical' concept of syphilis as 'carnal scourge' with the 'empirical-therapeutic' concept of syphilis as a disease entity responding to mercury. Although he considers the two concepts to be mutually contradictory, he also notes that they eventually became amalgamated (5/8-9). It may be doubted whether the 'empirical-therapeutic' concept ever constituted a full-blown concept of syphilis in its own right, but apart from that Fleck's contrast of this supposedly 'empirical' concept with the allegedly 'mystical' concept of 'carnal scourge' is too strong. It is significant that mercurial therapy was also interpreted in 'ethical-mystical' terms as an atonement for sin. Temkin cites the following statement from the 16th-century physician Jacques de Béthencourt, one of the first to use the name *mal vénérien* (venereal disease), about the influence of mercury on the body and soul of the syphilitic sinner: "It is never resolved except under the influence of a medication which imposes on the body the chastisement of its impurity and on the soul the punishment of its errors".[10] Hence the terror of the mercurial cure that was imposed on the poor sinful souls![11] The more 'empirical' effect on the body was interpreted in the light

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[9] Temkin, op. cit. (note 6), p. 313.

[10] O. Temkin, 'On the History of "Morality and Syphilis"', in: *The Double Face of Janus and Other Essays in the History of Medicine*, Baltimore and London (The Johns Hopkins University Press), 1977, p. 472. Compare C. Quétel, *History of Syphilis*, Cambridge (Polity Press), 1992, chapter 2.

[11] It is remarkable that also in later periods a disguised punitive intent has often been attributed to the application of such a painful and disagreeable cure as mercurial therapy: "Continuation of the unpleasant treatment long after the subsidence of the outward manifestations may well have been motivated by a Calvinistic sadism to discourage the miscreant from further immoral activities" (L.J. Goldwater, cited in J.R. Walkowitz, *Prostitution and Victorian Society*, Cambridge [Cambridge University Press], 1982, p. 55).

of the humoral theory, which regarded the elimination of the morbid humours through salivation or sweating as essential to the cure.[12] Fleck also notes that the mercurial therapy was aimed at the "evacuation of the syphilis toxin through the sputum" (4/7), i.e. at stimulating the flow of toxic saliva - on modern understanding a very dangerous endeavour! "And for about 200 years or more"' Temkin observes, "to treat syphilis by mercury or 'to salivate' became synonymous expressions".

The 'pathogenetic' concept of syphilis

The next concept of syphilis considered by Fleck is the idea of a pathogenetic disease entity, i.e. a view about the supposed mechanism of the pathological associations. According to Fleck, the pathogenetic concept of syphilis was based on the theory of humours. This theory could account for the 'constitutional' or 'generalized' character of (secondary) syphilis in terms of an *alteratio sanguinis* or 'change in the blood'. This *Uridee* or proto-idea of 'foul blood' would ultimately find its fulfilment in the Wassermann reaction, a serological test developed in 1906 to detect syphilis. Elsewhere, together with Bart Gremmen, I have criticized this suggested connection between the proto-idea of 'foul blood' and the Wassermann test as untenable.[13] In laying so much stress on the theory of humours as the underpinning of the pathogenetic concept of syphilis, Fleck unduly neglects other pathogenetic ideas. Important contributions of the 19th century which resulted in a gradual rounding out of the clinical picture of syphilis - e.g. Ricord's division of the course of the disease in a primary, secondary, and tertiary stage; Welch's recognition of the syphilitic origin of aortic aneurysm; and Fournier's view of tabes dorsalis and general paresis as 'para-syphilitic' conditions - cannot be fitted into Fleck's preconceived scheme. This scheme is also open to historical criticism. During the 18th century, as Temkin observes, many of the ideas of humoral pathology, in particular the belief in the occult powers of human blood, were relegated to the realm of superstition, "because the philosophy of the Enlightenment did not admit the occult".[14] Fleck is not blind to this general trend of medical thought, but he takes the case of syphilis to be the singular exception to this trend: "'Change in the blood' was a popular phrase used to explain all generalized diseases. Whereas it went progressively out of fashion for other diseases, however, its significance only increased in the case of syphilis" (11/18). In my opinion, it is largely Fleck's wish to see the Wassermann reaction as a realization of the proto-idea of foul blood which is responsible for this historical judgement. Temkin points

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12 Temkin, op. cit. (note 6), p. 313.

13 H. van den Belt and B. Gremmen, 'Specificity in the Era of Koch and Ehrlich: A Generalized Interpretation of Ludwik Fleck's "Serological" Thought-Style', *Studies in the History and Philosophy of Science* 21 (1990): 463-479.

14 Temkin, op. cit. (note 6), p. 312.

out that the abandonment of humoral ideas was not without effect on the treatment of syphilitics. It resulted in the spread of milder mercurial treatment regimes without salivation.[15]

The 'aetiological' concept of syphilis

The 'aetiological' concept of syphilis became established with the discovery in 1905 of the causative agent, *Spirochaeta pallida* (now *Treponema pallidum*), by Fritz Schaudinn and Erich Hoffmann. As Fleck rightly points out, this achievement does not constitute the final consummation of the development of the modern concept of syphilis as a specific disease, because such a process is "incomplete in principle, involved as it is in subsequent discoveries and new features of pathology, microbiology, and epidemiology" (19/28). In the next chapter I will deal in detail with the discovery and subsequent acceptance of the pale spirochaete as the aetiological agent of syphilis. Here I will use the example of syphilis to discuss a fundamental question that is often raised in the philosophy of medicine: How should diseases be defined? Can they be defined by their 'causes' or, in the case of infectious diseases, by their 'aetiological agents'? The case of syphilis presents some interesting complications which may shed a new light on this philosophical problem and stimulate further reflection. Discussion of these complications will also provide a suitable occasion for broaching the vexed question of the essentially venereal character of syphilis and for examining alternatives to the prevalent 'venereal fixation'.

In the history of medical science it is a quite normal occurrence that a particular disease, after its cause has been identified, will become *redefined* precisely in terms of that specific cause. A case in point is the redefinition of tuberculosis. Already in 1883, only one year after Koch's discovery of the tubercle bacillus, Adolf Strümpell wrote in the first edition of his well-known *Lehrbuch der speciellen Pathologie und Therapie*: "The definition of tuberculosis is no longer based on any outward anatomical characteristic. Any disease which is caused by the pathogenic action of a specific species of bacterium, the tubercle bacillus discovered by Koch, is considered tuberculous".[16] This redefinition raises a problem, because it would seem to transform the *prima facie* informative statement that tuberculosis is caused by the tubercle bacillus into a mere tautology.[17] The problem has been highlighted by the Danish physician and philosopher Henrik Wulff in a comment on the position

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15 Ibid., p. 313.

16 A. Strümpell, *Lehrbuch der speciellen Pathologie und Therapie*, Leipzig, 1883; quoted from F. Wibaut, *De Methode der Geneeskunde*, Haarlem (De Erven Bohn N.V.), 1962, p. 63.

17 Lester King holds that tuberculosis only became a neatly defined disease with Koch's discovery: "Instead of vague standards of tissue structure, such as tubercles or giant cells, Koch offered a precise standard, namely the identification of the tubercle bacillus. According to this scheme when the bacilli are identified, the case is one of tuberculosis *by definition*". See L.S. King, 'Dr. Koch's Postulates', *Journal of the History of Medicine and Allied Sciences* 7 (1952): 350-61, p. 356.

taken by the Australian philosopher J.L. Mackie. In a seminal article on causes and conditions, Mackie used the example of yellow fever:

> "[...] we may say that the yellow fever virus is the cause of yellow fever. (This statement is not, as it might appear to be, tautologous, for the yellow fever virus and the disease itself can be independently specified)".[18]

Not so, replies Wulff:

> "I think that he is wrong on this point. There are mild cases of infection with yellow fever virus which no clinician could recognize clinically with certainty, but we should still say that the patient suffered from yellow fever, if we succeeded in isolating the virus from a blood sample. Similarly we can imagine clinical pictures, which are indistinguishable from typical yellow fever, but we would not say that such patients suffered from yellow fever, if some other infective agent was isolated. *Yellow fever has been redefined to mean yellow fever virus infection* [emphasis added]." [19] Wulff holds that this case can be generalized and that causal factors are never (or very rarely) necessary in relation to a disease entity except by definition.

Although I agree with Wulff that redefinition of diseases in terms of presumed causes is what normally happens in medicine (the above example of tuberculosis is just another illustration), I would like to amend his position in order to avoid some of its unpleasant consequences. Is it really no more than a tautology to state that yellow fever is caused by the yellow fever virus, tuberculosis by the tubercle bacillus, or syphilis by the pale spirochaete?

It must be stressed that the introduction of an aetiological definition of a certain disease entity does not occur at point zero of historical development. As a purportedly stipulative definition it is not a free choice independent of all preceding history, as Fleck would also emphasize. It often presupposes a prior definition of the disease entity in clinical or other non-aetiological terms. In order to clarify a point of principle we can divide, somewhat artificially, the period following the discovery of a candidate microbial agent of a particular disease into two stages. In the first stage the relevant scientific community will have to reach a decision whether it will accept the proposed microbe as the aetiological agent responsible for the disease. Ideally, the community will be guided in this decision by the methodological rules for establishing causality laid down by Robert Koch and known as *Koch's postu-*

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[18] J.L. Mackie, 'Causes and Conditions', *American Philosophical Quarterly* 2.4 (October 1965); partly reprinted in : E. Sosa (ed.), *Causation and Conditionals*, Oxford (Oxford University Press), 1975, pp. 15-38, p. 29.

[19] H.R. Wulff, 'The Causal Basis of the Current Disease Classification', in: L. Nordenfelt and B.I.B. Lindahl (eds.), *Health, Disease, and Causal Explanations in Medicine*, Dordrecht (Reidel), 1984, p. 174.

lates.[20] To be meaningful at all, these rules presuppose that the candidate agent and the disease itself can be, to use Mackie's words, "independently specified". Of course, in practice the application of these rules is not always simple and straightforward.[21] Sometimes circularity cannot be completely avoided.[22] Let us suppose, however, that these difficulties have been adequately dealt with and that the microbial candidate Y applying for the vacancy of 'aetiological agent of disease X' has passed Koch's severe test with flying colours. At that moment we would have good reasons to assert "Disease X is caused by microbe Y" without thereby uttering an empty tautology. Now the second stage sets in and disease X will be *redefined* as 'the disease that is caused by microbe Y'. This means that the relevant scientific community will treat the proposition "Disease X is caused by microbe Y" from then on effectively as an analytic statement. It is to be left in place, come what may. Apparently contrary phenomena, such as those alluded to by Wulff, have to be assimilated by making adjustments elsewhere in the conceptual fabric. Perhaps they can be addressed by introducing Nicolle's notion of *infection apparente* (cf. Fleck: 18/27), or by splitting off a new disease entity called pseudo-X or para-X. Such at least is the solution the finitist or network theory would suggest for the problem of tautology or analyticity which engaged the philosophers Mackie and Wulff.[23] It will be clear that the finitist viewpoint represents an intermediate position between the views of both.

Finitism emphasizes, however, that analyticity is not an intrinsic property of statements. As Barnes explains:

> "Analytic propositions are intrinsically no more immune to experience or exempt from adjustment than any others; they are merely those which a particular community, *as a matter of convention*, is currently treating as analytic. As the practice of the community alters, so too does the status of its generalisations.

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20 K. Codell Carter, 'Koch's Postulates in Relation to the Work of Jacob Henle and Erwin Klebs', *Medical History* 29 (1985): 353-74.

21 Some of the typical difficulties involved in the application of Koch's postulates are illustrated for the case of influenza by Ton van Helvoort, *Research Styles in Virus Studies in the Twentieth Century: Controversies and the Formation of Consensus*, Maastricht (Thesis), 1993, Chapter 3, pp. 58-76.

22 Though not all circularity is necessarily of a 'vicious' sort; there is also 'virtuous circularity', as is argued by Thomas Nickles, 'Twixt Method and Madness', in N. Nersessian (ed.), *The Process of Science*, Dordrecht (Martinus Nijhoff), 1987, pp. 41-68.

23 A similar view has been developed by Herman Koningsveld. He argues that empirical generalisations acquire a lawlike status by being armed against negative instances - in the finitist terminology by becoming analytic statements -, but requires that the so-called 'exceptions' be adequately dealt with by on-going concept- and theory-formation. See H. Koningsveld, *Empirical Laws, Regularity and Necessity*, Wageningen (Thesis), 1973.

Yesterday's empirical claims may be made today's analytic truths; or the analytic statements of days gone by may be demoted into mere empirical generalisations - and false ones at that [..]".[24]

Thus in medical science statements about the causative agents of diseases routinely alter their status from empirical claim to analytic truth once the proposed aetiology is generally accepted. One marginal note should be added to the passage just quoted from Barnes: there is not always unanimous or community-wide agreement on treating certain propositions as analytic. Clinicians, for example, have sometimes objected to (re-)defining diseases in aetiological terms.[25]

After this philosophical excursion it is time to return to the historical case of syphilis. What were the consequences for the concept of syphilis when in May 1905 Schaudinn and Hoffmann proclaimed to the German medical world that they had identified a pale spirochaete, *Spirochata pallida*, as the probable causative agent of the disease?

Initially, their announcement met with extreme skepticism. The pale spirochaete also had to contend with a rival pretender for the title of aetiological agent of syphilis, John Siegel's *Cytorrhyctes luis*, which was supported by eminent protozoologists.[26] According to an official German report released on 12 August 1905, however, spirochaetes had already been found "by more than a hundred authors in the most diverse products of syphilis" (16/25). This large number of confirmations tipped the balance in favour of *Spirochaeta pallida*. On 18 January 1906, Germany's foremost dermatologist and venereologist, Albert Neisser, declared that Schaudinn's spirochaete was "in all likelihood" *(mit grösster Wahrscheinlichkeit)* the aetiological agent of syphilis, "although the compelling proof: 'experimental production of the disease through pure cultures', has not yet been provided".[27] This 'compelling proof' would never be produced. The spirochaete stubbornly resisted any attempt at pure culture. Koch's postulates could therefore not be fully satisfied. Even in the absence of this crowning

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24 Barnes, op. cit. (note 4), p. 78. A well-known example of such a transition is provided by the chemical law of constant composition: from a (false) empirical claim in pre-Daltonian chemistry it changed into an analytic truth when Daltonian chemistry redefined the concept of chemical compound. See T.S. Kuhn, *The Structure of Scientific Revolutions*, Chicago (University of Chicago Press), second and enlarged edition, 1973 [1970], p. 133.

25 The French clinician René Cruchet writes: "It is however not with the pneumococci or with Koch's bacilli that one can establish a clinical diagnosis. What characterizes pneumonia are the symptoms that it exhibits, and the same holds for pulmonary tuberculosis, bronchial pneumonia and purulent pleuresy [..]. The error began on the day when specificity became a function of the bacillus itself". See R. Cruchet, *De la Méthode en Médecine*, Paris, 1951, pp. 37-38, cited in Wibaut, op. cit. (note 16), pp. 279-80. This is contrary to Lester King's view quoted before in note 17. (Fleck himself, though no clinician, was of course also strongly opposed to aetiological definitions of diseases!)

26 The controversy between the adherents of the two microbes will be analyzed in the next chapter.

27 A. Neisser, 'Versuche zur Uebertragung der Syphilis auf Affen', *Deutsche medizinische Wochenschrift* 32 (1906), p. 102.

piece of evidence, *Spirochaeta pallida* was accepted as the causative agent of syphilis. The modalities which accompanied the attribution of its aetiological status ('probable', 'in all likelihood' etcetera) were eventually omitted.

It may be worthwhile to examine the views of one particular dissident who questioned the aetiological status of the pale spirochaete: Professor Ottomar Rosenbach, a colourful critic of bacteriology and a staunch defender of the virtues of clinical medicine.[28] In his book *Das Problem der Syphilis* (1903, second edition 1906) he had already launched a full-scale attack on modern ideas about syphilis. Rosenbach dissociated himself from the widely held alarmist view that the incidence of syphilis had increased dramatically during the previous decades. In his opinion, this impression was largely the result of a diagnostic fashion: "for one hardly meets a case history of myocardial affection, arteriosclerosis, or nervous disease, in which lues is not adduced as aetiology".[29] Rosenbach did not recognize the syphilitic nature of neurological disorders like tabes and general paresis or of cardiovascular affections like aortic aneurysm. He opposed what he called the "monistic way of thinking", which attempted to trace various kinds of affections back to a syphilitic origin and syphilis itself to a tangible microbial agent. Rosenbach accused the adherents of the pale spirochaete of taking insufficient care to avoid the vicious circle: "Because one considers the carrier of the spirochaetes to be luetic, one is naturally tempted to attribute to the microbes found with him the character of *Spirochaeta pallida* [...]".[30] The vicious circle was also supposed to turn the other way around: "The problem is to establish the causative agent of lues, and it is not allowed, if one considers *Spirochaeta pallida* as such, to assume as proved what has yet to be proved, i.e. to take the presence of spirochaetes as positive evidence for the existence of a luetic infection [...]".[31] Rosenbach therefore formulated the following methodological requirement: "One should, without knowing the clinical diagnosis, consequently without any subjective prejudice, attempt to determine the parasites in all tissues and lesions in which spirochaetes are to be found, or, even better, have them determined by a person uninformed about the nature of the disease case".[32]

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28 Information on Ottomar Rosenbach can be found in R.C. Maulitz, '"Physician versus Bacteriologist", the Ideology of Science in Clinical Medicine', in M.J. Vogel and C.E. Rosenberg (eds.), *The Therapeutic Revolution*, Philadelphia (University of Pennsylvania Press), 1979, pp. 91-107. See also K. Faber, *Nosography: The Evolution of Clinical Medicine in Modern Times*, New York, second edition, 1930, pp. 119-29.

29 O. Rosenbach, 'Genügt die moderne Diagnose syphilitischer Erkrankung wissenschaftlichen Anforderungen?', *Berliner klinische Wochenschrift* 43 (1906): 1157-60, p. 1158.

30 Ibid., p. 1159.

31 Ibid., p. 1160.

32 Ibid., p. 1159.

It was not too difficult for bacteriologists and clinicians to meet Rosenbach's challenge. In a rejoinder Julius Heller and Lydia Rabinowitsch pointed out that they had already taken the methodological precautions he demanded.[33] Vicious circles can sometimes be avoided. Once the aetiological status of the pale spirochaete had been accepted, its presence or absence could be used to clarify the nature of those conditions of which the syphilitic origin was still insecure or disputed. Hideyo Noguchi's demonstration of *Spirochaeta pallida* in the brains or spinal cords of patients with general paresis or tabes, for example, resulted in the definitive assimilation of these disorders to syphilis. They became even more firmly linked to syphilis than Fournier had admitted when he granted them the status of 'para-syphilitic' conditions.[34]

Although the aetiological concept of syphilis became more firmly entrenched, it also had to meet a challenge from an unsuspected quarter. By a strange coincidence the year 1905 not only witnessed the discovery of *Spirochaeta pallida* but also the discovery of another spirochaete, which was admittedly indistinguishable from the first one yet presumably different, because it was found in a disease that was (presumably) different from syphilis. This microbe was called *Spirochaeta pertenuis* by its discoverer, Aldo Castellani, director of the clinic for tropical diseases at Colombo (Ceylon). Castellani had found the spirochaete among sufferers from yaws (framboesia tropica) at his clinic. After consulting Schaudinn about his find - they jointly examined the microscopic preparations -, Castellani drew the following conclusion as to the nature of his spirochaete:

> "In my view this species is at the moment morphologically indistinguishable from Spirochaeta pallida (Schaudinn). But because I believe that yaws differs from syphilis, I am inclined to think that the spirochaete I have found in yaws - if it figures indeed in the aetiology - must be biologically different from the syphilis spirochaete. I therefore suggested the name Spirochaeta pertenuis seu Pallida".[35]

These considerations clearly support the observation made by Fleck in a related context, that it is the disease that defines the causative agent rather than the other way around (18/27). The same view was expressed several decades later. Writing about the classification of pathogenic treponemes, Paul Hardy Jr. stated in 1976 that "[..] the identity of some members of this group depends more upon certain features of the diseases they produce than upon specific characteristics of the organisms themselves".[36] If Castellani had concluded that

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33 J. Heller and L. Rabinowitsch, 'Erwiderung', *Berliner klinische Wochenschrift* 43 (1906), p. 1357.

34 H. Noguchi and J.W. Moore, 'A Demonstration of Treponema pallidum in the Brain in Cases of General Paralysis', *Journal of Experimental Medicine* 17 (1913): 232-38.

35 A. Castellani, 'Untersuchungen über Framboesia tropica (Yaws)', *Deutsche medizinische Wochenschrift* 32 (1906): 132-34.

36 P.H. Hardy Jr., 'Pathogenic Treponemes', in R.C. Johnson (Ed.), *The Biology of Parasitic Spirochetes*, New York (Academic Press), 1976, pp. 107-19, on p. 107.

Spirochaeta pallida apparently causes syphilis on some and yaws on other occasions, he would have violated the principle of aetiological specificity.[37] If the diseases are different, then the causes must be different (although as yet no difference was discernable!). But are syphilis and yaws really distinct disease entities?

On the basis of inoculation experiments performed on apes and monkeys during his expeditions to the Dutch East Indies (1905-1907), Albert Neisser tried to answer this question.[38] Primates which were first infected with yaws could be reinfected with syphilis and vice versa. Neisser therefore concluded that syphilis and yaws were distinct diseases. At the time, this conclusion found wide approval.

To the Dutch physician J.D. Kayser, a resident of the Dutch East Indies, those inoculation experiments were however far from conclusive.[39] The old opposition between 'dualists' and 'unitarians', so familiar from the history of syphilis, emerged in a new version. Initially, the second group counted only a small group of adherents, Kayser among them. The latter did not deny all the differences that were emphasized by the 'dualists' - the non-venereal mode of transmission, the absence or relative infrequency of congenital forms of yaws and of neurological disorders like tabes and paresis, the different appearance of the primary and secondary lesions etcetera -, but disputed their relevance. He stressed, on the other hand, the striking similarity of the tertiary lesions in syphilis and yaws, the indistinguishability of the respective spirochaetes, the fact that both syphilitics and sufferers from yaws reacted positively to the Wassermann test and that both responded to the same medication (mercury and Salvarsan). In other words, the case of syphilis and yaws exhibits the typical pattern of a crisscrossing of similarities and differences to which adherents of the finitist theory have drawn attention. Kayser also pointed out that ethnicity was often tacitly used as a diagnostic criterion in the medical practice of the Dutch East Indies: a person of European origin with certain symptoms would be diagnosed as having syphilis, an indigenous person exhibiting the same symptoms would be diagnosed as suffering from yaws. He concluded that yaws was probably a special form of syphilis. This conclusion was to be echoed by several subsequent investigators: "Yaws is regarded by many as a tropical type of syphilis, modified by continuous residence in the black races in tropical climates".[40]

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37 "The principle of etiological specificity of disease implies that every disease entity is produced by a quite particular cause, that different diseases cannot arise from the same cause, nor can different causes produce the same disease"; Jendrassik, cited in Faber, op. cit. (note 28), p. 183.

38 A. Neisser, 'Ueber experimentelle Syphilis bei Affen (Sitzungsberichte)', *Berliner klinische Wochenschrift* 43 (1906): 373-74.

39 J.D. Kayser, 'Vereenigingsverslagen: Over framboesia tropica', *Nederlandsch Tijdschrift voor Geneeskunde* 57 (1913): 1577-84.

40 E.O. Jordan and W. Burrows, *Textbook of Bacteriology*, Philadelphia and London (W.B. Saunders), 1948, p. 690.

By the time Fleck wrote his monograph, he noted that "the relation of syphilis to *Frambösia tropica* [..] is still disputed" (172 note 45/28 note 45). For him this circumstance illustrated once more the incompleteness in principle of the development of the concept of syphilis as a specific disease (19/28).

Around the mid-1930s, when Fleck's monograph appeared, non-venereal forms of syphilis, so-called 'endemic syphilis', were found among peoples living under more primitive hygienic conditions, where infection normally occurs during childhood. An interesting example was reported by E.H. Hudson in 1937.[41] The Bedouin semi-nomads of the Syrian desert were familiar with a non-venereal form of syphilis called *bejel* which affected almost 90 percent of the population. Although its causative agent was morphologically indistinguishable from *Treponema pallidum*, the agent of (venereal) syphilis, its clinical symptoms clearly set it apart from the venereal form. The Bedouins themselves recognized this difference by using two names, *bejel* and *franghi* respectively, to refer to the two types. According to Hudson, *bejel* occupied an intermediate position between (venereal) syphilis and yaws (whose aetiological agent, as we have seen, is also indistinguishable from *Treponema pallidum*). One year later, in 1938, the disease *pinta*, from Central America and related to syphilis and yaws, was added to this family. In 1946 Hudson offered a 'unitarian' interpretation, placing syphilis, yaws and pinta as well as forms of 'endemic syphilis' such as bejel and the newly discovered *njovera* of Rhodesia in the category of treponematoses, all caused by a single microbial species, *Treponema pallidum*. In his view they were not so much different diseases as different manifestations of the same disease appearing under varying climatic, environmental and hygienic conditions.[42]

The intention of the above discussion was not to argue that yaws and pinta are, or are not, forms of syphilis. That question should be left to the medical scientists themselves. Whatever decision is to be taken on this issue - and there may indeed be good reasons for choosing either of the two alternatives! -, the point is that it is not strictly predetermined by the established meaning the term 'syphilis' possessed when it was first confronted with these anomalies. Nor could the decision be *forced* by a possible future elucidation of the nature of the spirochaetes in question, for this would only shift the problem to the level of

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41 E.H. Hudson, 'Lecture on bejel (Verenigingsverslagen)', *Nederlandsch Tijdschrift voor Geneeskunde* 81 (1937): 4737-38.

42 See G.E. Davis, 'The Spirochetes', *Annual Review of Microbiology* II (1948): 305-33, on p. 318 (containing a reference to Hudson, *Am.J. Trop. Med.*, 26, 135-39 [1946]). See also E.H. Hudson, *Nonvenereal Syphilis*, Edinburgh and London (E & S Livingstone), 1958. The 1968 edition of a well-known textbook on microbiology stated: "While the differences among these diseases are felt by some to require considering them as separate entities, the differences are not much greater than those between syphilis in the Middle Ages and present-day venereal syphilis. It is believed by many that the similarities are more striking than the differences and these, together with other considerations, have led to a 'unitarian' view of treponemal infections in which the several clinical manifestations are considered to be variations on a central theme." (W. Burrows et al., *Textbook of Microbiology*, Philadelphia [W.B. Saunders], nineteenth edition, 1968, p. 751).

microbiological classification. This level is not inherently more unambiguous than the level of clinical symptoms. As Fleck observes: "The idea of the syphilis agent leads into uncertainties attending the concept of bacteriological species [..]" (19/28).[43] Our general conclusion is in agreement with the finitist or network theory.[44]

An interesting consequence of adoption of the 'unitarian' view which considers syphilis, yaws and pinta as different manifestations of the same disease would be a relaxation of the 'venereal fixation'. Recognition of the existence of non-venereal forms of 'endemic syphilis' such as bejel in the Middle East or njovera in Rhodesia points in the same direction. It is the latter consideration which led the Dutch venereologist J.R. Prakken to a carefully worded definition in a textbook on venereal diseases. As venereal diseases are designated, according to his definition, "a number of contagious diseases, which in societies with decent hygienic conditions are as a rule transmitted through sexual intercourse".[45] He insists that the restriction with regard to the general state of hygiene cannot be omitted in the definition. Still the venereal character of syphilis is not completely relinquished, far from it. But what, if anything, is a venereal disease? The fact that also in countries with high standards of hygiene syphilis is sometimes transmitted through non-sexual routes, would make it rather implausible to consider sexual transmission as an essential trait of the disease. However, the circumstance that those other routes are often referred to as 'accidental' provides food for thought. This usage carries the suggestion that the only 'non-accidental' transmission route is sexual intercourse, thereby also implying that syphilis is 'essentially' venereal. To counteract the force of this suggestion it may be helpful to cite the following 'relativistic' comment given by the venereologist Prakken on his own medical specialty:

> "Such a classification [i.e. the uniting of various conditions as venereal diseases, HvdB] according to the character of the contagious contact is very unusual. One can contract many different infectious diseases by eating contaminated food, but probably no one has hit upon the idea to bring them together as eating diseases within the framework of a separate subject of instruction, let alone of a specialty".[46]

The existence of the specialty of venereology, according to Prakken, cannot be justified on purely scientific grounds, but has to be legitimated on practical and social grounds.

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43 In a 1976 study on cross immunity between different species of treponemes and between different strains within the several treponeme species Paul Hardy Jr. concluded: "The differences [between strains within species] are greater than those previously shown to exist between the representatives of the different species"; Hardy, op. cit. (note 36), pp. 117-18.

44 Today the causative agents of syphilis, yaws and pinta can be distinguished from each other by using DNA probes. For a finitist, this would not be final proof that the diseases are 'really' distinct. Everything depends on weighing the relevance of similarities and differences.

45 J.R. Prakken, *Leerboek der Geslachtsziekten*, Amsterdam (Scheltema & Holkema), 1956, p. 11.

46 J.R. Prakken, *Geslachtsziekten*, Amsterdam (Querido), 1968, p. 8.

3. 'Syphilis insontium'

It is one thing to argue the conventional, constructed character of concepts of syphilis in a general way, but it is another matter to show in detail how moral considerations have entered into and helped to shape such constructions. In the preceding section we have seen that moral factors have influenced such early concepts as the notion of a 'carnal scourge' or the punitive element contained in the severe mercurial treatment of syphilis as an 'atonement for sin'. Moreover, there may even be a moral element hidden in the very definition of syphilis as a venereal disease. In this section I shall discuss a more recent and perhaps also more ambiguous example of moral considerations shaping medical classifications.

During the Victorian age the moral assessment of syphilis and other venereal diseases was often a clear and straightforward matter, perhaps somewhat too straightforward:

> "Syphilis is a venereal disease that is acquired above all through extramarital intercourse, *coitus impurus*. Acquisition of lues is proof of offence against morality, of an alliance with vice. Syphilis and vice are linked together, and the reward of vice is disgrace. Thus syphilis is inevitably stigmatized as disgraceful".[47]

Whereas Temkin considers such a moral assessment as characteristic of the epoch of bourgeois emancipation, in which virtue and the institution of marriage were held in high regard, others would rather point to the influence of the moral teachings of the Christian churches which defined extramarital sex as sin.[48] Whatever the emphasis, it is clear that this view of syphilis as a fitting punishment for immoral conduct does not encourage, and would even lead people to frown upon, active medical intervention to alleviate the suffering of syphilitics. Some physicians indeed refused to treat the inexcusable sufferers from this disgraceful affliction, but for many such a refusal would be too harsh. It would also be hardly compatible with the ethical tradition of the medical profession, embodied in its Hippocratic Oath, which enjoins physicians to help those who suffer from illness. As late as the 1950s the Dutch venereologist Prakken still had to insist that "the physician's moral judgement of those who are ill should not influence the execution of his work".[49] However, despite the counterpressure exerted by the Hippocratic Oath, it was not always feasible for physicians to dissociate themselves from the moral condemnation expressed by other members of society.

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[47] Temkin, op. cit. (note 10), p. 480.

[48] J. Cassel, *The Secret Plague: Venereal Diseases in Canada 1838-1939*, Toronto (University of Toronto Press), 1987, pp. 87-88.

[49] Prakken, op. cit. (note 43), p. 14; the continuation of the quoted passage is also interesting: "Moreover, through his knowledge and experience, the physician's judgement differs from that of the mass of the people. He knows that venereal disease can also be acquired outside sexual intercourse, and therefore in an 'innocent' way (*syphilis insontium* is a common designation for lues that is not acquired through sex)."

The concept of *syphilis insontium*, which entered into extensive use at the end of the 19th century, marks the peculiarly paradoxical route through which a partial 'de-moralization' and a reduction of the moral stigma attached to syphilis was obtained. This concept comprised three large categories: (1) hereditary syphilis (what we today would call congenital syphilis); (2) syphilis acquired through legitimate sexual intercourse; (3) syphilis acquired via transmission of the *contagium* through direct or indirect contact excluding sexual intercourse.[50] Uniting the three categories under one concept, despite the scientifically sounding Latin name, appears to make sense only from a moral, not from a strictly biological, point of view. Syphilis remained, as Allan Brandt remarks, "an essentially morally-defined malady".[51]

As long as the concept of *syphilis insontium* was limited to the first two categories, the implied distribution of moral 'culpability' and 'innocence' remained fairly clear. In accordance with Victorian assumptions about male and female sexuality it was supposed that married middle-class women got their infections in complete innocence. The blame was thus put squarely on men, and, by extension, on prostitutes. Moreover, the sins of the fathers were also thought to be visited, along the route of heredity, upon the 'innocent' children. The medical profession was however reluctant to translate such insights into concrete measures for protecting the health and wellbeing of the weaker parties. Physicians often acted in complicity with male patients to keep their infected spouses uninformed about the true nature of the infection. France's leading authority in venereology, Alfred Fournier, had even prescribed active deception as the line to be followed, for the sake of maintaining marital happiness.[52] As a consequence, infected married women often failed to get serious treatment, which probably resulted in more abortions and births of syphilitic children. After the turn of the century the wisdom of this policy of deception was increasingly questioned on ethical and medical grounds.[53]

Moral connotations became more diffuse with the addition of the third category of *syphilis insontium*, which underwent rapid extension at the turn of the 19th to the 20th century. It comprised various modes of extra-genital infection, e.g. through cups, cutlery, bedding, toilet seats, tooth brushes, inoculations, midwifery, ritual circumcision, tattoos

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50 M.S. Gutteling, 'Syphilis insontium', *Nederlandsch Tijdschrift voor Geneeskunde* 41 (1897) II: 841-42 (this is a review of a lecture by Professor Lesser in the *Berliner klinische Wochenschrift* of 12 July 1897).

51 A.M. Brandt, *No Magic Bullet: A Social History of Venereal Disease in the United States since 1880*, Oxford (Oxford University Press), 1987, p. 22.

52 A. Fournier, *Syphilis et mariage*, Paris, 1880.

53 G.O. Lotsy, 'De gedragslijn van den medicus volgens Fournier', *Nederlandsch Tijdschrift voor Geneeskunde* 50 (1906) I: 163-66; N. Macry, 'Standesangelegenheiten: Darf der Arzt der vom Ehemann mit Lues infizierte Frau die Natur ihres Leidens verschweigen?', *Deutsche medizinische Wochenschrift* 34 (1908): 2127-28.

etcetera. The list was rapidly growing. In his classic study on *Syphilis in the innocent*[54] L. Duncan Bulkley had already catalogued more than 100, sometimes rather idiosyncratic, modes of infection. According to modern medical opinion, however, all these suggested non-sexual modes of contagion must have been far-fetched and mostly imaginary, because outside the human body the pale spirochaete cannot survive long.

The consequences of the wider use (or inflation) of the notion of *syphilis insontium* were rather ambivalent. On the one hand, Brandt observes: "Venereal disease remained a stigma; the possibility of innocent infections only implied a larger susceptible population". On the other hand, however, he notes: "The belief in non-sexual transmission served to make treatment more respectable. For members of the middle class, these infections provided a safety-valve; patients could acquire a venereal disease within the boundaries of Victorian morality".[55] Likewise, Jay Cassel observes that "[t]he idea [of 'innocent infection'] provided a release for some people, a means of avoiding, at least in part, the opprobium cast on anyone with VD". On the other hand, however: "With the new theory came increased fears, for now it seemed that many more people might be threatened - even upright, morally virtuous individuals might be stricken".[56] These divergent assessments of the two historians probably reflect the ambivalence and complexity of the historical situation.

The concept of *syphilis insontium* could also provide a moral alibi to physicians who contracted the disease. Brandt writes: "Many physicians who became infected [..] suggested that they had received the contagion in the course of treating their patients - a possibility that today would be considered highly unlikely".[57] Leaving aside the question of the relative improbability of this mode of infection, we can infer that about the turn of the century German physicians had great difficulty to convince insurance companies of the reality and sincerity of *beruflicher Syphilisinfektion* (syphilitic infection contracted in the course of medical practice), from frequent complaints that those companies refused to pay compensation for alleged professional accidents involving syphilitic infection.[58] This illustrates once again the ambiguous status of the concept of *syphilis insontium*.

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[54] L.D. Bulkley, *Syphilis in the innocent*, New York, 1894.

[55] Brandt, op. cit. (note 51), p. 22.

[56] Cassel, op. cit. (note 48), p. 96.

[57] Brandt, op. cit. (note 51), p. 22.

[58] A. Neisser and M. Chotzen, 'Ersuchen an die deutschen Aerzte', *Berliner klinische Wochenschrift* 43 (1906), p. 464.

4. 'Hereditary syphilis': the sins of the fathers

In the *fin-de-siècle* climate prevailing at the end of the 19th century, syphilis was strongly associated with moral degeneration. It contributed to, but also reflected, an atmosphere of cultural pessimism and decadence during the Late-Victorian era.[59] No other disease, according to Fleck, was so much regarded as a cause of moral decay (176-77 note 25/102-103 footnote 25). The special stigma that was attached to syphilis resulted not merely from the fact that it was transmitted through sexual intercourse, but also from the fact that it was often passed on to the offspring, presumably along the route of heredity, as was assumed at the time. 'Hereditary syphilis' was a recognized and much-discussed item in medical journals and textbooks. In this section I will describe the cultural values and perceptions associated with this notion and analyze how it slowly and gradually gave way to our modern concept of 'congenital syphilis'.

Ibsen's Ghosts

The cultural image of 'hereditary syphilis' around the turn of the century can best be recovered through contemporary literature and drama. In the preface to a popular brochure on 'hereditary' diseases like syphilis and tuberculosis, Dr Stephan, medical director of the *Burgerziekenhuis* (Civilians' Hospital) in Amsterdam, wrote:

> "The doctrine of heredity has depressed many and brought them to a pessimism and fatalism, which has turned life into a burden from which death appears to be the only escape. Modern literature, informed by undoubtedly somewhat one-sided and certainly premature medical speculations, then got hold of the subject and furnished products so terrible and gruesome, that many people were driven into desperation and despair".[60]

Foremost among those "terrible and gruesome" literary products were the plays of the Norwegian dramatist Henrik Ibsen. He has influenced, as no other writer, his contemporaries' image of 'hereditary syphilis'.

The "connection between heredity and decadence" was a much-discussed theme around 1880 in the Scandinavian immigrant group in Rome to which Ibsen belonged.[61] They carefully studied Darwin's works. Ibsen was of the opinion that his contemporaries unduly

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59 W.F. Bynum, *Science and the Practice of Medicine in the Nineteenth Century*, Cambridge (Cambridge University Press), 1994, p. 221.

60 A. Reibmayr, *Het Immuniseerings-proces bij Erfelijke Ziekten* (translated and revised by Dr Stephan), Amsterdam (Scheltema & Holkema), 1908, pp. 5-6 [original German edition: A. Reibmayr, *Die Immunisirung der Familien bei erblichen Krankheiten (Tuberculose, Lues, Geistesstörungen): Ein Wort zur Beruhigung für Aerzte und Gebildete*, Leipzig and Vienna, 1899].

61 M. Meyer, 'Introduction' to H. Ibsen, *Ghosts* (translated by Michael Meyer), London (Eyre Methuen), 1973, p. 21.

neglected the significance of heredity. His notes dating from early 1881 show why he was occupied by this subject: "We raise monuments to the *dead*; because we feel a duty towards them; we allow lepers to marry; but their offspring - ? The unborn - ?".[62] Already in his play *A Doll's House* (also known as *Nora*), written in 1879, Ibsen had obliquely broached this theme. In this play Dr Rank, the family friend of Torvald and Nora Helmer, dies from the effects of a syphilis 'inherited' from his father - although the disease is not mentioned by name. To an old girlfriend Nora gives an explanation about their family friend: "But he's really very ill, poor man, he has consumption of the spine. The fact is, his father was a horrible man who had mistresses and that sort of things, so, you see, the son's been delicate all his life".[63] Somewhat later Dr Rank himself, in a cynical mood, declares to Nora: "Yet, indeed, the whole thing's nothing but a joke! My poor innocent spine must pay for my father's amusements as a gay young subaltern". After asking himself what justice there is in paying for someone else's sins, he observes on the basis of his practical knowledge as a physician: "Yet in one way or another there isn't a single family where some such inexorable retribution isn't being exacted".[64]

Ibsen's *A Doll's House* would cause some public uproar, but not because of this theme which was only incidentally treated. The reason was that Nora decided, at the end of the play, to leave her husband and children. She wanted to become an independent woman and no longer be her husband's doll. This urge towards female independence was too much for many of Ibsen's contemporaries.

In Ibsen's next play *Ghosts*, written in 1881, the theme of 'hereditary' syphilis was given a much more central and prominent place. Young Osvald Alving, next to his mother the main character of the play, heard mysterious biblical allusions when he consulted a physician for his complaints: "The sins of the fathers are visited on the children ...".[65] Just like Dr Rank, the innocent Osvald had to pay for the sinful life of his father, who, at the beginning of the play, appears to have died quite some time ago. At the end of the play the first signs of Osvald's insanity manifest themselves. Through this dramatic climax Ibsen managed to draw the public's attention much more emphatically to the theme of 'hereditary' syphilis. The response must have surpassed his worst expectations.

Although he had again not dared to mention the disease by name, "[w]hat was so offensive of Ibsen's *Ghosts* [..]", according to historian Peter Gay, "was that so many in his

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62 Ibid., p. 20.

63 H. Ibsen, *A Doll's House and Other Plays*, Harmondsworth (Penguin Books), 1987, pp. 183-84.

64 Ibid., p. 192.

65 H. Ibsen, *Ghosts and Other Plays*, Harmondsworth (Penguin Books), 1984, p. 74.

audiences knew precisely what he was talking about".[66] The piece aroused hostile and negative criticisms; Ibsen's publisher was forced to take back most copies from the book shops - in those days plays were first read as books; for the time being a dramatic performance was out of the question. In the year 1882, however, the play received its world première in Chicago, of all places, before an audience of Scandinavian immigrants. Now it started on a slow but steady European career. After performances in Stockholm, Christiana (Oslo), and Helsinki, the play was performed in Vienna and Amsterdam in 1890, followed the next year by a scandalizing première in London. Critics reacted violently, if not hysterically. In a leading article, the *Daily Telegraph* spoke of "an open drain"; *Era* called the piece "[a]s foul and filthy a concoction as has ever been allowed to disgrace the boards of an English theatre..". Many similar characterizations can be found in the anthology of extracts from press criticisms which *Pall Mall Gazette* had collected three weeks after the performance.[67] Only a single anonymous critic dared to defend the play: *Ghosts* was no "mere hospital ward play" but a "great spiritual drama".[68]

This same interpretative strategy of sublimation and spiritualization was followed by later critics and by Ibsen's biographer, Michael Meyer. The problem of 'hereditary' syphilis was ignored as much as possible. The full emphasis was shifted to the ethical problematics of the piece; and Osvald's mother (who had placed duty before love) instead of Osvald became its central figure. To quote the critic Halvdan Koht: "Osvald was branded with disease, not because his father was a beast, but because Mrs Alving had obeyed the immoral ethics of society [..] *Ghosts* is a play about ethical, not physical debility".[69]

Such attempts to sublimate the theme of Ibsen's play to the purely ethical aspects appear rather forced and artificial to me. For Ibsen and his contemporaries, I suppose, the play was about what could only be seen as both an ethical and a physical debility; the ethical debility could not be separated from its physical consequences, nor could the latter be abstracted from its moral meaning. Precisely the view of syphilis as both a moral and physical degeneration can explain its strong emotive connotations. It was this view that excellently fitted the general cultural pessimism of the *fin-de-siècle* era. After all, had the German educationist Dr Hermann Rohleder not pointed out that the progress of 'civilization' corresponded to the progress of 'syphilization'?.[70]

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66 P. Gay, *The Bourgeois Experience: Education of the Senses*, Oxford (Oxford University Press), 1985, p. 326.

67 Reprinted in B. Shaw, *The Quintessence of Ibsenism*, London (Constable and Company), 1913, pp. 87-90.

68 Meyer, op. cit. (note 61), p. 103.

69 Quoted in Meyer, op cit. (note 61), p. 21.

70 Quoted in Gay, op cit. (note 66), p. 326.

It is amusing to read the rather unhelpful attempt of Ibsen's biographer Meyer to defend the play against medical criticism:

> "In view of the oft-repeated complaint that syphilis cannot be inherited from one's father, it is worth pointing out that it can be inherited from one's mother, and that a woman can have syphilis without realizing it or suffering any particular discomfort. In other words, and this is a far more frightening explanation of Osvald's illness than the usual one, Mrs Alving could have caught syphilis from her husband and passed it on to her son. Dr Jonathan Miller has pointed out to me that Osvald could also have been infected by smoking his father's pipe. Ibsen knew more about medicine than some of his critics".[71]

Meyer has indeed heard something about the matter, but not quite enough. According to modern insights, syphilis is not inherited at all, neither from one's father nor from one's mother. A syphilitic mother can give birth to a luetic child, but this is not an example of inheritance but of (intra-uterine) infection. Still it remains true that a case of 'congenital' syphilis always presupposes a luetic mother. If Osvald had congenital syphilis, Mrs Alving *must* have had syphilis too. Finally, it appears doubtful to me whether Ibsen, by having Osvald smoke his father's pipe, wanted to suggest a possible route of infection. If so, it would not have been a case of inheritance! And it would have been too dramatic to cite Exodus on the sins of the fathers being visited upon the children.

Of course, it would be absurd to reproach Ibsen that he did not know what we know today.[72] Below we will see that medical science did not always draw such a clear-cut distinction between inheritance and infection as it does now. Indeed the possibility of a direct 'inheritance' of syphilis from the father, as against an exclusively maternal 'inheritance', was the object of intense medical debate around the turn of the century.

The imprint of Lamarckian heredity

Views on the inheritability of syphilis can be broadly characterized as Lamarckian. Lamarck, as is generally known, assumed the possibility of inheriting acquired characteristics.[73] It is not difficult to detect such views in medical treatises on syphilis from the second half of the 19th century. In a brochure from 1884 we can read for instance: "It must be accepted with certainty that acquired syphilis turned into inherited syphilis ..". A treatise from 1867 states: "[Syphilis clings] like the breath of pestilence to youth and beauty, fastening like an evergrowing and monstrous burden of sin onto a single lapse as well as poisoning the blood

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71 M. Meyer, *Ibsen*, Harmondsworth (Penguin Books), 1985, p. 514 (footnote).

72 What is implausible in Ibsen's play from a medical point of view (on the assumption that Osvald had congenital syphilis), is that the symptoms of insanity manifested themselves only at a relatively late date after an apparently vigorous and healthy youth. See J.B. Lyons, *Thrust Syphilis Down to Hell and Other Rejoyceana*, Dublin (The Glendale Press), 1988, p. 33.

73 For a useful discussion of (Neo-)Lamarckism, see P.J. Bowler, *Evolution: The History of an Idea*, Berkeley (University of California Press), 1984, pp. 243-52.

of unborn innocent children" (both passages quoted in Fleck: 176, note 25/102-103, footnote 25).

The last quotation suggests that an *Uridee* or proto-idea (to use one of Fleck's favourite concepts) of respectable biblical antiquity might also be echoed in Lamarckian views on heredity. Here is a direct allusion to Exodus 20: 5-6 in an older medical publication from 1846: "The innocent victims of syphilis are infinitely more numerous than the guilty; for it is a disease which follows vice down to the third and fourth generation ..".[74] A more recent example is found in a publication from 1901: "Now, if there is one conclusion to which we think experience surely leads us as medical men, it is that the sins of the father do tend to be visited upon the children even unto the third and fourth generation. We think we see this demonstrated day after day".[75] Here, appeal is made to old wisdom and daily experience to argue against those who, like August Weismann, denied the inheritance of acquired characteristics. It is no coincidence that Ibsen also took his inspiration from the Great Code of world literature when he worked up the theme of heredity offered by medical science. As we saw above, an unsuspecting Osvald heard from his physician: "The sins of the fathers are visited on the children ...".

According to Exodus, the sins of the fathers will be visited on the children "unto the third and fourth generation". In the medical literature of the late 19th century we find reports on cases of three successive generations suffering from syphilis (presumably furnishing proof for *two* succesive generations with inherited syphilis). Particularly suggestive were cases in which both mother and child exhibited typically deformed teeth, socalled Hutchinson's teeth, which were considered as specific *stigmata* of hereditary syphilis.[76] It was admitted that the empirical proof for inheritance of syphilis over more than one generation was not watertight because of the practical impossibility of excluding new infections in the second generation[77] but that was no reason for most physicians to doubt the reality of the phenomenon.

It is also possible to relate the cultural pessimism of the *fin-de-siècle* to the dominant ideas about heredity. The last 'heirs' of European culture - "the latecomers, those dull late offshoots of more vigorous and joyful generations", as Nietzsche characterized them[78] - were weighed down, as it were, by the heavy burden of sins of their numerous ancestors. This conviction was not without a scientific, or as we might prefer to say a pseudo-scientific,

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74 *Lancet* 1 (1846): 279; quoted in Walkowitz, op. cit. (note 11), p. 49.

75 J.G. Adami, 'On Theories of Inheritance with Special Reference to the Inheritance of Acquired Conditions in Man', reprint from the *New York Medical Journal* of June 1, 1901, pp. 6-7.

76 'Tot in het derde geslacht?', *Nederlandsch Tijdschrift voor Geneeskunde* 43 (1899) II, p. 1111.

77 E. Finger, 'Ueber die Nachkommenschaft der Hereditärsyphilitischen', *Wiener klinische Wochenschrift* 13 (1900): 383-89.

78 F. Nietzsche, *Unzeitgemässe Betrachtungen*, in *Sämtliche Werke, Kritische Studienausgabe in 15 Bänden*, Volume 1, Munich (Deutsche Taschenbuch Verlag/de Gruyter), 1980, p. 303.

foundation: "It is only with fairly advanced culture, as soon as natural selection is artificially tampered with, that one sees degenerative and hereditary processes occurring among domestic plants and animals and among men".[79] Hereditary diseases were thought to be an exclusive white man's burden, socalled *Naturvölker* were supposedly free from them. The advance of culture became almost synonymous to progressive degeneration. The progress of 'syphilization' was thus considered to be an integral part of the progress of civilization.

For some 19th-century medical writers the heredity of syphilis did not merely concern the transmissability of a specific condition to the offspring but also involved a more broad and diffuse pattern of inheritance according to which a general predisposition to a variety of 'constitutional' disorders could be transmitted. French physicians, who were particularly fond of the idea, called this broad pattern *l'hérédité syphilitique* in contradistinction to *la syphilis héréditaire*. Anglo-Saxon writers had less use for the theory and spoke of *occult syphilis*.[80] In 1900 the Austrian syphilologist Ernest Finger expressed this view as follows:

> "Hereditary-syphilitic children already exhibit, in addition to the manifestations of true syphilis, other symptoms, several forms of nutritive disturbances and developmental retardations. Flabby, delicate, grey-headed at birth, they remain badly nourished, backward in growth, the development of teeth and hair is insufficient, sexual maturity sets in hesitantly and insufficiently, nutritive disturbances and certain characteristic affections like [..] Hutchinson's trias occur [..]. *All these symptoms are also inherited independently, apart from the inheritance of syphilis as such*".[81] [Emphasis added].

Significantly, many of these symptoms were designated as the dystrophic *stigmata* of hereditary syphilis.[82] They were held to epitomize most graphically the degenerative influence of syphilis on future generations.

A variation on this theme was provided by Sigmund Freud in his case history of *Dora*. Freud held the opinion that "the offspring of syphilitics are particularly predisposed to severe neuropsychoses". "A remarkably high percentage of my psychoanalytically treated patients", he wrote, "comes from fathers who suffered from tabes or paralysis". He concluded that "syphilis of the father should certainly be considered as an aetiological factor in the neuropathic constitution of children".[83] Dora's 'hysteria' could therefore be partially

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79 Reibmayr, op. cit. (note 60), p. 26.

80 E. Lomax, 'Infantile Syphilis as an Example of Nineteenth Century Belief in the Inheritance of Acquired Characteristics', *Journal of the History of Medicine and Allied Sciences* 34 (1979): 23-39, pp. 31-33.

81 Finger, op. cit. (note 77), p. 384.

82 E. Fournier, 'Beitrag zum Studium der hereditären Syphilis in der zweiten Generation', *Wiener klinische Wochenschrift* 13 (1900): 985-89.

83 S. Freud, *Bruchstücke einer Hysterie-Analyse: Krankengeschichte der 'Dora'*, Frankfurt am Main (Fischer), 1981, p. 74 and pp. 23-24 (footnote).

explained from the frivolous life of her syphilitic father. Syphilis was thus supposed to lead to a more general hereditary 'taint'. It goes without saying that in this way the picture of heredity has become rather complicated and diffuse.

Paternal or maternal 'inheritance'

The breakthrough of more 'enlightened' views on heredity met with many obstacles in medical circles. Before 1900, August Weismann, the great crusader against Lamarckism, had already proclaimed that the case of syphilis did not exemplify "true inheritance", only infection by microbes.[84] Weismann's theory of the continuity of germ plasm proposed a rigorous distinction between germ cells and somatic cells; whereas the germ cells could influence the somatic cells, the vicissitudes of the latter could have no influence on the former. (The Central Dogma of molecular biology can be considered a modern version of Weismann's theory: information can pass from DNA to proteins, but not from proteins back to DNA.) A consequence of this theory was that the inheritance of acquired characteristics was impossible. For most physicians, however, Weismann's theory was either too difficult to understand or too speculative. Its esoteric details were intelligible to a few pathologists, but inaccessible to most clinicians.

Some pathologists, like John Adami and H. Beitzke, agreed with Weismann that it was absurd to speak of 'hereditary syphilis'.[85] Adami did not spare his medical colleagues in his criticism of current linguistic usage:

> "[..] it is necessary to lay down clearly what is not inheritance, for in medical writings and in ordinary medical parlance a terrible confusion prevails upon this point, and much that is certainly not inherited is commonly spoken of as being hereditary. There is, for example, no such thing as hereditary syphilis".[86]

We should not let ourselves be deceived, however, by this apparently firm rejection of the possibility of hereditary syphilis. Both Adami and Beitzke were convinced that Weismann had not been successful in blocking all possible loopholes for the inheritance of acquired characteristics. Adami found it implausible to assume that "the immature germ cells lie absolutely dormant in the organism".[87] These cells could be affected by the toxins or other metabolic products of the microbe responsible for syphilis. The results of such "damaged

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84 A. Weismann, *Das Keimplasma: Eine Theorie der Vererbung*, Jena (Gustav Fischer), 1892, p. 510.

85 "It must be simply obvious that it is wrong-headed to speak of inherited infectious diseases, e.g. hereditary syphilis"; H. Beitzke, 'Ueber Vererbung und Vererbbarkeit in der Pathologie', *Berliner klinische Wochenschrift* 42 (1905): 1156-58, p. 1156.

86 Adami, op. cit. (note 75), p. 12.

87 Ibid., p. 30.

germ plasm" (Beitzke) would show up in inferior offspring suffering from severe general disturbances, or "parasyphilitic lesions", as Adami called them:

> "[..] the offspring may show, not syphilitic lesions, but parasyphilitic lesions - various forms of arrested and imperfect development of different tissues due to the intoxication and therefore modification of the germ plasm while still a portion of the parental organism".[88]

By 'parasyphilitic lesions' Adami referred to phenomena that were also known as the manifestations of 'occult syphilis' or *l'hérédité syphilitique*.[89] His view was in agreement with the theory elaborated by the Austrian syphilologist Ernest Finger, who considered the general dystrophic disturbances of 'hereditary' syphilis as the effects of a 'depravation' of sperm or ovum caused by toxins of the syphilitic agent.[90] Finger had already suggested that these general dystrophic disturbances could be passed on to the progeny of syphilitic parents without 'syphilis as such' being transmitted. While the inheritability of 'syphilis as such' was increasingly questioned, Lamarckian heredity found a final refuge in the realm of 'occult' syphilis.

Meanwhile, shortly after the turn of the century, clinical syphilologists were engaged in a related but different debate. For them the issue was whether syphilis could be 'inherited' from (or transmitted by) the mother only or whether a direct 'inheritance' from the father (without first infecting the mother) was also a definite possibility. Most clinicians, among them the leading authorities of syphilology such as Alfred Fournier, Albert Neisser, and Ernest Finger, defended the possibility of paternal inheritance. A minority, headed by the Austrian clinician Rudolf Matzenauer, emphatically rejected this possibility and admitted placentary infection of the foetus or child *in utero* as the only possible mode of transmission. Although the semantics of 'inheritance' and 'infection' was not uniform between both parties, Virchow's definition could be used as a common ground: "Inheritance is realized through the act of conception. Whatever influences or modifies the embryo or foetus afterwards [..] has no claim to being called hereditary".[91] According to this definition a so-called *germinative infection* (from contaminated seed or ovum at the moment of conception) would be an instance of inheritance, a so-called *post-conceptional infection* would not. Of course, this does not correspond to modern usage. The net result of Matzenauer's attempts to exclude any

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88 Ibid.

89 Lomax, op. cit. (note 80), p. 36.

90 Finger, op. cit. (note 81), p. 384.

91 Cited in Beitzke, op. cit. (note 83), p. 1156; Alfred Fournier had offered a similar definition of heredity. See T. Broes van Dort, 'Review of A. Fournier, *Die Vererbung der Syphilis*, translated and revised by E. Finger, Leipzig and Vienna, 1892', *Nederlandsch Tijdschrift voor Geneeskunde* 36 (1892) II: 694-705.

possibility of inheritance or germinative infection, however, was that the old concept of 'hereditary syphilis' was transformed into the modern concept of 'congenital syphilis'.

Why did physicians believe in the paternal inheritance of syphilis in the first place? For one thing, many were familiar in their own private practice with cases of syphilitic fathers begetting syphilitic children (or stillborn foetuses), without the mothers showing any visible symptoms at all. The idea of paternal inheritance found further support in the remarkable fact, first observed in 1837 by the Irish surgeon Abraham Colles and therefore known as *Colles's Law*, that mothers suckling their luetic children never became infected, while wet nurses easily got ulcerations at their breasts. This apparent immunity of the mother was interpreted in either of two ways. According to some, it indicated that the foetus had communicated the disease, albeit in a mild and harmless form, to its mother (the socalled *choc en retour*). According to others, it indicated that the mother had only received from her foetus toxic products produced by the microbe but not the microbe itself.[92] Both interpretations assumed that the foetus had received the disease directly from its father. Later writers argued in an even more subtle way that a handful of reported cases of *exceptions* to Colles's Law furnished the most compelling proof for the existence of paternal inheritance: if the mother could still become infected when suckling her child, she must have been healthy before.[93]

In 1903, Rudolf Matzenauer launched an all-out attack on the supposed inheritability of syphilis in general and the idea of paternal inheritance in particular, which was followed by a heated controversy.[94] Matzenauer subjected alleged cases of paternal inheritance to a painstaking scrutiny; not a single case could withstand his criticism. There were actually no exceptions to Colles's Law. The immunity of mothers from their sucking luetic babies could be simply explained from the fact that these mothers themselves had latent syphilis. Matzenauer denied the very possibility of germinative infection, be it from a contaminated ovum or sperm cell. It would be unthinkable that a fertilized ovum, containing a microbe capable of such extreme ravages, could just develop further into an embryo and foetus.

Responding to Matzenauer's criticism, the adherents of paternal inheritance appealed once again to the many cases from their own professional experience in which women without visible signs of syphilis had given birth to luetic children or stillborn foetuses. They challenged Matzenauer to come forward with an adequate explanation for this remarkable fact. Simply sticking the label 'latent syphilis' would not do: "It is inadmissable to diagnose

92 Broes van Dort, op. cit. (note 91), pp. 699-701.

93 Ibid., p. 697; Hofrat Professor Neumann [without initials], 'Ueber Vererbung der Syphilis', *Wiener klinische Wochenschrift* 17 (1904): 551-58, p. 557.

94 R. Matzenauer, 'Die Vererbung der Syphilis. Ist eine paterne Vererbung erwiesen?', *Wiener klinische Wochenschrift* 16 (1903): 175-181; with ensuing discussions on pp. 229-36, 263-67, 292-96, 325-30, 361-68, and 392-98.

latent syphilis on the basis of existing immunity without proof of a previous infection".[95] This was a real point. However, the apparent absence of syphilitic symptoms in so many cases could also be partially explained from the fact that physicians, for reasons of prudence and discreetness, did not always thoroughly examine their female clients. As Elizabeth Lomax states: "The physician usually accepted a patient's statement that she had never suffered from a chancre or disseminated rash. Since respectable women simply did not confess to venereal disease, the concept of paternal transmission was strengthened further".[96]

Finally, the adherents of paternal inheritance put forward as an argument that the denial of this doctrine would bring in its train very evil social consequences. Neumann appealed to the French syphilologist Fournier, who had called "the denial of spermatic infection a dangerous view, because it would deprive syphilis from the greater part of its hereditary hazards and carelessly open the gates of marriage to a group of syphilitic men, to whom those gates had hitherto been closed for fear of injuring the offspring".[97] The almost circular character of this argument illustrates the tenacity with which physicians stuck to the idea of paternal inheritance. Were they still under the spell of the biblical *Uridee* that the sins of the *fathers* are visited on the children? After all, Exodus 20: 5-6 had been silent on the sins of the mothers.

Actually, professional interests too weighed in the balance when physicians showed themselves reluctant to reject the doctrine of paternal inheritance. Neumann asserted that the denial of paternal inheritance would require "the treatment of the assumed latent syphilis of the mother and the putting aside [*Ausserachtlassung*] of the treatment of the paternal syphilis". As a matter of fact, this is a most disingenious criticism (no one argued for not treating the father), but the reproach could be easily returned in mirror-image to the adherents of paternal inheritance. Their private practices were aimed primarily at treating *male* patients for syphilis. This was also conceded by Neumann, when he stated that the experience acquired in private practice was more relevant than experience in the maternity clinics for discussing paternal inheritance, because "in private practice above all syphilitic men come into observation".[98] It was quite normal for general practitioners to act in complicity with the husband to deceive the spouse. Ironically, Neumann himself nourishes the suspicion that married women might not have received much serious antiluetic therapy by pointing at the many cases of *exclusive* treatment of the husband resulting in subsequent births of healthy children as a massive empirical support for the doctrine of paternal inheritance. Clearly, this

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95 Neumann, op. cit. (note 93), p. 554.

96 Lomax, op. cit. (note 80), p. 31.

97 Neumann, op. cit. (note 76), p. 557.

98 Ibid., p. 552.

doctrine constituted a theoretical legitimation for a private practice that was primarily oriented to serving the medical needs of *male* syphilitics.

An unresolved issue in the controversy about paternal inheritance concerned the alleged 'latent syphilis' of the mothers of luetic children. The Wassermann reaction would contribute decisively to the solution of this problem. It turned out that most of these mothers without visible symptoms (90 to 95 %) reacted positively on this serological test.[99] The present-day view is that the foetus is infected by the mother through the placenta; a direct infection from the sperm can be considered virtually impossible. This does not mean that men are now exonerated from all blame, if only because it is often (but how often?) they who infected their wives in the first place. Paternal sin is no longer associated with the fatalism of heredity. The fatal chain of contagion can now be interrupted by the means of medical science.

5. Conclusions

In this chapter I have attempted to consolidate Fleck's constructivist approach towards the historical genesis of the concept of syphilis, drawing additional inspiration from Mary Hesse-'s finitist or network theory. Following Fleck, I have traced the shifting boundaries attributed to this disease concept during the four or five centuries of its history. Ironically, the discovery of the causative agent, *Treponema pallidum*, in 1905 did not end debate about the proper extension of syphilis but led to new complications. The re-definition of syphilis in aetiological terms ran up against the subsequent identification of the agents of *yaws* (1905) and later also of *pinta* (1938) which proved morphologically indistinguishable from *Treponema pallidum*. For a minority of medical investigators this circumstance provided a reason to count syphilis (including endemic forms such as bejel and njovera), yaws, and pinta as a single disease, but the majority did not want to give up the established distinctions. Neither the minority nor the majority decision, as I have argued in conformity with the finitist theory, can be seen as predetermined by the meaning the term 'syphilis' already possessed when it was confronted with these anomalies. Current usage of a concept does not determine future applications.

I have also followed up Fleck's suggestion that moral considerations in particular have entered into the construction of concepts of syphilis. For older notions like that of the carnal scourge or the empirical-therapeutic concept of syphilis (mercury treatment as atonement for sin!) this is clear enough, but it can also be demonstrated for more recent concepts such as 'hereditary syphilis' and 'syphilis insontium'. The example of the latter concept is very significant; as an explicitly moral category it aimed at an alleviation of the moral stigma

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[99] F. Bering, 'Welche Aufschlüsse gibt uns die Seroreaktion über das Colles-Baumèssche und das Profetasche Gesetz?', *Deutsche medizinische Wochenschrift* 36 (1910): 219-21; H. Rietschl, 'Das Vererbungsproblem der Syphilis', *Deutsche medizinische Wochenschrift* 36 (1910): 531.

attached to syphilis. The case of 'hereditary syphilis' shows that in the later part of the 19th century medical views on the heredity of the disease were closely intertwined with cultural perceptions of moral (and physical!) degeneration and of gender roles.

The limitations of the analyses presented in this chapter should not be overlooked. Due to the long stretch of history that had to be covered, I haven't always been able to do full justice to the tenets of symmetry and impartiality that are constitutive of the sociology of scientific knowledge. Sometimes, as in my description of the amalgamation and subsequent differentiation of the three venereal diseases, the story had a rather 'Whiggish' tenor. I think this is almost inevitable when the span of history to be dealt with is so extensive. A more thorough-going symmetrical analysis is only possible if the object of inquiry consists of a much briefer episode. (The next chapter on the discovery of the pale spirochaete discusses in depth the controversy over the aetiology of syphilis covering a period of no more than three years.) The broad historical scope of this chapter may also be responsible for certain unsought reminiscences of the old-fashioned history of ideas. Only in section 4 have I been able to connect the prevalent ideas of medical practitioners on paternal inheritance to their professional interests and the built-in 'gender bias' of their therapeutic practices. In later chapters more attention will be paid to the analysis of scientific and clinical practices and the role of professional interests.

Have I succeeded in demonstrating the 'culture-laden' character of the specifically *modern* concept of syphilis? I must admit that section 4 in particular is susceptible to a Whiggish reading. It is true - so the argument might go - that the ideas of hereditary syphilis and paternal inheritance testify that in the bad old days medical science was indeed caught up in cultural prejudices about moral degeneration and gender roles, but this is no longer the case today: the analysis in section 4 shows precisely that by sustained effort medical investigators were able to cut through the tangle of confusions and to break away from prejudice altogether. I must confess that I am somewhat at a loss for a convincing answer to this objection. The standpoint it expresses has been forcefully defended by Susan Sontag. In her book *Illnes as Metaphor* she asserts that only diseases which are poorly understood and therefore mysterious (like cancer in our time) can function as metaphors and symbols for what is felt to be socially and morally wrong; once such diseases are fully understood and become effectively treatable, the metaphors and symbols will wither away automatically.[100] In this view science occupies an Archimedean place outside the ambit of culture from where it can liberate us from the rule of metaphors. A contrasting view has been expressed by Owsei Temkin: "[..] neither as scientists nor physicians can we live outside the world of culture and morality".[101] Fleck would certainly have sided with Temkin. He

100 S. Sontag, *Illness as Metaphor - AIDS and its Metaphors*, Harmondsworth (Penguin Books), 1990, p. 62.

101 Temkin, op. cit. (note 6), p. 316.

would have considered it 'egocentric' to assume that the insights of the present time enjoy the unique privilege of being free from cultural admixtures while simultaneously granting that the medical views of the past have always been tainted with moral overtones.[102]

[102] The American epidemiologist-cum-anthropologist Robert Hahn offers an illuminating analysis of the current use of the category of so-called 'culture-bound syndromes' which touches on several points of the above discussion. Hahn notes the prevalent tendency to find such syndromes almost exclusively among non-western peoples: "Like the popular understanding of accents in speech, culture-bound syndromes are what other people have", to which he opposes his own view according to which "all syndromes are regarded as equally culture-bound, so that the concept of 'culture-bound syndrome' is itself not a useful distinction". See R.A. Hahn, *Sickness and Healing: An Anthropological Perspective*, New Haven and London (Yale University Press), 1995, p. 50 and p. 49.

CHAPTER IV

THE DISCOVERY OF THE PALE SPIROCHAETE

1. Introduction

In an early review of Ludwik Fleck's monograph on the *Genesis and Development of a Scientific Fact*, appearing in the German medical journal *Klinische Wochenschrift*, Hans Petersen accused him of adopting a position of extreme idealism by not recognizing the distinction between concept and object and conflating facts with our knowledge or interpretation of facts:

> "Reading this book, one gets the astonishing conviction that for Fleck no tangible distinction at all exists between fact and concept; discovery and invention; designation, linguistic expression, verbal or pictorial representation and what these refer to. Later he deals with a 'discovery', but what does he analyze? An *invention*, to wit the Wassermann reaction! Thus he reaches the conclusion that strictly speaking there are no facts and that discovery and invention are fundamentally the same".[1]

Similar charges are often brought against modern variants of constructivism. Contemporary sociologists of scientific knowledge are accused of treating solid scientific facts as mere 'constructions' or 'fabrications', thereby effectively turning them into 'artefacts'. It may be worthwhile, therefore, to discuss Petersen's criticism in some detail.

The scientific fact to which the title of Fleck's monograph alludes is "the fact that the so-called Wassermann reaction is related to syphilis" (XXVIII/2). I agree with Petersen that the choice of this example is rather unfortunate. We can easily recognize that the 'fact' chosen by Fleck is not a very suitable candidate if we slightly modify its formulation from

(1) The so-called Wassermann reaction is related to syphilis

to

(2) The Wassermann test for detecting syphilis is related to syphilis.

As the latter formulation brings out clearly, it is much more natural to consider the development of the Wassermann reaction as an invention rather than as a discovery of a fact. This, indeed, was also Petersen's view. Of course, all this does not exclude the possibility that a more thorough-going epistemological analysis might establish that discoveries should be

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[1] H. Petersen, 'Ludwig [sic] Flecks Lehre vom Denkstil und dem Denkkollektiv', *Klinische Wochenschrift* 15 (1936): 239-42, pp. 240-41.

viewed as inventions. To substantiate such an epistemological claim, however, one would need a more suitable example than the development of the Wassermann reaction. Such an example is provided by the discovery of *Spirochaeta pallida* (now *Treponema pallidum*) as the causative agent of syphilis. This case is also, albeit much more briefly, discussed in Fleck's monograph. In this chapter I will offer a detailed analysis along constructivist lines of this particular discovery.

Petersen also uses the example of the discovery of the pale spirochaete in syphilitic lesions to criticize Fleck's relativization of facts:

> "For Fleck too it is a fact that in 1905 Schaudinn discovered living entities, 'spirochaetes', in the fluid of certain lesions which he and others designated as syphilitic papules. He [Fleck] does not render the validity and reality of this event dependent on a particular thought style. Fleck will admit that one could have seen such spirochaetes in certain papules already in 1805, 1705, 1605, and 1505, if one had been in the possession of our means of investigation. This temporal regressus is not verifiable, but a comparable spatial displacement is - to India for instance [..] where indigenous physicians are completely indifferent toward these spirochaetes. For Fleck too it is therefore not dependent on a certain thought style that the presence of spirochaetes in papules of a particular complexion is a 'fact'. It is, however, style-dependent [*denkstilgebunden*] how one takes up this matter [*was man mit dieser Angelegenheit anfängt*]. It is wrong to designate as a 'discovery' that Schaudinn considers these structures [*Gebilde*] as causative agents [*Erreger*], because this implies a theory; already the generalization of the fact of the presence contains a theoretical element".[2]

I am convinced that Fleck would not have admitted anything of this sort! Putting aside the imaginary regressus into previous centuries as unverifiable, Fleck could have confidently taken up the challenge of a spatial displacement to India, provided that it would be *native* physicians and not Westerners who have to discover the spirochaetes in syphilitic lesions. From their alleged "indifference", reflecting a different thought style, it would be fairly safe to predict that they would not come up with the required discovery, even if they had been provided with all the necessary means of investigation. Moreover, Petersen's quest for 'bare' facts, uncontaminated by any style or theory, appears to reduce itself to absurdity. Can the (general) presence of spirochaetes in syphilitic papules be considered as a simple and straightforward fact, if this generalized formulation already implies - as Petersen himself admits - a theoretical element? What about the recognition of these microbes as 'spirochaetes', implying kinship relations with other species of spirochaetes? On Petersen's strict view, it would not even be allowed to speak of the discovery of the causative agent of syphilis, because the attribution of an aetiological status to a particular species of microbes involves a theory. Only the finding of a 'bare fact', not the proposal of a corresponding theoretical interpretation, counts as the real discovery. Claude Bernard has explicitly disavowed this

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2 Petersen, op. cit. (note 1), p. 241.

view.[3] It has also been repudiated in the actual history of the discovery of the pale spirochaete. After Schaudinn's untimely death in 1906, his 'co-discoverer' Erich Hoffmann unveiled a maquette with his effigy bearing the words: "Dem unvergesslichen Fritz Schaudinn, Entdecker der Syphilisspirochaete (Berlin, 3. März 1905), im Namen der deutschen Aerzteschaft". This official recognition of Schaudinn's discovery also extends to the assumed aetiological status of the pale spirochaete, contrary to Petersen's strictures.

Petersen concedes that for him too science is not a simple inventory of facts, but he denies that the latter are in any way conditioned by a particular thought style. In his view, a fact remains a fact whatever the dominant style. A thought style manifests itself only in the way in which different facts are related to each other. Using Fleck's example of astrological speculations in the early history of syphilis, Petersen asserts that we certainly recognize, as did people in the 16th century, the eruption of a peculiar epidemic in Naples in 1495 and the existence of a particular planetary constellation as facts; only the connection of those two facts betrays a particular thought style. Science is seen by Petersen as a stylized edifice erected on a foundation of facts which themselves are not contaminated by any style.

Although many of Petersen's specific objections against Fleck's views merit further scrutiny, it is easy to see that his general defence of the style-independent status of facts does not hold water. By granting that the *connections* between facts are style-dependent, he effectively gives away his position. To defend his view he has to establish that (purported) facts are 'atomic' in the sense that they cannot be further analyzed as consisting of other, more 'elementary' facts. This would seem to be an almost impossible task. At any rate, the condition does not hold for the two 'facts' of the above example.

According to Petersen, Fleck's conflation of discovery and invention stems from the general overestimation of the importance of experiments in biological research: "Here the confounding of discovery with invention is always close at hand: one discovers what one has fabricated oneself".[4] Echoing Goethe's aversion to the experimental torture of nature, Petersen breaks a lance for a morphologically oriented biology that will liberate itself as much as possible from "all special experimental conditions, destruction of the organ, fixation, staining, preparation" to eliminate the suspicion that "the entire theory is based on its own artefacts [*Kunstprodukte*]". His eschewal of experimental manipulation, weird as it is, makes sense in view of the fact that some modern constructivists like Latour and Woolgar and Knorr-Cetina have indeed based their non-realist reading of scientific knowledge on the

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3 "We usually give the name of discovery to recognition of a new fact; but I think that the idea connected with the discovered fact is what really constitutes the discovery [..]. Discovery, then, is a new idea emerging in connection with a fact found by chance or otherwise". See C. Bernard, *An Introduction to the Study of Experimental Medicine*, New York (Dover), 1957, pp. 34-35. Of course, this formulation still begs the question if one can meaningfully speak about a fact in isolation from the idea connected with it.

4 Petersen, op. cit. (note 1), p. 242.

thorough artificiality of the laboratory conditions in which such knowledge is ordinarily produced. Knorr-Cetina points out that all or most of the source materials with which scientists work are 'preconstructed' (e.g. specially bred assay rats, purified chemical substances obtained from industry, sterilized water running from a special faucet etc.): "[..] nowhere in the laboratory do we find the 'nature' or 'reality' which is so crucial to the descriptivist [=realist] interpretation".[5] In this connection, the realist philosopher Hans Radder speaks of the *Bachelardian challenge* to the realist interpretation of science (after Gaston Bachelard who already in the 1930s emphasized the productive character of scientific experimentation):

> "Simply stated, it is the question how scientific knowledge can be about a human-independent reality, if this reality is so thoroughly dependent on human work".[6]

If modern realists do not want to abandon the experimental, interventionist approach to nature, they will have to confront the 'Bachelardian challenge' head on. Petersen chose the easy way out by giving up experimental biology altogether. Undoubtedly he thereby gave too much away to his constructivist opponent.

2. The social construction of facts and artefacts

Many philosophers of science have criticized the relativist claims of contemporary sociologists of scientific knowledge. Here I will discuss in some detail the objections raised by the Dutch philosopher Anthony Derksen as a representative example of these criticisms.

Derksen addresses what he calls the "pseudo-problem of the sociology of science", i.e. whether or not scientific facts are merely 'social constructions'. By portraying scientific facts as the outcome of a consensus obtained through negotiation, sociologists of science like Harry Collins unjustly call the robustness of those facts into question and degrade them to the status of 'artefacts'. Or so Derksen maintains. Scientists will undoubtedly become involved in a social process to convince each other of the reliability or otherwise of experimental results and interpretations. This process should not, Derksen holds, be described as 'negotiating' or 'argle-bargle'. The reason is that this social process amounts to an exchange of arguments in which "ultimately norms and criteria with a good record turn out to be decisive".[7]

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5 K.D. Knorr-Cetina, 'Towards a Constructivist Interpretation of Science', in K.D. Knorr-Cetina and M. Mulkay (eds.), *Science Observed*, London (Sage), 1983, pp. 115-140, on p. 119. See also K.D. Knorr-Cetina, *The Manufacture of Knowledge*, Oxford (Pergamon Press), 1981, pp. 3-4.

6 H. Radder, 'Science, Realization and Reality: The Fundamental Issues', *Studies in History and Philosophy of Science* 24 (1993): 327-49, on p. 328.

7 A.A. Derksen, *Wetenschap of willekeur*, Muiderberg (Coutinho), 1992, p. 159.

Derksen has also found an ambiguity in the use of the expression 'scientific fact': "Two elements are involved in this [notion]: its acceptance and its being a (real) fact (the being-constituted-such-and-such of reality)". For convenience we could designate this conception as the *two-in-one* view of a scientific fact. Sociologists of scientific knowledge, by contrast, adhere to a one-dimensional conception of scientific facts: "In the alternative usage of the expression 'scientific fact' it is sufficient that the scientific community accepts something as a fact. Here a scientific fact is not necessarily a (real) fact". After introducing the distinction between these two conceptions, however, Derksen immediately retracts the second one as a legitimate possibility, for he continues: "The difference between scientific views and scientific facts thus disappears".[8] In other words, the adherents of the second conception do not 'in fact' talk about 'scientific facts' at all! Exit second conception. Through a simple terminological analysis Derksen has gained a victory over his constructivist opponents.

Indeed, the victory may have been somewhat too easy. A sociologist of science like Harry Collins will readily admit, I presume, that he does not subscribe to Derksen's 'two-in-one' definition of scientific facts. The reason is simple. In studying the pursuits of scientists, the sociologist of science does not want to saddle himself with the additional obligation to find out about natural reality being such-and-such or perhaps so-and-so (the latter aim is precisely the point of the game of the scientists who constitute the object of his sociological inquiry!). All realist claims about nature are bracketed. For a student of science, in contradistinction to the natural scientists themselves, this self-imposed limitation is quite sensible. It does not preclude making significant discoveries *about* science. Although Derksen maintains that in the second conception the difference between scientific views and scientific facts disappears (which, from his perspective and terminology, is a correct observation), the sociologist of science still can make significant distinctions between a view that is accepted only in laboratory X and a view that is also accepted in laboratories Y, Z, A, B, C etcetera. Once a view has gained such a wide acceptance, it may happen that it will be taken by the natural scientists involved as the formulation of a 'real fact'. The relativist approach has a methodological advantage. As students of science, sociologists are always dealing with 'views' of scientists, whether or not such 'views' are given the status of a 'fact'. Only from this point of view will it be possible to inquire why some views held within the scientific community will acquire a factual status.

The Collinsian approach to the study of scientific facts might be described as *agnostic*: all realist claims with regard to natural reality are radically put within brackets. Among the believers (i.e. rationalist and realist philosophers of science) such an agnostic stand is considered offensive and easily mistaken for a form of *atheism*. Hence the oft-repeated accusation that constructivists portray solid scientific facts as *mere* 'social constructions' or 'arte-

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8 Derksen, op. cit. (note 7), p. 162.

facts'. In return, sociologists of science like Andrew Pickering have protested against the philosophers' use of the qualifier 'mere' to dismiss the constructivist approach.[9]

If Derksen, as a philosopher, is to put his two-in-one conception of scientific facts to profitable use, he will need a privileged access to natural reality in order to be able to decide for himself whether it is constituted such-and-such or perhaps so-and-so. If he does not dispose of such an independent access, then in his search for 'real facts' he will necessarily have to fall back on what are accepted as facts within the scientific community - that is, on 'views'. Practically speaking, the difference between (mere) 'views' and (real) 'facts' then also disappears for Derksen. His two-in-one conception thus collapses into the detested alternative conception according to which scientific facts are 'merely' what is accepted as such within the scientific community.[10]

Derksen still holds one last trump, his 'good reasons' or 'good arguments':

> "If after negotiation it is accepted within science that such-and-such is the case, then this happens because to the community's judgement it is a real fact that such-and-such is the case. The scientific community therefore requires *good arguments for the acceptance* of a candidate-fact. I have already argued that in the course of scientific development it has learned, and continues to learn, which arguments are good ones. Given the quality of these arguments we may assume with reason that what we take to be scientific facts are also (real) facts".[11]

In accordance with his *naturalistic epistemology*, Derksen does not supply us with a General Methodology specifying what are 'good reasons' and 'good arguments'. This makes it difficult to assess Derksen's claim that the *actual* 'reasons' used in science offer us sufficient ground for holding scientific facts to be 'real' facts. Let us take, for example, Koch's postulates within microbiology. Do these in general constitute 'good reasons' for recognizing a certain microbe as the causative agent of a particular disease? Presumably yes. But how should we judge those instances in which Koch's postulates are departed from, as happened with the acceptance of *Spirochaeta pallida* as the aetiological agent of syphilis? Are there

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9 A. Pickering, 'Knowledge, Practice and Mere Construction', *Social Studies of Science* 20 (1990): 682-729.

10 There is an interesting parallel here with a fundamental debate within the sociology of scientific knowledge. Harry Collins and Steven Yearley object to the so-called 'extended symmetry principle' (which grants 'agency' to non-human organisms and things like scallops and doors) of their French colleagues Michel Callon and Bruno Latour, precisely on the grounds that sociologists do not have independent access to the world of non-human organisms and things. That means that in practice sociologists will have to fall back selectively on scientists' reports about the behaviour of organisms and things. The attempt to extend the symmetry principle will thus, paradoxically, reinstate the old asymmetric approach. See H.M. Collins and S. Yearley, 'Epistemological Chicken', in: A. Pickering (ed.), *Science as Practice and Culture*, Chicago and London (University of Chicago Press), 1992, pp. 301-26.

11 Derksen, op. cit. (note 7), p. 164.

also 'good reasons' for such a departure? Presumably also yes, because science itself finds out what are 'good reasons'. Derksen's naturalism threatens to boil down to the complacent position that science (almost) always knows best. Of course, the sociology of scientific knowledge also adopts a naturalistic approach. Here, however, all rhetoric of 'good reasons' and realist claims with regard to natural reality are deliberately avoided.

After having disposed of the "pseudo-problem of the sociology of knowledge", i.e. whether or not scientific facts are merely 'artefacts' or 'social constructions', Derksen broaches the interesting question of (real?) *artefacts* in the context of his discussion of theory-ladenness of observation. Is what we observe through a microscope a product of the apparatus, or does it inform us about reality? Does what we see teach us something about the living cell, or only about the mutilations which we bring about in subjecting the latter to a drastic preparation? Because of the use of staining techniques, one could say in a most literal sense: "Our observation is coloured, and in our own favour".[12]

Derksen holds the view that a philosopher of science cannot make a lot of this problem. Whether or not we are dealing with an 'artefact' in our observations, constitutes indeed a recurrent and important problem for science. Such a problem is however solved by science itself through developing new methods and techniques and independent controls. Derksen holds that the problem of the theory-ladenness of observation and the role of theory has been grossly exaggerated by philosophers. For a philosopher his general conclusion must be somewhat disappointing.[13]

To the sociology of scientific knowledge, by contrast, the problem mentioned by Derksen might present an interesting challenge, to wit, showing not just the social construction of 'facts' but also the social (de-)construction of 'artefacts' within one single analytical framework.[14] The genesis of 'the fact that *Spirochaeta pallida* (or *Treponema pallidum*) is the causative agent of syphilis' appears to be a suitable example because the problem of coloured observation in a literal sense has figured prominently in this particular case.

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12 Derksen, op. cit. (note 7), p. 165.

13 Ian Hacking, in his book *Representing and Intervening*, devotes a complete chapter to 'Microscopes' (Chapter 11). He also objects to a theory-dominated philosophy of science: "The experimental life of microscopy uses non-theory to sort out artifacts from the real thing", I. Hacking, *Representing and Intervening*, Cambridge (Cambridge University Press), 1986, p. 200.

14 See for a similar attempt N. Rasmussen, 'Facts, Artifacts, and Mesosomes: Practicing Epistemology with the Electron Microscope', *Studies in History and Philosophy of Science* 24 (1993): 227-65.

3. The discovery: prelude and aftermath

In the bacteriological era, the idea that infectious diseases were caused by specific pathogenic agents was confirmed by an apparently interminable series of discoveries.

Already in 1879, the aetiological agent of gonorrhoea had been discovered by the then 23-year-old assistant to the Breslau dermatological clinic, Albert Neisser. The microbe responsible for another venereal disease, chancroid or ulcus molle, was identified in 1889 by Augusto Ducrey. Both identifications were followed by pure cultures of the causative organisms. These discoveries reinforced the separation of syphilis from the other two venereal diseases. Because of these and a host of other bacteriological achievements it was understandable that around the turn of the century the medical world anxiously looked forward to the discovery of the causative agent of syphilis. It is for this reason that Fleck can assert with some exaggeration: "The discovery of the causative agent of syphilis is actually to be attributed mainly to bacteriologists active in other fields" (15/24). According to Fleck, in this *bakterienlustige Epoche* or "era when bacteria were 'popular'" (15/24) the ground was well-prepared for the discovery of the pale spirochaete as the causative agent of syphilis. As we will see, however, this fails to do full justice to the fact that it was not so much the older field of *bacteriology* but the more recent field of *protozoology* (or *Protistenkunde*, 'protistology') that made a decisive contribution to this particular discovery.

For quite some time, then, there existed a vacancy for the function of 'aetiological agent of syphilis'. In the decades before 1905 many putative causative organisms were proposed to fill this vacancy; none of those candidates, however, possessed sufficiently strong credentials to avoid an eventual return to the "latency stage of oblivion".[15]

The artificial transmission of syphilis to apes and monkeys, first accomplished with chimpanzees in 1903 by Elie Metchnikoff and Emile Roux of the *Institut Pasteur* in Paris, raised new hopes for the detection of the causative agent. The German dermatologist Albert Neisser was among those who eagerly seized upon the new opportunities for experimental syphilis research. In the beginning of 1905 Neisser organized a scientific expedition to the Dutch East Indies to continue his experiments with primates on a much larger scale and under more congenial tropical conditions. The avowed aim of his expedition was to find an immunization method against syphilis. According to his biographer, Sigrid Schmitz, Neisser also entertained the secret hope that his expedition would lead to the elucidation of the aetiology of syphilis, or, in other words, to the discovery of the causative agent.[16] But things would run differently.

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[15] O. Lassar, 'Ueber neuere Protozoën-Befunde', *Berliner klinische Wochenschrift* 42 (1905): 728-30, p. 730.

[16] S. Schmitz, *Albert Neisser: Leben und Werk auf Grund neuer, unveröffentlichter Quellen*, Düsseldorf (Michael Triltsch Verlag), 1967, p. 48. For more on Neisser's research on apes and monkeys, see Chapter V.

On 8 February 1905 a young dermatologist, Erich Hoffmann, assistant to the dermatological clinic of the Royal Charité Hospital in Berlin, published a review article on the results so far obtained in the experimental research on primates. The article contains the following passage on the prospects of Neisser's expedition, which in the light of subsequent events makes for curious reading:

> "That is why we should hail Neisser's decision to continue his promising experiments on the Sunda Isles with great joy. May it be granted to the great Breslau dermatologist, whom we owe the discovery of the causative agent of the most frequent venereal disease, gonorrhoea, also to lighten the darkness of the aetiology and pathogenesis of syphilis, to the glory of himself and of German science and to the welfare of suffering humanity".[17]

At the time of publication, Hoffmann could not yet know that only a few weeks later he would be assigned by his boss, Professor Edmund Lesser, as a clinical assistant to the team led by the protozoologist Fritz Schaudinn which was to inquire into the possible cause of syphilis (cf. Fleck: 16/24). Thanks to this involvement, Hoffmann would eventually gain the reputation of being the 'co-discoverer', with Schaudinn, of *Spirochaeta pallida*. While Neisser and his collaborators were busily experimenting with primates in the Dutch East Indies (on Java, not on the Sunda Isles), the darkness of the aetiology of syphilis was lightened in Berlin.

But how did Schaudinn become involved? Why was a significant contribution expected to come from a protozoologist? To answer these questions, I have to devote a few words to the emergence of the field of protozoology.[18]

The groundwork for this new field had been laid in 1876 by Otto Bütschli when he constructed the vast new kingdom of 'Protozoa' as a special category of unicellular organisms. In Germany, the study of these organisms became a specialized branch of zoology. Well-known authorities in this area, in addition to Bütschli, included Richard Hertwig, Franz Eilhard Schulze, Franz Doflein, Fritz Schaudinn, and Richard Goldschmidt. In 1902 a specialized professional journal, *Archiv für Protistenkunde*, was launched. Around the turn of the century medical interest in protozoology was also increasing. In 1903, Robert Koch's Institute for Infectious Diseases in Berlin added a division for protozoology. The following year the Imperial Health Office (*Kaiserliches Gesundheitsamt*) in Berlin also instituted, at Koch's instigation, a special protozoology laboratory which was to be directed by Schaudinn. In 1902 and 1907, respectively, Ronald Ross and Alphonse Laveran received the Nobel Prizes for Medicine or Physiology for their work on malaria parasites and the role of insect

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17 E. Hoffmann, 'Die Bedeutung der neueren Versuche, Syphilis auf Tiere zu übertragen', *Berliner klinische Wochenschrift* 42 (1905), p. 156.

18 The *Journal of the History of Biology*, Vol. 22, no. 2 (Summer 1989) is a special issue devoted to the history of protozoology. I found the contributions of Natasha Jacobs (215-42), Marsha Richmond (243-76), and John Corliss (307-23) particularly enlightening and informative.

vectors, and other pathogenic protozoa. During the heyday of colonialism, there was much interest in protozoa as possible aetiological agents of various tropical diseases, especially in the *trypanosomes* held to be responsible for such conditions as Nagana and sleeping sickness. In this connection it is also important to point out that Paul Ehrlich's chemotherapeutic research programme, initiated after he became director of the Institute of Experimental Therapy in Frankfort in 1899, was originally oriented toward trypanosomal diseases.[19] The reorientation of this programme toward syphilis occurred later and was linked with Schaudinn and Hoffmann's discovery (see Chapter VII).

As a protozoologist, Schaudinn had been working on coccidians, malaria parasites, rhizopods, trypanosomes and spirochaetes. He had a good professional reputation.[20] Some of his contributions, however, were highly controversial. Just before his involvement in the search for the syphilis agent, Schaudinn had developed a speculative theory which stated that bird malaria parasites, trypanosomes and spirochaetes were merely different developmental forms of one single type of organism. This theory was 'exploded' by the American bacteriologists Frederick Novy and Ward McNeal, who showed that Schaudinn's interpretations were based on mixed infections. Schaudinn, however, stuck to his theory. With his usual dramatization, Paul de Kruif wrote about the consequences of Schaudinn's 'erroneous' view: "It might have wrecked him ... scientifically. Yet strangely, it got him ready for the famous day of March 3rd, 1905, that made him immortal".[21] On that day Schaudinn first discerned the pale spirochaete through his microscope in a syphilitic papule freshly excised by Hoffmann.

But, again, how did he become involved in the first place? In 1904 and 1905, another protozoologist, John Siegel, working at the Zoological Institute of Professor Franz Eilhard Schulze (who was also Schaudinn's former teacher!) in Berlin, claimed to have found protozoal agents of a number of acute exanthemas: smallpox, cowpox, foot-and-mouth disease, scarlatina, and syphilis. All these alleged aetiological agents were said to belong to

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19 P. Ehrlich and K. Shiga, 'Farbentherapeutische Versuche bei Trypanosomenerkrankungen', *Berliner klinische Wochenschrift* 41 (1904): 329-32, 362-65; P. Ehrlich, 'Chemotherapeutische Trypanosomen-Studien', *Berliner klinische Wochenschrift* 44 (1907): 233-36, 280-83, 310-14, 341-44.

20 On 17 March 1905, when Schaudinn and Hoffmann were already on the trail of the pale spirochaete, the German zoologist Professor J.W. Spengel wrote a letter to Elie Metchnikoff, Pasteur's successsor and a former zoologist, asking to support Schaudinn's candidacy for the nomination of the Nobel Prize for Medicine or Physiology. Spengel motivated his request in the following way: "It appears to me that the entire modern research on protozoa and especially on pathogenic protozoa rests on the investigations which were so brilliantly started with Schaudinn's contribution on the propagation of coccidians and which he later extended in such an excellent way to malaria parasites and rhizopods and finally to trypanosomes and spirochaetes". Metchnikoff replied that he could not support any other candidate as long as Robert Koch had not received the coveted Nobel Prize (which he duly received in 1905!). For Spengel's letter and Metchnikoff's answer, see H. Zeiss, *Elias Metchnikow: Leben und Werk*, Jena, 1932, p. 174.

21 P. de Kruif, *Men against Death*, Hamburg/Paris/Bologna (Albatross), 1934, p. 192. De Kruif had received his scientific training from Frederick Novy.

the class of Flagellates. The supposed causative agent of syphilis was designated as *Cytorrhyctes luis*.[22] In view of the importance that could be attached to the latter finding, if true, the president of the Imperial Health Office called upon Schaudinn to verify the results of his colleague. With many misgivings about his bureaucratic obligation to check up on another man's work instead of doing original research of his own, Schaudinn arranged cooperation with the Royal University Clinic for Skin and Venereal Diseases. The clinic was to provide pathological material from syphilitic patients. Staff surgeon Erich Hoffmann took charge of the clinical side of the cooperation. Schaudinn's collaborators from the Health Office, Neufeld and Gonder, were also involved.

Schaudinn was not able to confirm the findings of John Siegel, but already on 3 March 1905 he discerned, in a freshly excised secondary syphilitic papule supplied by Hoffmann, a live, motile, corkscrew-like organism of extreme transparence which he immediately recognized as a spirochaete. Schaudinn called Hoffmann and the others to have a look, but at first they were unable to see anything. For the next few weeks Schaudinn and Hoffmann continued their investigations with new pathological material. In these fresh cases spirochaetes could also be found, but again with great difficulty. Attempts were undertaken to stain the spirochaetes in fixed preparations - which finally succeeded to some extent with a so-called *Giemsa solution* (an eosin-azure mixture developed by G. Giemsa), but only after staining for 24 hours. Because of this difficulty in staining the microbe received the baptismal name of *Spirochaeta pallida* or pale spirochaete.[23] By 21 March 1905 Schaudinn and Hoffmann were convinced of the likely aetiological role of *Spirochaeta pallida*. They also thought that this species could be regularly distinguished from other kinds of spirochaetes, in particular from a coarser type found in papillomas and balanitis which was dubbed *Spirochaeta refringens*.

Writing about Schaudinn, Paul de Kruif describes the difference in attitude on two dates, 3 and 21 March 1905, as follows:

> "So, on the very first day on the very first case on the 3rd of March he'd seen the pale spirochaetes. But this day, Sunday, March 21st, he knew he'd *discovered* the pale horror at the bottom of syphilis".[24]

This raises the question on which date the causative agent of syphilis has been discovered. In official histories the date is usually set at 3 March 1905. Should it perhaps be shifted to 21 March? Or should the discovery be located at an even later date, taking the *recognition* by the scientific community as the relevant mark? And who should be credited as being the

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22 A summary of Siegel's findings is given in the review written by Oscar Lassar, op. cit. (note 15), pp. 728-29.

23 At first the name Spirochaete pallida was used; later on this was changed into Spirochaeta pallida. Throughout this chapter I have used the latter name.

24 De Kruif, op. cit. (note 21), p. 200.

'discoverer'? Only Schaudinn? Or was he, as Fleck maintains, "rather a standard-bearer in discovery than its sole agent" (42/37)?

The first occasion for the scientific community to pass its judgement on the presumed discovery of the aetiological agent of syphilis was on 17 and 24 May 1905, when Schaudinn and Hoffmann presented their findings before the Berlin Medical Society. By this time they had already received confirmation from Elie Metchnikoff in Paris, who, on Schaudinn's request, had searched for pale spirochaetes in the syphilitic lesions of artificially infected apes and monkeys.[25] Two German investigators, A. Buschke and W. Fischer, had also found spirochaetes in the liver and spleen of a stillborn syphilitic child. They had their find authenticated by Schaudinn and Hoffmann: "A preparation was immediately submitted for assessment to Messrs. Schaudinn and Hoffmann, who declared that the spirochaetes we found resemble those found by them in morphological and tinctorial respects".[26] Although the pale spirochaete had by now been encountered in 7 primary lesions, 9 secondary papules, 12 typically affected inguinal glands, in the liver and spleen of a child with congenital lues, and in syphilitic lesions of primates, Schaudinn and Hoffmann were still very reticent to pronounce on the aetiological status of the pale spirochaete. As Hoffmann stated: "[..] also today we are still far from passing already a definitive judgement on the aetiological significance of this particular hitherto unknown microbe".[27] In fact, this caution had been imposed by the President of the Imperial Health Office.[28]

This imposed reticence did not however prevent a direct confrontation, before the forum of the Berlin Medical Society, between Schaudinn and Hoffmann, on the one hand, and two staff members of Professor Franz Eilhard Schulze's Zoological Institute, Dr. Thesing and Walter Schulze, on the other hand. The latter two supported John Siegel's *Cytorrhyctes luis* as a rival candidate for the function of 'causative agent of syphilis' and did everything they could to question the credentials of the pale spirochaete. Thesing maintained that the pale spirochaetes shown in Schaudinn's preparations did not come from syphilitic tissue but were actually brought in with contaminated Giemsa stain:

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25 See Schaudinn's letters to Metchnikoff, 2 and 8 May 1905, reprinted in Zeiss, op. cit. (note 19), p. 173.

26 A. Buschke and W. Fischer, 'Ueber das Vorkommen von Spirochäten in inneren Organen eines syphilitischen Kindes', *Deutsche medizinische Wochenschrift* 31 (1905): 791-92, p. 792. It would thus be too much to say that their find constituted *independent* confirmation of Schaudinn and Hoffmann's findings.

27 F. Schaudinn and E. Hoffmann, 'Ueber Spirochaete pallida bei Syphilis und die Unterschiede dieser Form gegenüber anderen Arten dieser Gattung', *Berliner klinische Wochenschrift* 42 (1905): 673-75, p. 675.

28 A. Schuberg and H. Schlossberger, 'Zum 25. Jahrestag der Entdeckung der Spirochaete pallida', *Klinische Wochenschrift* 9 (1930): 582-86, p. 583.

> "Giemsa stain is often mixed with dextrin and, as experience shows, offers a favourable nutritive medium to numerous microorganisms, among others to cocci, bacteria, and also to spirochaetes. Therefore, the suspicion cannot be dismissed that the demonstrated syphilis spirochaetes come for the most part not from the tissue but from the dyes".[29]

Thesing himself showed photographs of object-glasses treated with Giemsa stain (without preparations from pathological material!) which exhibited a certain resemblance to Schaudinn's photographs of pale spirochaetes. In Thesing's opinion, Schaudinn's preparations could therefore be dismissed as 'artefacts', representing a clear case of coloured (or stained) observation.

Another objection was directed at the alleged protozoal nature of *Spirochaeta pallida*. According to Thesing, there was nothing to support this assumption: no nuclei, no undulative membranes and no flagella were visible. In his view, spirochaetes had to be classified as belonging to the bacteria.

Finally, the differentiation between the pale spirochaete and other species of spirochaetes was also questioned. Schaudinn had pointed at the small size and delicacy, the number, steepness, and rigidness of coils, and the difficulty of staining as specific characteristics of *Spirochaeta pallida* distinguishing it from other spirochaetes:

> "If one has imprinted the characteristic image of this spiral in one's mind, then, in my opinion, one will always easily recognize this form again".[30]

The opponents of the pale spirochaete called into question the very idea that there was a constant and characteristic form of this putative species.

The controversy between the adherents of the two rival candidates for the aetiological agent of syphilis encouraged a sceptical attitude among other members of the medical community. At the Berlin meeting old Oscar Lassar, a died-in-the-wool dermatologist, remarked that in the past 25 years exactly 25 different syphilis agents had been proposed. And the Chairman, Excellenz von Bergmann, concluded the meeting with the words:

> "Herewith the debate is closed until yet another syphilis agent will claim our attention".[31]

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29 'Berliner medizinische Gesellschaft: Sitzung vom 17. Mai 1905', *Berliner klinische Wochenschrift* 42 (1905): 694.

30 Schaudinn and Hoffmann, op. cit. (note 27), pp. 673-74.

31 'Berliner medizinische Gesellschaft: Sitzung vom 24. Mai 1905', *Berliner klinische Wochenschrift* 42 (1905): 731-34, p. 734.

If a purported discovery requires *recognition* by the relevant scientific community to be considered a genuine discovery[32], then even by 24 May 1905 the causative agent of syphilis could not be said to have been 'discovered' already. During the following months of that year, however, events would take a more favourable turn for Schaudinn and Hoffmann.

In June 1905, Thesing's charge that the pale spirochaetes in Schaudinn's preparations derived from Giemsa's stain rather than from syphilitic tissue aroused an angry reaction from G. Giemsa. He first accused Thesing of having maltreated his stain by mixing it with dextrin. Giemsa also expressed his conviction that the socalled 'spirochaetes' shown by Thesing at the meeting of the Berlin Medical Society were nothing else than tiny crystals of methylene blue and methylene azure which had precipitated during the drying of the Giemsa solution: "These [crystals] can, as anyone can convince himself most easily, delusively suggest the most beautiful bacterial flora, but they will be immediately resolved when the preparation is washed off with water in the usual manner".[33] Now it was the turn of Thesing's 'spirochaetes', rather than those of Schaudinn, to be exposed as artefacts.

In the fall of 1905, *Spirochaeta pallida* clearly had the edge over its rival, *Cytorrhyctes luis*. An official report released on 12 August 1905, which is also cited by Fleck, declared that spirochaetes had already been found "by more than a hundred authors in the most diverse products of syphilis" (16 / 25). Such a large number of confirmations carried weight and could not fail to tip the balance in favour of the pale spirochaete. Still, the opposition, organized from the Zoological Institute in Berlin, did not give in. The adherents of *Cytorrhyctes luis* seized upon what constituted *Spirochaeta pallida*'s heel of Achilles: its low visibility. Artificial means of visualization had to be used to make the microbe (more) visible. When Giemsa staining was followed by the more powerful silver impregnation method, the opponents of the pale spirochaete did not hesitate to denounce the results obtained with the new method again as artefacts. They thus opened a new round in the debate on coloured (stained) observation and artefacts. In Section 5 I will deal with this debate in more detail. First, however, I will discuss Fleck's views on the discovery of the causative agent of syphilis in the next Section.

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32 Cf.: "Discoveries occur because they are made to occur socially by processes of social recognition", A. Brannigan, *The Social Basis of Scientific Discoveries*, Cambridge (Cambridge University Press), 1981, p. 169.

33 G. Giemsa, 'Bemerkungen zur Färbung der Spirochaeta pallida (Schaudinn)', *Deutsche medizinische Wochenschrift* 31 (1905): 1026-27, p. 1027.

4. Fleck's views on the discovery of the pale spirochaete

According to Fleck, "[t]he discovery of the causative agent, *Spirochaeta pallida*, was the result of steady, systematic work by civil servants" (15-16/24). In his view, the title 'discoverer of the syphilis agent' should properly be awarded to the "team of civil servants" that "carried out its work and judged its own results in [a] careful, rational, and conscientious manner" (16-17/25). He also referred to the Health Office as *der eigentliche Entdecker* (German: 25). Schaudinn is seen as no more than "a standard-bearer in discovery"; he "personified the excellent team of health officials whose work [..] cannot easily be dissected for individual contribution" (41-42/57). Here as elsewhere in his work Fleck assumes an "extremely anti-individualistic standpoint".[34] Can it be sustained in this particular case?

To support his view that the discovery of the pale spirochaete was a collective achievement, Fleck relies heavily on an official report to the Secretary of State of the Interior, which stated that to see in *Spirochaeta pallida* the causative agent of syphilis is "a not unjustified conclusion" (16/25). Such an account is of course produced for bureaucratic reasons and does not necessarily reflect the points of view of those involved. We have already seen that the initial reticence of Schaudinn and Hoffmann with regard to the aetiological significance of the pale spirochaete was actually *imposed* on them. Fleck appears to have taken the bureaucratic manner of *reporting*, with its typical stress on the "careful, rational, and conscientious" procedure, as directly reflecting the character of the discovery process itself. It is illuminating to contrast Fleck's account with the reminiscences of Richard Goldschmidt, a protozoologist and geneticist who had cooperated with Schaudinn at the zoological station in Rovigno on the Adriatic Coast, before the latter was called back to Germany. To judge from the portrait sketched by Goldschmidt, Schaudinn was by no means a bureaucratic personality. Goldschmidt describes Schaudinn's attitude to the Imperial Health Office and the circumstances leading to his involvement in the search for the syphilis agent in the following passage:

> "In 1904 the Imperial Health Office called him back to Berlin. He was very unhappy with the prospect of spending most of his time at a desk writing administrative reports for burial in some files. That office was known as a hotbed of bureaucracy, where so-called administrators moved the unending merry-go-round of reports and files as an end in itself. But things happened differently. At this time the Kaiser became shocked by a report on the incidence of syphilis in the armed forces, and he gave orders to the Health Office to try to find the cause of the disease and a cure. Thus it happened that Schaudinn finally

34 R.S. Cohen and T. Schnelle, 'Introduction', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, p. XI. A similar criticism was already voiced in Hans Petersen's early review; see Petersen, op. cit. (note 1).

received the command of His Majesty to discover the syphilis germ, if we may describe the happenings a little facetiously".[35]

Regrettably, I am not in a position to confirm or refute Goldschmidt's assertion that it was the Kaiser's alarm about the incidence of syphilis in the armed forces which induced the Health Office to search for the syphilis agent. This assertion, if true, would of course be grist to Fleck's mill. In connection with the Wassermann reaction he emphasizes the stimulus exercised by a high ministry official, Friedrich Althoff, as an important social motive for the development of this diagnostic test (68-69/90). If Goldschmidt's recollections are correct on this score, then we have an even more impressive social influence personified by the Kaiser himself in this case. In contradistinction to Fleck, however, Goldschmidt does not minimize the individual contribution of Schaudinn to the discovery of the pale spirochaete: "His former work had made him well acquainted with this group of organisms [i.e. spirochaetes], and his uncanny power of observation made him see where others had failed".[36] Schaudinn, then, was not an easily exchangeable and replaceable member of an anonymous team of civil servants. For Goldschmidt, there was no question that Schaudinn was the real discoverer of *Spirochaeta pallida*. He did not even grant the title of 'co-discoverer' to Hoffmann.[37] Hoffmann's principal, Professor Lesser, strongly defended the latter's claim to the title of 'co-discoverer' on several occasions, awarding the title of 'discoverer' to Schaudinn.[38] For my part, I do not wish to engage in the dispute about who precisely is to share in the honour of the discovery. My only aim was to point out that Fleck's designation of the Imperial Health Office (or the team of civil servants organized by it) as the 'true discoverer' is highly problematic.

In a sense, if one takes an 'anti-individualistic' point of view, Fleck's designation of the Health Office as the collective discoverer does not even go far enough. Why should one limit oneself to this particular bureaucracy, why not also award the title of 'discoverer' to the collectivity of the "more than a hundred authors" who, according to the official report of August 1905, have shown pale spirochaetes "in the most diverse products of syphilis" (16/25)? There is nothing in Fleck's approach that militates against this extension of the title

35 R. Goldschmidt, *Portraits from Memory: Recollections of a Zoologist*, Seattle (University of Washington Press), 1956, pp. 133-34.

36 Goldschmidt, op. cit. (note 35), p. 134. Paul de Kruif also emphasizes Schaudinn's anti-bureaucratic mentality and his special capacity for observation ("his hawk-eye used to spotting extremely tiny, almost non-existent spirochaetes"), see de Kruif, op. cit. (note 21), p. 204.

37 "After Schaudinn's early death all this was forgotten, and in the course of time Hofmann [sic] emerged as codiscoverer"; Goldschmidt, op. cit. (note 21), p. 134.

38 See, for instance, Lesser's letter to Metchnikoff, 25 May 1905, reprinted in Zeiss, op. cit. (note 19), p. 173; see also 'Gesellschaft der Charité-Aerzte: Sitzung vom 8. Juni 1905', *Berliner klinische Wochenschrift* 42 (1905): 9991.

of discoverer. But, of course, it would reduce the very notions of 'discoverer' and 'discovery' to absurdity.

Despite the inadequacies of Fleck's collectivist approach, it contains the valuable insight that a discovery, to be recognized as such, has to undergo a social process of validation. It is the latter that stamps a purported discovery as a genuine discovery, or as Fleck asserts about Schaudinn's finding: "The meaning and the truth value of Schaudinn's finding is [..] a function of the community of those who, maintaining intellectual interaction on the basis of a shared intellectual past, made his achievement possible and accepted it" (40/56).

To substantiate this view, Fleck offers what would be called in modern constructivist parlance a *symmetrical* account of the acceptance of *Spirochaeta pallida* and the rejection of *Cytorrhyctes luis* as the causative agent of syphilis. Before turning to Fleck's analysis, I will briefly discuss the clearly *asymmetrical* account of the same episode offered by Goldschmidt. Goldschmidt describes the direct aftermath of the memorable meeting of the Berlin Medical Society on 17 and 24 May 1905:

> "Subsequently the entire profession declared the spirochete nonexistent and cracked jokes about its paleness which allowed only Schaudinn to see it. The worst of the jeering crowd was his old teacher, the zoologist Franz Eilhard Schulze, who, though an excellent zoologist, was the prototype of what I have described as a *Bonze*. He had become involved somehow with a young man named Siegel who claimed to have discovered the syphilis germ in the form of an almost submicroscopic body, a 'discovery' which was soon exploded. But Schulze defended him and attacked Schaudinn in the nastiest way [..] But slowly the confirmations of Schaudinn's findings came in [..] and Schaudinn finally received credit for his discovery".[39]

In his description of events Goldschmidt uses the so-called 'contingent repertoire', a type of discourse that is called upon to account for the activities of those scientists who, according to the author or speaker, subscribe to incorrect views.[40] The attribution of a truth value comes first; in this case it is taken that Siegel's microbe is *not* the syphilis agent (his 'discovery' - within inverted commas - was soon 'exploded'). Then the behaviour of the adherents of this 'pseudo-agent' can be described in social and psychological terms and not in terms of scientific rationality (or the 'empiricist repertoire' in the terminology of Gilbert and Mulkay). Thus Goldschmidt tells about Schulze that he was "the worst of the jeering crowd"; that he was an unpleasant authoritarian person (a '*Bonze*'): that he "had become involved somehow with" Siegel (surely not a rational process!) and that he "attacked Schaudinn in the

39 Goldschmidt, op. cit. (note 35), pp. 134-35.

40 G.N. Gilbert and M. Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse*, Cambridge (Cambridge University Press), 1984, pp. 79-82.

nastiest way".[41] The rhetorical effect of this 'contingent repertoire' is that it will obviate the need for assessing the arguments put forward by Schulze and company on their own merits. The point of the principles of symmetry and impartiality of modern constructivism is precisely to avoid using two different discursive repertoires, the empiricist and the contingent, to account for scientific views held to be correct or incorrect.

In his more symmetrical analysis, Fleck imagines the possibility that John Siegel's *Cytorrhyctes luis* had been accepted as the causative agent of syphilis: "If his [Siegel's] findings had had the appropriate influence and received a proper measure of publicity throughout the thought collective, the concept of syphilis would be different today. Some syphilis cases according to present-day nomenclature would then perhaps be regarded as related to variola and other diseases caused by inclusion bodies" (39/55). Fleck abruptly ends his thought experiment, however, by observing that such a possibility can only be envisioned logically but not construed as a historical possibility. By the turn of the century, Fleck holds, the concept of syphilis had already become too rigid for Siegel's microbe to be accepted as the causative agent.

Fleck's analysis, though intriguing as an attempt at symmetrical explanation, is not convincing. It is doubtful whether the existing syphilis concept really constituted the decisive obstacle for the acceptance of Siegel's microbe. It is true that Siegel postulated family relationships between his 'agent of syphilis' and the causative agents of variola, smallpox, foot-and-mouth disease and scarlet fever. In his turn Schaudinn construed a kinship relation between *Spirochaeta pallida* and the spirochaete causing relapsing fever, to say nothing of the controversial connection with the trypanosomes which he also suggested. In the eyes of an experienced dermatologist like Oscar Lassar the relationship between syphilis and the exanthemas mentioned by Siegel was actually much more plausible than the relationship between syphilis and relapsing fever:

> "One cannot ignore that there are also many analogies between the various exanthemas and syphilis. The relapsing fever spirillum [better: spirochaete] creates such heavy reactions within the visceral organs, which we do not encounter even approximately in syphilis".[42]

The existing concept of syphilis was therefore not the real stumbling block for *Cytorrhyctes luis*. It can also be argued that Fleck, by simply assuming in his thought experiment that Siegel's findings "had received a proper measure of publicity throughout the thought collec-

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41 There is a striking parallel with a recent *cause célèbre* in Dutch AIDS research, the so-called Buck/Goudsmit affair. After Henk Buck's production of phosphate methylated DNA (to be used as medicine against AIDS) was exposed as an 'error', the evaluation of his personal character changed dramatically: from a 'fatherly genius' he turned into a 'tiran' intimidating his collaborators. See R. Hagendijk and J. Meeus, 'De Buck/Goudsmit Affaire: Feiten, fictie en blind vertrouwen', *Kennis en Methode* 17 (1993): 147-91, p. 171.

42 Lassar, op. cit. (note 15), p. 730.

tive"[43], evades the main point, namely, to explain why Siegel's microbe did nowhere come near the "measure of publicity" *(denkkollektive Verbreitung)* that was achieved by Schaudinn's agent. Why had the pale spirochaete, less than half a year after its discovery, already been found by more than a hundred investigators in various syphilitic lesions, whereas Siegel's findings were only confirmed by a few collaborators of the Zoological Institute in Berlin led by Professor Schulze? It would appear that such a preponderance in sheer numbers constitutes a real challenge for a symmetrical, constructivist explanation. Would it not be more natural to conclude that Schaudinn and Hoffmann's findings got so much more confirmation, because *Spirochaeta pallida* really is the agent of syphilis?

5. The silver spirochaete

As we have seen, by 12 August 1905 *Spirochaeta pallida* had already been found by more than a hundred investigators in the most diverse products of syphilis. In October 1905 Fritz Schaudinn confidently expressed his expectation that "[t]he findings will steadily increase even further when all investigators acquire the necessary experience in finding and staining these delicate forms".[44] Although he reiterated his view that *Spirochaeta pallida* possessed a characteristic shape of its own which allowed it to be clearly distinguished from other spirochaetes, he also admitted that the recognition of this characteristic form required "a certain feeling for the typical" *(ein gewisses Gefühl für das Typische)*. Moreover, staining with Giemsa solution also demanded experience and skill to avoid the occurrence of all kinds of artefacts [*Kunstprodukte*]. Schaudinn referred in particular to the hazard of insufficient staining, which would make other spirochaetes appear just as pale as the pale spirochaete would be under normal staining conditions.[45]

It was about this time that two investigators, the Italian Bertarelli and the French-Romanian microbiologist Constantin Levaditi, independently of each other developed a new method of staining (or rather impregnation) by means of silver nitrate. Levaditi had been inspired by the silver impregnation method used by Santiago Ramon y Cajal to demonstrate nervous tissue. The advantage of the silver impregnation method was its capacity to show pale spiro-

43 The English translation is somewhat inappropriate here; the German original reads: "Wäre seiner Erkenntnis entsprechende suggestive Wirkung und denkkollektive Verbreitung zu Teil worden [..]" (German: 55).

44 F. Schaudinn, 'Zur Kenntnis der Spirochaete pallida: Vorläufige Mitteilung', *Deutsche medizinische Wochenschrift* 31 (1905): 1665-67, p. 1665.

45 Schaudinn maintained that this had happened to two investigators, Kiolemenoglori and von Cube, who had found spirochaetes, which were said to be indistinguishable from *Spirochaeta pallida*, in carcinomas and other non-syphilitic lesions.

chaetes regularly as black spirals in *tissue sections*, whereas Giemsa staining was largely limited to the demonstration of those microbes in *smears*.

Bertarelli had developed the silver impregnation method in the course of his experimental research on the transmission of syphilis to rabbits. By inoculating luetic material into the cornea he was able to produce a typical keratitis syphilitica. As a control procedure he retransmitted the disease from infected rabbits to monkeys, which in due course exhibited all the characteristic symptoms of monkey syphilis. Bertarelli's success made a new test animal, the rabbit, available for the experimental study of syphilis. The availability of a cheaper and more tractable experimental animal species than primates was actually one of the preconditions for the reorientation of Paul Ehrlich's chemotherapeutic research programme toward syphilis. Bertarelli used his new silver impregnation method to demonstrate the presence of pale spirochaetes in tissue sections of the cornea. He sent his preparations to Schaudinn, who recognized the black spirals as specimens of *Spirochaeta pallida*. Thus the silver impregnation method received the imprimatur of the discoverer of the pale spirochaete.

The priority of the successful transmission of syphilis to rabbits had already been claimed by two adherents of *Cytorrhyctes luis*, John Siegel and Walter Schulze. Their experimental results were however dismissed as spurious by Erich Hoffmann and others, because the monkeys that were re-infected from their 'syphilitic' rabbits exhibited phenomena which did not correspond to the typical manifestations of monkey syphilis established by other investigators.[46] Siegel and Schulze also claimed to have demonstrated, using a normal aniline dye stain, *Cytorrhyctes luis* in the corneas of their 'syphilitic' rabbits. No wonder that the silver impregnation method should arouse their suspicion.

On 7 December 1905, at a meeting of physicians of the Charité hospital, Erich Hoffmann demonstrated several preparations of pale spirochaetes obtained with the aid of the new methods of Bertarelli and Levaditi.[47] He made an important statement:

> "Finally, I would like to emphasize on this occasion, that now, after so many authors have furnished evidence for the specific nature of *Spirochaeta pallida* and the demonstration in tissue sections [*Schnitten*] also has succeeded, there can be no longer any doubt about the aetiological significance of this organism".[48]

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46 'Berliner ophthalmologische Gesellschaft: Sitzung vom 21. Dezember 1905', *Berliner klinische Wochenschrift* 43 (1906): 370-71; Dr. Wechselmann, 'Experimenteller Beitrag zur Kritik der Siegelschen Syphilisübertragungsversuche auf Tiere', *Deutsche medizinische Wochenschrift* 32 (1906): 219-20.

47 Those methods differed in details. The silver impregnation method of Levaditi became the most widely used one.

48 'Gesellschaft der Charité-Aerzte: Sitzung vom 7. Dezember 1905', *Berliner klinische Wochenschrift* 43 (1906): 175-76.

Thus, by this time, Hoffmann was willing to drop all provisos and reservations with regard to the aetiological status of the pale spirochaete.

Thanks to the silver impregnation method, the stream of new finds of pale spirochaetes, in particular in tissue sections of internal organs, continued to flow during the next year. Many authorities, e.g. Flügge, Gaffky, and Löffler, abandoned their former scepticism with regard to the probable aetiological status of *Spirochaeta pallida*. The pathological anatomist C. Benda declared that the results of the new method made him completely change his mind in the period between March and June 1906: from a "doubter" [*Zweifler*] he turned into a "confessor" [*Bekenner*].[49]

After Schaudinn's early death on 22 June 1906, the adherents of *Cytorrhyctes luis* launched a full-scale attack on what they derisively called the "silver spirochaete", in a last effort to stem the tide. They propounded the view that the preparations obtained by silver impregnation were mere artefacts. Those black spirals made visible through silver nitrate were not spirochaetes at all, they maintained, but nerve endings, connective tissue fibres, elastic fibres or other tissue constituents which had disintegrated through tissue degeneration and subsequently taken on a spiral form as a consequence of the alcoholic preservation and fixation treatment required by the silver impregnation method. In September 1906, Walter Schulze reported about his attempts to demonstrate 'silver spirochaetes', using Levaditi's method, in the cornea of rabbits inoculated only with street-refuse. His article contained photographs showing the "silver pseudo-spirochaetes" thus obtained which closely resembled the 'silver spirochaetes' obtained by Bertarelli and other investigators.[50] In the same issue of the *Berliner klinische Wochenschrift* Schulze's colleague Hans Friedenthal presented photographs of silver precipitations in carcinoma tissue "which bear a deceptive resemblance to the silver spirals described as Spirochaeta pallida".[51] Likewise, a third member of the Zoological Institute in Berlin, Dr. Saling, also contributed to what was apparently a concerted action (Benda spoke about a *Feldzug* or 'campaign') to destroy the credibility of the "silver spirochaete". It is not difficult to imagine why they chose this as the strategic target for their attacks, for as Dr. Saling concluded from the alleged exposure of this 'artefact': "Therefore all those hundreds of confirmations, which allegedly prove the presence of the so-called 'lues spirochaete' in the internal organs, are dissolved".[52]

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49 C. Benda, 'Zur Levaditifärbung der Spirochaeta pallida', *Berliner klinische Wochenschrift* 44 (1907): 428-32, 480-84, p. 428.

50 W. Schulze, 'Die Silberspirochaete', *Berliner klinische Wochenschrift* 43 (1906): 1213-16.

51 H. Friedenthal, 'Ueber Spirochaetenbefunde bei Carcinom und bei Syphilis', *Berliner klinische Wochenschrift* 43 (1906): 1217-18, p. 1217.

52 'Berliner medizinische Gesellschaft: Sitzung vom 20. Februar', *Berliner klinische Wochenschrift* 44 (1907): 254-59, p. 258.

The question of the "silver spirochaete" was put on the agenda of the Berlin Medical Society during four successive meetings in February and March 1907. In this "long debate between friends and opponents of the spirochaete" (Blaschko[53]), almost all the issues which divided the two parties were extensively discussed. The oppositional party, represented by Saling, Walter Schulze, Hans Friedenthal, and Jancke[54], stated its case clearly. The pale spirochaetes which were mostly found in skin lesions and could be demonstrated in smears through Giemsa stain, had to be considered as harmless saprophytes just like some other spirochaetes such as *Spirochaeta refringens*. Indeed, there was no set of characteristics by which *Spirochaeta pallida* could be clearly distinguished from other spirochaetes and which would justify to consider it a separate species. The fact that pale spirochaetes were also occasionally found in syphilitic lesions, didn't say anything about their aetiological significance. As such the pale spirochaetes stained with Giemsa solution had nothing to do with the so-called "silver spirochaetes" demonstrated in tissue sections by means of the silver impregnation method. The latter represented nerve fibrils, elastic fibres or other normal tissue constituents which had disintegrated through processes of tissue necrosis or maceration.

The adherents of *Cytorrhyctes luis* had several arguments to back up their assertion about the non-identity of "Giemsa spirochaetes" and "silver spirochaetes". First, the two clearly differed in appearance. The "silver spirochaetes" looked much shorter and thicker. They did not accept Hoffmann's explanation that this could simply be accounted for by shrinkage as a consequence of fixation and paraffin preservation and the precipitation of a coating of silver grains around the spirochaete.[55] Secondly, Saling also pointed at the "enormous disproportion which is manifested in the fact that in the same piece of tissue myriads of socalled 'spirochaetes' are present after silver impregnation on sections, but that not a single spirochaete appears after staining with a true dyestuff!".[56] Such disproportion, he maintained, was not known of any other bacterium or protozoon which could be stained both with dyes and with silver. He formulated a methodological requirement which the adherents of *Spirochaeta pallida* had to fulfil:

> "The identity of the 'Giemsa spirochaete' with the so-called 'silver spirochaete' could be made plausible only if in sections of material treated in accordance with all the rules of the histological art, precisely on the analogous sites where, in the sections impregnated with silver, the so-called 'silver spirochaetes' are

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53 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 355.

54 Franz Eilhard Schulze and John Siegel were conspicuously absent on both occasions (May 1905 and February-March 1907) when the Berlin Medical Society discussed the question of the spirochaete.

55 'Berliner medizinische Gesellschaft: Sitzung vom 20. Februar', *Berliner klinische Wochenschrift* 44 (1907): 254-59, p. 256.

56 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 355.

> located in myriads, the same spiral fibres could also be demonstrated in equivalent quantities by using a dyestuff".[57]

This is indeed a very exacting demand. During the meetings of the Berlin Medical Society in February and March 1907, the "friends of the spirochaete" attempted to counter the criticisms put forward by the opponents. They pointed out that 'silver spirochaetes' had not only been demonstrated in tissues but also in the lumen of blood and lymph vessels. Such findings, they held, could not be explained by the tissue decay theory of the opponents.[58] The latter answered that fibres and other tissue constituents might be inadvertently displaced from the tissue by the microtome knife.[59]

The pathological anatomist Orth was not impressed by the artificial 'silver spirochaetes' produced by the adherents of *Cytorrhyctes luis*. He could not hide his irritation about their ways of arguing:

> "Hearing the opponents and reading their publications, one could believe that they were dealing with scientific novices whom they had to teach the first principles of microscopic observation. For my part, I have to protest when Mr Friedenthal, for example, pretends that those black things which he showed in his pictures in the *Berliner klinische Wochenschrift* [..], are taken, or could be taken, for spirochaetes by competent investigators. No one would have hit on that idea. This is a struggle against windmills".[60]

Although not all the things that were stained black by Levaditi's method were spirochaetes, it could safely be entrusted to the critical judgement of the competent investigators to reach a reasonable decision.

Two investigators, Bab and Mühlens, emphasized that the findings of 'silver spirochaetes' in the livers of luetic foetusses were confirmed by the outcomes of the recently developed Wassermann reaction used for antigen determination of 'liver extracts'.[61] Saling

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57 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 353.

58 This argument had already been used by Levaditi in a first reaction to Walter Schulze's accusations. See C. Levaditi, 'Bemerkungen zu dem Aufsatz "die Silberspirochaete" von W. Schulze in No 37 dieser Wochenschrift', *Berliner klinische Wochenschrift* 43 (1906): 1368-69.

59 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 350.

60 'Berliner medizinische Gesellschaft: Sitzung vom 6. März', *Berliner klinische Wochenschrift* 44 (1907): 318-20, p. 319.

61 'Berliner medizinische Gesellschaft: Sitzung vom 20. Februar', *Berliner klinische Wochenschrift* 44 (1907): 254-59, p. 259; 'Berliner medizinische Gesellschaft: Sitzung vom 27. Februar', *Berliner klinische Wochenschrift* 44 (1907): 291-95, p. 293.

replied that the originators of this serological test themselves had declared that it was not ready for practical use, as it did not yet furnish reliable results in every case.[62]

To support the identity of the 'silver spirochaete' and the 'Giemsa spirochaete', the pathological anatomist Benda showed photographs and preparations from the livers of syphilitic children. Smears taken from the same material which in silver-impregnated tissue sections exhibited the presence of 'silver spirochaetes', showed pale spirochaetes after staining with a Giemsa solution.[63] The opposite party, in the person of Saling, reacted by denying that the spirochaetes stained with Giemsa belonged to the species *Pallida*; they were said to exhibit the species characteristics of *Refringens*.[64] To Benda this assertion was "completely at odds with the fact" [*durchaus der Tatsache widersprechend*]. The venereologist Alfred Blaschko commented on this disagreement: "In my view, the preparations of Mr Benda are not conclusive to a *malevolent* judge, but only to those who have any inkling of the extreme difficulty with which these organisms can be stained with Giemsa [emphasis added]".[65] Blaschko also argued that the methodological requirement of a complete congruence, even in quantitative terms, between the results of silver impregnation and Giemsa staining was highly unreasonable. Given its delicacy and its special tinctorial properties, it was no more than to be expected that *Spirochaeta pallida* could not be made visible with the normal dyes in the relatively thick tissue sections.

During the long discussions in the Berlin Medical Society, many "friends of the spirochaete" must have had the feeling that in dealing with their opponents from the Zoological Institute they were rapidly reaching the limits of reasonable debate. Their exasperating experience was that they could not force their opponents into line by what they considered as rational and convincing arguments. Their determined opponents acted much like "Awkward Student" or "the obstinate dissenter" in the modern sociology of science textbooks of Harry Collins and Bruno Latour.[66] No wonder that the "friends of the spirochaete" some-

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62 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 354. As a matter of fact, 'antigen determination' would later be abandoned as spurious; 'antibody determination' would become the only reliable part of the Wassermann test. See Chapter VI.

63 'Berliner medizinische Gesellschaft: Sitzung vom 27. Februar', *Berliner klinische Wochenschrift* 44 (1907): 291-95, p. 293; 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 357.

64 'Berliner medizinische Gesellschaft: Sitzung vom 27. Februar', *Berliner klinische Wochenschrift* 44 (1907): 291-95, p. 293; 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 356.

65 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 356.

66 H.M. Collins, *Changing Order: Replication and Induction in Scientific Practice*, London (Sage), 1985, p. 13; B. Latour, *Science in Action*, Milton Keynes (Open University Press), 1987, p. 64.

times resorted to *authority arguments*. Both Hoffmann and Orth, for example, disputed the competence of non-medical scientists to speak about medical subjects like necrosis and maceration. Hoffmann also appealed to the views of Walter Schulze's own teacher, Professor Greeff, to defend Bertarelli's priority against Siegel and Schulze's claims to have transmitted syphilis to rabbits for the first time. He also emphasized that "virtually all syphilologists" and "almost all pathological anatomists" supported the aetiological status of *Spirochaeta pallida*.[67]

As will be clear by now, the controversy about the 'silver spirochaete' was not settled. In his final word Saling reaffirmed his position: "All those hundreds of 'confirmers' [*'Bestätiger'*] have fallen victim to a severe delusion; and Mssrs Bertarelli, Hoffmann, Benda etcetera may not take it ill of me that I have some doubts about their critical judgement and their capacity of observation". Blaschko, in his final speech, noted that all rational arguments had been idle and impotent: "Who does not want to be convinced by what Mr Benda and I, and by what Mssrs Mühlens, Hoffmann, Bab etcetera have expounded in truly sufficient extension, such a person cannot be convinced in any possible way".[68] Blaschko concluded that it was time to close the debate and to continue the work on the *Spirochaeta pallida* without regard for the views of the obstinate opponents.

And this is indeed what would happen: from then on, the opponents' criticisms would be simply ignored.

6. Problems of classification

In an essay written in 1962, Thomas S. Kuhn noted: "[..] to discover something one must also be aware of the discovery and know as well what it is that one has discovered".[69] He immediately added however: "But [..] how much must one know?". Presumably Priestley did not know enough to be called the discoverer of oxygen, because, although he did produce the gas, he thought that he had 'dephlogisticated air' in his hands. Even Lavoisier's claim to the title of discoverer of oxygen is not fully secured, because his notion of oxygen as an atomic 'principle of acidity' does not correspond to present-day views. Such considerations are also pertinent when we discuss Schaudinn's claims to the title of discoverer of the syphilis agent. He held some idiosyncratic views on the nature of this agent which were already

67 'Berliner medizinische Gesellschaft: Sitzung vom 20. Februar', *Berliner klinische Wochenschrift* 44 (1907): 254-59, p. 256.

68 'Berliner medizinische Gesellschaft: Sitzung vom 13. März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 355 and p. 356.

69 T.S. Kuhn, 'The Historical Structure of Scientific Discovery', reprinted in T.S. Kuhn, *The Essential Tension*, Chicago (University of Chicago Press), 1977: 165-77, p. 170.

controversial in his own time and which are no longer considered credible today. So, did Schaudinn 'know' what it was that he had 'discovered'?

His most controversial view on the nature of *Spirochaeta pallida* derived from a speculative theory which he had already developed before he became involved in the search for the syphilis germ. According to this theory, malaria parasites, trypanosomes and spirochaetes were merely different forms in the development of one single type of organism. That would entail that *Spirochaeta pallida* would be a developmental form of an organism, which would also comprise specific but as yet unknown types of trypanosomes and malaria parasites as developmental stages. In his presentation before the Berlin Medical Society in May 1905, Schaudinn was very reticent to enfold his precise views on this point. He stated that of only one cognate microbe, *Spirochaeta ziemanni*, more was known of its developmental cycle and hinted that more research needed to be done before definitive statements could be made.[70] As we have seen, Schaudinn's entire theory had already been severely criticized by the American bacteriologists Novy and McNeal as being based on mixed infections.

Interestingly enough, Schaudinn's "tremendously erroneous" and "idiotic" theory - to use Paul de Kruif's descriptions[71] - was not without significant effects on anti-syphilitic therapy. His speculative view on the close relationship between the syphilis agent and the trypanosomes induced Paul Ehrlich, who had attended the discussions in the Berlin Medical Society, to extend and subsequently redirect his chemotherapeutic research programme from trypanosomal diseases to syphilis. Ehrlich was not the only medical investigator to be influenced by Schaudinn. The direct effect of the latter's views was that the chemical substance *atoxyl*, already in use against sleeping sickness, was widely tried as a medicine against syphilis.[72] The side-effects of *atoxyl* soon turned out to be unacceptably severe (it caused blindness), but the attempts would ultimately lead to the discovery of the 'magic bullet'.

From the very outset, Schaudinn's view of spirochaetes as belonging to the kingdom of Protozoa has met with disagreement as well as assent. We have already seen that at the first meeting of the Berlin Medical Society in May 1905, Thesing objected to the presumed protozoal nature of *Spirochaeta pallida* and argued for its classification among the bacteria. The latter view was by no means confined to the opponents of the pale spirochaete. Benda, for instance, also subscribed to the bacterial nature of this organism.[73] Criteria that were used at the time for assigning the group of spirochaetes to the kingdom of Protozoa included: nearly perfect flexibility of the body, the presence of an undulating membrane, and multipli-

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70 Schaudinn and Hoffmann, op. cit. (note 27), p. 673.

71 De Kruif, op. cit. (note 21), p. 190 and p. 204.

72 O. Lassar, 'Atoxyl bei Syphilis', *Berliner klinische Wochenschrift* 44 (1907), p. 684; Lassar acknowledges the influence of Schaudinn's views on his trials of atoxyl against syphilis.

73 'Berliner medizinische Gesellschaft: Sitzung vom 13 März', *Berliner klinische Wochenschrift* 44 (1907): 350-58, p. 358.

cation by longitudinal division.[74] It was difficult to ascertain these criteria for each and every member of the group of spirochaetes, and also to justify the pertinence of the criteria themselves.

For several decades, the precise position occupied by the spirochaetes within the system of nature has been disputed among protozoologists and bacteriologists. Both disciplines have "claimed" this group of organisms.[75] According to some, "spirochetes represent[ed] a kind of no man's land between the true bacteria and protozoa".[76] Modern textbooks tend to class the spirochaetes (or the order of 'Spirochaetales') among the *Schizomycetes* (bacteria in the broadest sense), but also stress their special characteristics which set them apart from the 'true' bacteria.[77] Since the appearance of the sixth edition of *Bergey's Manual of Determinative Bacteriology* in 1948, the order of 'Spirochaetales' is generally divided into two families, each of which is in turn divided into three genera.[78] Schaudinn's pale spirochaete now belongs to the family of Treponemataceae and to the genus of *Treponema*; correspondingly, it has been renamed *Treponema pallidum*.

Did Schaudinn 'know' what it was that he had 'discovered' when he thought that it was a protozoon? One might reasonably argue that the precise assignment of the pale spirochaete to *higher-order* taxa is not relevant for judging his claims to the title of discoverer, as long as he correctly delineated the pale spirochaete as a distinct *species* (to count as the discoverer of whales, one need not know that they belong to the mammals). But on this account there are also problems. These difficulties have been exploited by the adherents of *Cytorrhyctes luis*. Hans Friedenthal devotes an ironic, even sarcastic paragraph to the wide variety of descriptions given of the pale spirochaete:

> "Whereas in Schaudinn and Hoffmann's first publication *Spirochaeta pallida* did not have a fixed longitudinal axis, but performed bending, twisting and lashing movements with its entire body, was also provided with an undulating membrane and could be distinguished from spirilla through the absence of flagella, the image had already completely changed in Schaudinn's second communication. *Spirochaeta pallida* had lost its undulating membrane and had received flagella in return. Its life habits had also totally changed. Rather than about the bending and lashing movements of the whole body, we read in the second communication about the fixed, coiled complexion of *Spirochaeta pallida*. Further contributions to the

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74 G. Keysselitz, 'Ueber die undulierende Membran bei Trypanosomen und Spirochäten', *Archiv für Protistenkunde* 10 (1907): 127-38.

75 G.E. Davis, 'The Spirochetes', *Annual Review of Microbiology* 2 (1948): 305-33, p. 305.

76 Q.M. Geiman, 'Metabolism of Spirochetes', *Annual Review of Microbiology* 6 (1952): 299-316, p. 299.

77 See for example R. Cruickshank et al., *Medical Microbiology*, Edinburgh and London, 1973 (12th edition), p. 47.

78 R.S. Breed et al., *Bergey's Manual of Determinative Bacteriology*, Baltimore (The Williams & Wilkins Company), 1948 (6th edition).

> medical literature grant a head-like thickening of the fore-end or even of both ends to the spirochaete, whereas other authors claim to distinguish *Spirochaeta pallida* easily from other spirochaetes by its tapered ends. *Spirochaeta pallida* is described as having from three to eighty coils, so that it would be nearly visible with a magnifying-glass. Depending on the observers, it multiplies by longitudinal or by transverse division. Some even had the opportunity to catch a pair of spirochaetes copulating".[79]

This passage can be read as an ironic comment on Schaudinn's previously cited statement: "If one has imprinted the characteristic image of this spiral in one's mind, then, in my opinion, one will always easily recognize this form again".[80] Friedenthal concludes that, on the basis of the often contradictory characterizations given, it is impossible for him to delineate and identify pale spirochaetes. The reader might assume that Friedenthal has unduly exaggerated the variety of descriptions for rhetorical reasons. The quoted passage exhibits indeed some exaggeration, but not, I think, out of all proportion. We may therefore ask again: Did Schaudinn 'know' what it was that he had 'discovered'?

The extreme variability of characterizations of *Spirochaeta pallida* is comparable to a similar situation which obtained with respect to descriptions of diphteria bacilli. In his essay 'Scientific observation and perception in general', Fleck analyzes the descriptions given of these microbes in several bacteriological textbooks dating from different years.[81] He notes that for the layman these descriptions would be simply inconsistent, while for the specialist - not! -, "because [the specialist] knows that they are to be taken *cum grano salis*; each of [these descriptions] suggests certain pictures which *may be*, but *do not have to be found* everywhere. The most important and the most essential is the fact that the system is *characteristic* [..]".[82] A wide variety of descriptions of the 'same' entity might not be as dramatic and alarming as was suggested by Friedenthal.

Elsewhere, in his postwar essay 'To look, to see, to know', Fleck describes the emergence of a common *Gestalt* or standard pattern as arising out of the "oscillating pictures" and "phantastic images" proposed by individual investigators:

> "[T]he collective life produces among these oscillating possibilities a novel prescribed form, which is then fixed and pressed upon the individual. The collective experience and custom determine which feature is fundamental and what can be variable, and how far this variability can extend".[83]

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79 Friedenthal, op. cit. (note 51), p. 1217.

80 Schaudinn and Hoffmann, op. cit. (note 27), pp. 673-74.

81 L. Fleck, 'Scientific Observation and Perception in General', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, pp. 59-78, esp. on pp. 67-72.

82 Ibid., p. 69.

83 L. Fleck, 'To look, to see, to know', in Cohen and Schnelle, op. cit. (note 81), pp. 129-151, on p. 140.

From this perspective it would be unreasonable to expect that a single researcher could set the standard for the community of researchers. It is therefore not surprising that Schaudinn did not lay down the definitive set of characteristics by which to distinguish the pale spirochaete from its kin. Perhaps he is not to be considered the discoverer of the syphilis agent after all?

7. Conclusions

In the introduction I argued that the discovery of the pale spirochaete as the causative agent of syphilis offers a better example for studying the social construction of a fact than Fleck's own example of the Wassermann reaction, because from a common-sense point of view the latter could be considered an invention rather than a discovery of a fact. So what does it mean to say that the scientific fact that *Spirochaeta pallida* (or *Treponema pallidum*) is the causative agent of syphilis has been (socially) constructed?

Does it imply that this purported fact is not a real fact at all, that it is just a forgery, that it has been 'all made up' or 'fabricated out of thin air'? Of course not! It is, however, testimony to the force of the prevalent way of thinking that words such as 'making', 'fabrication' or 'construction' which accentuate the *active* involvement of the scientists in the process of discovery automatically arouse the suspicion that the results are spurious. This is also noted by the American philosopher Nelson Goodman in a chapter bearing the title 'The Fabrication of Facts':

> "[..] 'fabrication of fact' has a paradoxical sound. 'Fabrication' has become a synonym for 'falsehood' or 'fiction' as contrasted with 'truth' or 'fact'. Of course, we must distinguish falsehood and fiction from truth and fact; but we cannot, I am sure, do it on the ground that fiction is fabricated and fact found".[84]

One should recall that 'fact' is etymologically derived from the Latin verb 'facere', 'to make'!

Another interpretation of the above-mentioned statement could be that microbiological science has fashioned a new kind of microbe, called it *Spirochaeta pallida*, furnished it with the property of causing syphilis among humans, and finally let it loose unto an unsuspecting world. Of course, this view would be plainly ridiculous. It would turn 'discovery' of a pathogenic microbe into a criminal offence. Some interpretations of the construction metaphor in the works of Knorr-Cetina and Latour, however, seem to come perilously close to this view. Knorr-Cetina holds that "science secretes an unending stream of entities and

[84] N. Goodman, *Ways of Worldmaking*, Indianapolis (Hackett), 1992 [1978], p. 91.

relations that make up 'the world'"[85], whereas Latour sees science as engaged in the continuous multiplication or proliferation of new quasi-objects. Thus, it is said that Pasteur and the Pasteurians multiplied our numbers by making room for millions of invisible microbes as new members of the social body.[86] In the final chapter I will reconsider this version of the constructivist thesis. Here it must suffice to say that *this* interpretation of the construction metaphor was not intended in the account of the present chapter.

What, then, would be a more plausible reading of the thesis that scientific facts are (socially) constructed? In my view, it should be read as saying that scientific facts, or rather *decisions as to what the facts are* (this reformulation is important!), are always the outcome of a process of social interaction among scientists in which data and arguments are exchanged but in which an element of negotiation is also inevitably present. The outcome can never be fully accounted for by the available empirical evidence and the accepted methodological rules alone - which for rationalist philosophers constitute the incarnation of rational procedure. Such rules combined with evidence do not decide the issues by themselves but leave ample space for more than one interpretation ('interpretive flexibility'). Constructivists can argue for this position philosophically by an appeal to the so-called Quine-Duhem thesis (or the thesis of the underdetermination of theories by the data) or Wittgenstein's insight that rules cannot determine their own application, but they preferably try to base their case on a multiplicity of empirical case studies.

Microbiology offers a striking example of the 'negotiability' of methodological rules. As we have already seen in the previous chapter, *Spirochaeta pallida* was eventually accepted as the causative agent of syphilis even if it turned out to be impossible to satisfy Koch's postulates (because the microbe resisted every attempt at cultivation in nutrient media). There are more exceptions to Koch's rules in the history of medical science. Would such deviations from the ideal procedure be condoned by rationalist philosophers?[87]

In the present case of the aetiology of syphilis, the 'construction of a fact' was intimately connected with the question of the possible creation of 'artefacts'. Seen from the pers-

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85 K. Knorr-Cetina, 'Strong Constructivism - from a Sociologist's Point of View: A Personal Addendum to Sismondo's Paper', *Social Studies of Science* 23 (1993): 555-63, p. 557.

86 B. Latour, *The Pasteurization of France*, Cambridge MA and London (Harvard University Press), 1988, pp. 35-38.

87 Koch's postulates are also at issue in the more recent controversy on whether or not HIV is the cause of AIDS. Virologist Peter Duisberg, the notorious critic of the widely accepted view that HIV is indeed the cause of AIDS, bases his case inter alia on the fact that, in his view, Koch's postulates have not been fulfilled. Until 1994 his opponents conceded this point, but denied that this circumstance constitutes a convincing reason to reject the aetiological status of HIV. In the year 1994 an accident involving three laboratory workers occurred. As a consequence, AIDS researchers now claim that Koch's postulates have been fulfilled, but Duesberg still disagrees: "Duesberg told *Science* that, in his view, the lab-worker data don't prove that HIV satisfies Koch's postulates" (J. Cohen, 'Fulfilling Koch's Postulates', *Science* 266 [1994]: 1647).

pective of the minority group of 'opponents of the pale spirochaete' the same episode should rather be described as the failed deconstruction of an artefact - 'failed' in the sense that the opponents were unable to persuade the larger medical community of their views. According to rationalist and realist philosophers such as Anthony Derksen, Ian Hacking or Allan Franklin, science has available several ready-made methodological criteria by which to distinguish genuine results from mere artefacts. One possibility is to use the 'argument from coincidence' by obtaining the same result using different experimental apparatus or procedures. Hacking gives the example of seeing dense bodies in cells by the use of both electron and light microscopes: "I say that if you can see the same fundamental features of structure using several different physical systems, you have excellent reason for saying, 'that's real' rather than, 'that's an artifact'".[88] In this chapter we have seen that two different staining methods, Giemsa staining and silver impregnation, were deployed to visualize the pale spirochaete. This could not, however, resolve the issue at stake.

For the 'friends of the spirochaete' the matter was clear: silver impregnation confirmed the results obtained by Giemsa staining and substantially enlarged the visibility of the microbe. The 'opponents', however, saw things differently. They stressed the dissimilar appearances of the 'Giemsa spirochaete' and what they referred to as "the socalled 'silver spirochaete'", discounting the explanation given by the other side for this difference. They also pointed at the great discrepancy in numbers of spirochaetes detected in similar tissues by using the two staining methods, thus arguing that there was no congruence in quantitative terms between the results of both methods. Of course, for the 'friends' such lack of exact quantitative correspondence was precisely what was to be expected, given the character of the spirochaete (its difficult stainability with Giemsa dye). In the present case, then, the issue at stake - genuine result or artefact? - could not be settled to the satisfaction of the opposing parties by the deployment of different staining methods, because there was no agreement on what constituted comparable outcomes or sufficient congruence.

Nor could an appeal to the large number of independent confirmations of Schaudinn's and Hoffmann's findings settle the matter. The charge that the 'silver spirochaete' was a mere artefact aimed precisely at destroying the credibility of the greater part of those confirmations. Moreover, particularly in the initial stages, the characteristics attributed to the pale spirochaete varied almost from one publication to the next. It was only in the course of a collective process that a common *Gestalt* or standard pattern emerged out of the 'oscillating pictures' and 'phantastic images' proposed by individual investigators. (This is in full agreement with Fleck's observations on the variability of descriptions of diphteria bacilli and other bacteria.) In the end the controversy on the aetiology of syphilis was 'closed' by ignoring

[88] I. Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science*, Cambridge (Cambridge University Press), 1986 [1983], p. 204. For a more rationalist treatment of the 'epistemological rules' involved in experimentation, see A. Franklin, 'The Epistemology of Experiment', in D. Gooding et al. (eds.), *The Uses of Experiment*, Cambridge (Cambridge University Press), 1989, pp. 437-60.

rather than painstakingly refuting the detailed objections of the minority group of 'opponents'. As this group was able to invent new criticisms after each attempt of refutation, it appears that this was the only way to achieve 'closure'.

In this chapter I have analyzed the discovery of a fact as a process of social construction. The final question that I must now consider concerns the status and meaning of the common-sense notion of a 'discovery'. In the preceding sections I have tried to problematize this notion by repeatedly raising the (admittedly somewhat rhetorical) question of *when* and *by whom* the causative agent of syphilis has been discovered. Was it on March 3rd, 1905, or on March 21st, or on May 24th, or perhaps later still; was it by Schaudinn or by Schaudinn and Hoffmann or by the Imperial Health Office (the 'true discoverer' according to Fleck) or perhaps even by the wider scientific community? By now we must be able to understand why such questions are rather futile. In his famous article on the discovery of oxygen Thomas Kuhn already observed that discovering something is not "a single simple act unequivocally attributable [..] to an individual and an instant in time" but "a complex process which involves recognizing both *that* something is and *what* it is".[89] Constructivists will make one further step. They will stress that the very idea of making a discovery implies, in the popular view, the validated status of that what has been discovered. This validated status, however, can only be bestowed on a certain result by the relevant scientific community in a social process of validation. It is not contained in the presumed original act of discovering something. As Harry Collins remarks, "[..] if discoveries were private things which did not need public recognition there would be as many discoveries as there are fools".[90] What makes the common-sense notion of 'discovery' unsuitable for analytical purposes in science studies is precisely the presupposition that recording and validation are both encompassed in a single act or procedure.[91] Fleck's suggestion that the Imperial Health Office was the 'true discoverer' of the pale spirochaete may have been somewhat of the mark, but he can be seen as groping toward a more adequate view when he asserted about Schaudinn's finding: "The meaning and the truth value of Schaudinn's finding is [..] a function of the community of those who, maintaining intellectual interaction on the basis of a shared intellectual past, made his achievement possible and accepted it" (40/56). The individual investigator proposes, the scientific community disposes. The category of discovery must be transferred from what traditional philosophy of science calls the 'context of discovery' to what it calls the 'context of justification', or rather, to the social context of validation and acceptance.

89 Kuhn, op. cit. (note 69), p. 171.

90 Collins, op. cit. (note 66), p. 18.

91 B. Barnes, *T.S. Kuhn and Social Science*, London (Macmillan), 1982, p. 45.

CHAPTER V

BETWEEN LABORATORY AND CLINIC: HOW THE WASSERMANN REACTION WAS MADE PRACTICALLY USEFUL (1906-1910)

1. Introduction

This chapter re-examines the early history of the Wassermann reaction which forms the central topic of *Genesis and Development of a Scientific Fact*. As Fleck explains in the Prologue of his monograph, a recently established scientific fact would be much more suitable for epistemological reflections than such commonly used examples as the facts of everyday life or classical physics: "I have therefore selected one of the best established medical facts: the fact that the so-called Wassermann reaction is related to syphilis" (XXVIII/2). Without doubt, this particular choice had also been inspired by the circumstance that as a practising bacteriologist and serologist Fleck was already thoroughly familiar with the Wassermann reaction and its mode of execution.

Fleck used the supposedly well-established fact of the relationship between the Wassermann reaction and syphilis to highlight the deficiencies of the prevalent individualistic approach in epistemology. In order to understand how a scientific discovery comes about, he argued, it is absolutely necessary to adopt a social, rather than an individualistic, point of view (76/102). From the latter perspective such an occurrence was bound to remain a miracle. Only by viewing discovery as a social event (*soziales Geschehen*) could the problem be resolved. Or so Fleck maintained.

Among the modern adherents of the sociology of scientific knowledge (SSK) Fleck is famous for this pioneering and bold step of taking a social point of view in epistemology and the study of science. But Fleck seems to be everybody's hero. He is also popular among the newer breed of constructivists like Andrew Pickering and Joseph Rouse who reject the 'social' explanations offered by SSK proponents and advocate (different versions of) a science-as-practice approach as an attractive alternative. Rouse insists on the need to examine more closely how the 'social' is being conceived in contemporary forms of (social) constructivism. He accuses SSK constructivists like Collins and Bloor of reifying the categories of the social. More specifically, he charges them with viewing scientific communities "as relatively self-enclosed, homogeneous and unengaged with other groups or cultural practices".[1] The analysis of scientific work in terms of practices, by contrast, would make it possible to understand the coherence of a scientific 'field' without relying on the problematic notion of a communally shared background consensus. Fleck's detailed account

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[1] J. Rouse, *Engaging Science: How to Understand Its Practices Philosophically*, Ithaca and London (Cornell University Press), 1996, p. 249.

of the practical work of August Wassermann and his collaborators in developing a serological test for syphilis is seen as an early paradigmatic example of the type of approach advocated by Rouse.[2] In a similar vein Pickering also interprets Fleck's case-study on the formation of the Wassermann reaction as an exemplary microstudy of scientific practice. He offers the following paraphrase of this case-study:

> "Fleck describes [the establishment of the Wassermann reaction] as a process of the reciprocal tuning of people and things. The serologists tuned the Wassermann reaction as a material procedure, adding now a little more now a little less of each reagent, letting the reaction proceed now a little longer or a little shorter, until the success rate of the test increased from 15% to 75%. At the same time a specific social community was formed: the community of disciplined practitioners competent to carry out the Wassermann reaction, having the 'serological touch', and internally differentiated in the 'quasi-orchestral' performance of the reaction".[3]

This is of course Pickering's reading of Fleck's analysis; he lays more stress on the 'tuning of people' (as distinct from the 'tuning of things') than the latter would probably allow. The quoted passage illustrates, however, which aspects of Fleck's contribution are of particular interest to adherents of a science-as-practice approach: his view of the development of the Wassermann reaction as a matter of 'tuning' the material procedure, his stress on the importance of competence and practical skills (the "serological touch"), and his view (at least according to a *prima facie* plausible reading) of the simultaneous formation of a social community of competent practitioners, which lends support to a 'de-centring' of the social.

It is important to point out, however, that in addition to this emphasis on the 'tuning' of the material procedure -which is so much admired by the adherents of the science-as-practice approach -, we also find in Fleck's monograph a conception of the so-called serologists' collective as (to use the terms employed by Rouse in the passage quoted above) "relatively self-enclosed, homogeneous and unengaged with other social groups or cultural practices". Furthermore, he thinks of this serologists' collective as sharing a common background consensus, i.e. the set of precepts making up the so-called serological thought style (64/84). We therefore have to admit that Fleck's work exhibits the same tenet that Rouse holds to be a characteristic shortcoming of SSK-type constructivism (but not of Fleck). What is more, there appears to be an intimate connection between this particular 'defect' and Fleck's presumably exemplary analysis of serological practice. How is this curious paradox to be explained? In this chapter I will defend the view that what appears to have particular

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2 Ibid., p. 185. In his earlier book, *Knowledge and Power* (Ithaca and London [Cornell University Press], 1987), Rouse had already made extensive use of Fleck's work to illustrate his conception of the practical character of scientific knowledge.

3 A. Pickering, 'Practice and Posthumanism: Social Theory and a History of Agency', Ms. (Paper presented at a workshop on 'Practices and Social Order', ZiF, University of Bielefeld, Germany, 4-6 January, 1996).

merit from the perspective of the science-as-practice approach, to wit Fleck's (almost exclusive) focus on the 'tuning' of the material procedure, is not such an unqualified virtue after all. It betrays, or so I will argue, a very inadequate understanding of the formation of the Wassermann reaction as a practically usable test for detecting syphilis. This shortcoming is indeed closely linked with the other defect in Fleck's analysis, his treating of the serologists' collective as a relatively self-enclosed community. Using Rouse's terminology, one could say that Fleck does not actually trace out the 'epistemic alignments'[4] by which serological practice is linked to relevant situations elsewhere, so that, strictly speaking, he does not elucidate at all how some locally variant experimental manipulations with blood samples came to be informative about the disease entity syphilis.[5] Another way of formulating this criticism is to say that Fleck largely ignores the 'clinical connection' and depicts the development of the Wassermann reaction as if it occurred exclusively within the four walls of the laboratory, with serologists busily "tuning their sets" (86/113). I shall present an alternative account of the whole episode in which systematic attention will be paid to the interaction between serologists and clinicians. In this way I hope to disclose the 'epistemic alignments' linking serological and clinical (diagnostic and therapeutic) practices.

Readers acquainted with Fleck's monograph might be inclined to object to my criticism that the latter treats the serologists' collective as a relatively self-enclosed community. After all, Fleck attributes an important role to the prevailing social attitude towards syphilis as a morally stigmatized disease, lending a special urgency to any attempt to find a blood test for detecting the dreaded scourge (77/102-103). He also refers to the existence of a socalled 'proto-idea' (*Uridee*) of foul syphilitic blood demanding its own realization (77/103) and speaks of "the insistent clamor of public opinion for a blood test" (77/103). In addition to this presumed public 'demand for a blood test' Fleck also posits a more narrowly construed clinically motivated interest in a serological test for syphilis as the answer to "the quandary in which physicians found themselves because of the pleomorphism of the syphilis symptoms" (6/11). So does my charge against Fleck still stand? With regard to the presumed influence of the ancient idea of syphilitic blood, striving to realize itself behind the backs of the actors involved, I think that this represents a romantic dramatization rather than a serious historical hypothesis.[6] Apart from that, it is clear from Fleck's exposition that the social

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4 Rouse, op. cit. (note 1), p. 27: "Knowing is [..] mediated by 'epistemic alignments'; skills, models, concepts, and statements become informative about their objects only when other people and things interact in constructive alignment with them."

5 Compare the following statement in Rouse, op. cit. (note 1), p 185: "Ludwik Fleck's study of the Wassermann reaction can be understood as working out how locally variant protocols for standardly identifying a condition of blood came to be informative about the historically interpreted disease entity syphilis [..]".

6 See for the reasoning behind this judgement, H. van den Belt and B. Gremmen, 'Specificity in the Era of Koch and Ehrlich: A Generalized Interpretation of Ludwik Fleck's "Serological" Thought Style', *Studies in the History and Philosophy of Science* 21 (1990): 463-79, on pp. 476-77.

attitudes and public 'demands' that figure in his analysis occupy the place of stable background factors, providing as it were the motive power that drives the efforts of the serologists' collective without however entering the central core of their activities. This holds even for the more narrowly technical interest in a blood test that he imputes to physicians with a view to facilitating the precise delineation of the disease entity and its diagnosis. Although it seems *prima facie* plausible to postulate such a clinical interest, my historical account of the development of the Wassermann reaction will show that there did not exist among clinicians a uniform 'interest' in better diagnosis *tout court*, but several variously specified 'interests', depending *inter alia* on the medical specialism of the physicians and the therapeutic doctrines they happened to hold.

My own analysis of the development of the Wassermann reaction is loosely (perhaps too loosely) inspired by Bruno Latour's ideas on 'enrollment' and 'translation of interests'.[7] Let me elaborate ('translate') Latour's ideas for the (simplified) situation in which serologists and clinicians typically find themselves, viewing it from the standpoint of the former. At no time can serologists afford to ignore their clinical colleagues. They are dependent on them for providing clinical rationales to prove the medical significance of their work, for access to 'patient material', and in a host of other ways. A project to develop a new serodiagnostic test for a particular disease can get off the ground only if the serologists succeed in securing clinical cooperation. But to get clinicians involved, their 'interest' has to be gained, that is, they have to be persuaded that their 'interests' (diagnostic or otherwise) will be served by the new test. In the course of this process 'interests' will usually become redefined ('translated') in a rather subtle way; they are to some extent malleable but not, I think, infinitely elastic. If the end-product of this development is a simple and ready-made test procedure (or perhaps a 'kit'), it can be handed over to physicians and left to their discretion. Alternatively, the test might involve such complicated procedures that its execution must remain the exclusive domain of specialized serologists (as happened with the Wassermann reaction). But in that case too, clinicians have to become convinced of the usefulness and trustworthiness of the test. They must be willing to take a detour through the serological laboratory and grant it the recognized status of an obligatory station along the route to a sound diagnosis.

To show that the exercise of capturing and translating interests is a rather delicate affair,

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7 B. Latour, *Science in Action: How to Follow Scientists and Engineers through Society*, Milton Keynes (Open University Press), 1987. In contrast to Latour himself, I use his notions as exclusively applicable to the human realm. In my view it does not make sense, except metaphorically, to state for instance that August Wassermann 'enrolled' serum antibodies, guinea pigs, and syphilis treponemas, or that he attempted to translate their 'interests'. Quite another matter is whether it would be useful in science studies to pay more attention to the (raw) material side of scientific practices. For a stimulating analysis of the relevance of research materials for the conduct of scientific investigation, see A.E. Clarke, 'Research Materials and Reproductive Science in the United States, 1910-1940', in S.L. Star (ed.), *Ecologies of Knowledge: Work and Politics in Science and Technology*, Albany (SUNY Press), 1995, pp. 183-225.

I will borrow from an article by Michael Mulkay, Trevor Pinch and Malcolm Ashmore on the so-called 'dilemma of application'.[8] Their discussion refers to the relationship between health economists and medical practitioners in contemporary Britain, but can be readily extended to the situation confronting serologists and clinicians in the early 20th century. According to these authors the delicacy of the endeavour to put scientific knowledge into practice arises from the fact that, in order to be persuaded of the usefulness of a particular piece of knowledge, participants must admit some deficiency in their current practices (for outside experts read: serologists; for participants or practitioners read: clinicians):

> "The outside experts must be able to convince participants - who, as insiders, might expect to 'know best' - that they *do not* know best. Moreover, the outsiders must convince practitioners of the inadequacy of existing practices without generating undue hostility and without thereby jeopardizing practitioners' collaboration [..]".[9]

Let me translate this 'dilemma of application' more concretely to the historical case under consideration. Serologists had to convince clinicians of the value and reliability of the Wassermann reaction, but in carrying out this task they appeared to be confronted with an insurmountable problem: If the outcome of the Wassermann reaction confirmed the clinicians' judgement, this would be fine, but it would not add anything new to what clinicians already claimed to know. The situation would be different if laboratory reaction and clinical judgement pointed in opposite directions - but in that case the serologists would be hard-pressed to persuade the clinicians of the correctness of the reaction! My historical account of the development of the Wassermann reaction will show which ways were found to deal with this dilemma.

Mulkay et al. emphasize the need not to generate "undue hostility" among the practitioners on whose cooperation outside experts will be dependent for realizing their projects. In this connection it may be relevant to mention that, according to several medical historians, in the first decade or so of the 20th century the relationship between laboratory and clinic was often strained and conflictual.[10] The 'encroachments' of new biomedical sciences like bacteriology and immunology/serology were sometimes bitterly resisted. Many clinicians were concerned that decisions on diagnosis and therapy would be transferred from the ward to the laboratory bench. In Germany, for instance, the outstanding clinician Ottomar Rosenbach had written, in 1903, a sharp indictment of the pretensions of bacteriology and

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8 M. Mulkay et al., 'Colonizing the Mind: Dilemmas in the Application of Social Science', *Social Studies of Science* 17 (1987): 231-56.

9 Ibid., p. 233.

10 See the contributions of Robert Kohler, Gerald Geison and Russell Maulitz to M.J. Vogel and C.E. Rosenberg (eds.), *The Therapeutic Revolution*, Philadelphia (University of Pennsylvania Press), 1979, pp. 27-66, pp. 67-90 and pp. 91-107.

immunology in his significantly titled book *Arzt contra Bakteriologe* (Physician versus Bacteriologist). Rosenbach criticized the bacteriologists for their eagerness "to transfer decisions from the bedside to the laboratory" and to regulate aetiology, diagnosis, and therapy "according to an artificial scheme".[11] All this would, in his opinion, injure the standing of the medical practitioner. If such strain and tension characterized the general relationship between biomedical scientists and their clinical colleagues, then of course the prescription not to antagonize the clinicians could not be easily fulfilled. In what follows we will see if there was indeed such a deep fault line between serologists and clinicians on the more limited terrain of syphilology.

A preliminary remark about the period of the history of the Wassermann reaction chosen to be covered in this chapter. When Wassermann, Neisser, and Bruck first published on "a serodiagnostic reaction with syphilis" (the title of their paper[12]) in 1906, this did not yet constitute the discovery of the 'Wassermann reaction'. Fleck too recognizes that the test had to be 'developed' (by technical improvements and modifications), before the factual relationship with syphilis could be rightly said to be established. He does not, however, locate an exact end-point in time for the completion of this process. In a sense, of course, such an end-point is always relative and somewhat arbitrary, because the process is never fully completed. In the case of the Wassermann reaction there is the additional difficulty that the theoretical unclarity as to its essential character lingered on for several decades. Disregarding this theoretical difficulty, I have chosen the year 1910 as a provisional end-point (developments beyond that year will be briefly discussed in a separate section). This has the advantage that my historical analysis will remain within the limits of the 'glorious decade' of syphilology, in which such breakthroughs as the discovery of the causative agent (1905) and the development of an effective medicine (1909-1910) also took place. A more substantial justification is that by 1910 the 'Wassermann reaction' (as it was now commonly called) had already won widespread recognition in medical circles. The circumstance that from that year on it would be used as a validation instrument in the clinical testing of Salvarsan, Ehrlich's new medicine against syphilis, testifies to this wide acceptance.

The structure of the chapter is as follows. In section 2 I will give an overview of the state of the art in medical dealings with syphilis at the beginning of the 20th century, paying special attention to some of the therapeutic doctrines that were held in specialist circles charged with its treatment. This provides the background information necessary for understanding what happened at the 'clinical pole' in the development of the Wassermann reaction. Then we move in section 3 to the serological side with a more technical exposition of the principle of the so-called complement fixation method which formed the historical

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[11] Cited in R.C. Maulitz, 'Physician versus Bacteriologist: The Ideology of Science in Clinical medicine', in Vogel and Rosenberg, op. cit. (note 10), pp. 91-107.

[12] A. Wassermann, A. Neisser, and C. Bruck, 'Eine serodiagnostische Reaktion bei Syphilis', *Deutsche Medizinische Wochenschrift* 32 (1906): 745-46.

starting-point for the development of the Wassermann test. On this basis Fleck's account of this development will be recapitulated in section 4 to have a backdrop for my own interpretation in the sections that follow. Section 5 will be devoted to deciphering the role of the German dermatologist Albert Neisser, who was very active in the initial stages of the development of a serological test for syphilis but whose exact role has not been adequately treated by Fleck. In section 6 I will critically examine Fleck's interpretation of the somewhat mysterious switch from antigen to antibody determination which occurred in the course of attempts to develop a clinically useful test. Close examination of the work of the serologist Julius Citron will largely resolve this riddle. The decisive step in the development of the Wassermann reaction was not so much a matter of 'tuning' the material procedure as of finding a new clinical meaning for antibody determination. In the process Citron also found ways to deal with the 'dilemma of application'. Section 7 continues with discussing the work of other serologists (and clinicians) who followed in Citron's track in order to overcome this dilemma. In section 8 I will inquire, with the slightly dubious benefit of using present-day insights, whether the relationship between the Wassermann reaction and syphilis was actually such a well-established medical fact as Fleck and his contemporaries thought it was. Finally, in section 9 I will formulate some general conclusions about viewing science as practice, the conception of the 'social', the usefulness of interest explanations and the role of raw materials on the basis of this particular case-study.

2. Clinical background

To call the period 1900-1910 the 'glorious decade' of syphilology involves some injustice to the large amount of effort that had been spent by the previous century in obtaining a more adequate knowledge of the dreadful disease. Without this preparatory work, the great breakthroughs at the beginning of the 20th century would have been unthinkable. At the turn of the century there was available a more or less complete 'clinical picture' of syphilis, which provided the starting-point for the discoveries and achievements that followed.

The clinical picture of syphilis had been gradually rounded out in the course of the 19th century. It was as if all efforts had been oriented toward a common purpose, namely, to use Fleck's felicitous phrase, the precise *Herausmeisselung* (chiseling out) of the disease entity (6/11). First, syphilis had to be clearly distinguished from the other venereal diseases, gonorrhoea and ulcus molle. Clinical experience and inoculation experiments brought some enlightenment on these matters. Contributing to further enlightenment was the discovery of the specific agent of gonorrhoea in 1879 by the then 23-year-old assistant to the Breslau dermatological clinic, Albert Neisser, whom we will meet again in later stages of his career (the microbe was dubbed 'gonococcus' by Neisser's former classmate at the Breslau *Gymnasium*, Paul Ehrlich). After the causative agent of ulcus molle had also been identified in 1889 by Augusto Ducrey, the separation of syphilis from the other two venereal diseases was definitive.

Traditionally, syphilology is intimately linked with dermatology. An earlier generation of clinical investigators had patiently described and catalogued all the various cutaneous manifestations which syphilis shows in its 'secondary' and 'tertiary' stages (a terminology in accordance with Philippe Ricord's [1800-1889] division of the natural course of the disease into three stages). Since then medical students had to be thoroughly trained in distinguishing syphilitic eruptions from closely resembling non-syphilitic skin diseases. For Albert Neisser, director of the dermatological clinic in Breslau since 1882, this circumstance provided a motive to decline the vacant dermatology chair in Berlin offered to him, as he explained in 1890 in a letter to the all-powerful Prussian official Friedrich Althoff:

> "Of the one hundred questions, which force themselves on the specialists or the practical physicians working in the relevant area during their daily practice, at least ninety turn on the decision: syphilis or no syphilis? The fact that syphilis mimicks nearly all known socalled idiopathic diseases implies that in any instance of cutaneous eruption first of all the differential diagnosis with respect to syphilis has to be made and examined in depth. A separation of the two subjects in a syphilis-clinic and a skin-diseases clinic [as corresponded to the situation in Berlin - HvdB], however, makes it impossible for the lecturer to teach the students precisely that which he should impart on them as the first and most important thing."[13]

Actually, Neisser was already involved in planning a new, carefully designed and well-equipped dermatological clinic in Breslau, which was opened in 1892 and became before long an internationally famous centre of training and research.[14] Syphilitic patients at this clinic would later be the first to provide human sera for the sake of finding a serodiagnostic reaction.

In addition to the special association with dermatology, syphilology also forged links with other medical specialties, because the disease not only caused eruptions on the skin but could also affect internal organs and organ systems ('visceral syphilis'). Indeed, as more and more effects of syphilis were tracked down, it became apparent that hardly any part of the human body, with the exception of the crystalline eye lens, was immune to its devastations. Particularly important was the recognition that an often lethal affection of the aorta, aneurysm, could also be traced to a syphilitic origin (Welch, 1875).

Syphilology also entered the realm of neurology. In the 1870s the famous French syphilologist Alfred Fournier (1832-1915) suggested, on the basis of statistics from his own private clientele, a connection between syphilis and tabes dorsalis - popularly known as spinal

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13 Cited in S. Schmitz, *Albert Neisser: Leben und Werk auf Grund neuer, unveröffentlichter Quellen*, Düsseldorf (Michael Triltsch Verlag), 1967, p. 25.

14 A. Scholz and G. Sebastian, 'Albert Neisser and His Pupils', in: J.J. Herzberg and G.W. Korting (eds.), *Zur Geschichte der Deutschen Dermatologie/On the History of German Dermatology*, Berlin (Grosse Verlag), 1987, pp. 167-77.

consumption, "a terrifying name for patients and laymen".[15] In the 1890s Fournier traced another affection of the central nervous system, 'general paresis of the insane' (also known as 'progressive paralysis', 'dementia paralytica' or popularly as 'softening of the brain') or general paresis in present-day terminology, to a syphilitic origin. The connections of tabes and paresis with syphilis were only reluctantly accepted or sometimes even, as in the case of the great Charcot, rejected out of hand. The latter accused Fournier of "misappropriation of neurologic property".[16] Although the statistical evidence was impressively robust (Fournier's data were later supplemented by those of Wilhelm Erb), the problem was that the relationship between the two neurological disorders and syphilis was not easily understandable. Between the syphilitic infection and the onset of tabes or paresis perhaps two or three decades could elapse, long years often passed in apparent good health. Many authors did not consider tabes and paresis as tertiary forms of syphilis, but called them 'para-syphilitic' or 'post-syphilitic' affections.[17] The connection with syphilis was thereby somewhat loosened theoretically, but not denied in actual practice.

In the years after 1900 several attempts were made to determine statistically how many syphilitic patients died from syphilis. The results differed widely, but the estimates obtained in 1904 by the dermatologist Alfred Blaschko (1858-1922), general secretary of the German Society for the Control of Veneral Diseases, on the basis of material from the Berlin life-insurance company *Victoria* were considered the most authoritative. According to Blaschko, some 33 percent or one-third of all syphilitics died of either tabes, paresis, or aortic aneurysm (6.7, 24, and 2.7 percent, respectively). The estimated incidence of syphilis was also high; in Blaschko's calculation, 20 percent of all men in the larger German towns contracted the disease during their lives.[18] To judge from these figures, syphilis must indeed have exacted a formidable death-toll.

However, according to more recent views, Blaschko's figures on the prognosis of syphilis must have been too pessimistic. We have gained a better insight into the 'natural history' of the disease thanks to the famous 'Oslo study' of untreated syphilis. During the years 1890-1910 syphilitic patients in the city of Oslo were diagnosed and hospitalized but not given antisyphilitic treatment, because the physician in charge, Cesar Boeck (1845-1917),

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15 P.K. Pel, 'Die Aetiologie und Therapie der Tabes dorsalis', *Berliner klinische Wochenschrift* 37 (1900), p. 629.

16 Cited in J.T. Crissey and L.C. Parish, *The Dermatology and Syphilology of the Nineteenth Century*, New York (Praeger), 1981, p. 222.

17 Pel, op. cit. (note 15), pp. 662-63.

18 A. Blaschko, 'Syphilis und Lebensversicherung', *Zeitschrift für die gesamte Versicherungswissenschaft*, 1904; C. Bruhns, 'Die Lebensprognose des Syphilitikers', *Berliner klinische Wochenschrift* 44 (1907): 1147-52. For an overview of different estimations in different countries, see J. Cassel, *The Secret Plague: Venereal Disease in Canada 1838-1939*, Toronto (University of Toronto Press), 1987, pp. 17-21.

was convinced that the remedy (mercury!) was worse than the disease. After the surface lesions had been healed, the patients would be discharged from hospital. Many years later, in the 1940s, most of these former patients were traced by Gjestland, who reconstructed their further vicissitudes in life or the probable causes and circumstances of their deaths. This comprehensive investigation showed that about 83 percent of all those initially diagnosed as syphilitically infected, when untreated, continue their lives without confronting serious health problems deriving from this infection.[19] The chances of developing, in case of untreated syphilis, a truly serious affection of the central nervous system are 6.5 percent; of developing an impairment of the cardio-vascular system, 10 percent. Walsh McDermott even declares, perhaps somewhat light-heartedly, that the disease of which former generations had had to live with such a devastating fear, turns out to have been "largely a paper tiger".[20] The generation living at the beginning of this century, however, by no means considered syphilis a relatively minor inconvenience. The evil prospects for developing 'spinal consumption' or 'softening of the brain' in later life were taken deadly seriously.

The fact that the disease could also be transmitted to the 'innocent' children, or lead to spontaneous abortions and stillbirths, added to its stigma. As Crissey and Parish write, "(..) congenital syphilis was for everyone concerned a confrontation with reality in its harshest form".[21] A literary author like Henrik Ibsen had exploited the theme of the 'sins of the fathers' in his play *Ghosts* (1881). There was as yet no clear recognition that the mode of transmission was not strictly hereditary in nature.

Incidentally, both the (from our point of view) unduly pessimistic prognosis of the disease and the prevalent perception of it as being hereditary in nature contributed to the special sense of urgency which public opinion in the early 20th century attached to the syphilis question. Syphilis became strongly associated with moral and physical degeneration.[22] It was, according to Fleck, this special moral emphasis on syphilis that would give a powerful impetus to research on the Wassermann reaction (77/102).

After our reconstruction of the clinical picture of syphilis around 1900, we can proceed by reviewing the therapeutic arsenal that was available for dealing with the dreaded scourge. The outstanding fact about the disease, that every therapeutic intervention had to confront, was its insidious character. "What marks syphilis as an exceptionally gruesome (*unheimliche*) disease", Hermann Eichhorst wrote in 1891, "is the circumstance that one is never secure

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19 T. Gjestland, 'The Oslo Study of Untreated Syphilis: An Epidemiologic Investigation of the Natural Course of the Syphilitic Infection Based upon a Re-Study of the Boeck-Bruusgaard Material', *Acta Dermatology and Venereology* 35 (1955), Supplement 34 (Stockholm).

20 W. McDermott, 'Evaluating the Physician and his Technology', *Daedalus* 106 (1977): 135-57, on p. 147.

21 Crissey and Parish, op. cit. (note 16), p. 92.

22 See Chapter III for an exploration of this theme.

from relapses".[23] Acute eruptions alternate with 'latent' periods without symptoms. As a consequence, patients who thought themselves to be completely healed were often taken aback by a new onslaught of the disease. All these uncertainties were aggravated and overshadowed by the chances of developing late-syphilitic affections like aortic aneurysm and 'post-syphilitic' disorders like tabes and paresis in later life.

Within the medical orthodoxy, therapeutic intervention was based on two substances, mercury and iodine, of which several compounds or preparations were used in a variety of treatment schemes differing in mode of administration, dosage, intensity and time schedule. Mercury was thought to do the basic anti-syphilitic work. Iodine was used in a supplementary role; it was particularly helpful in suppressing the visible manifestations of tertiary syphilis. Mostly outside the confines of official medicine (with only a few supporters within) there was a large contingent of so-called *anti-mercurialists* who scorned the use of mercury and iodine. "Those eccentrics", as Eichhorst referred to them, "recommend hunger, purgation, and sweating cures, and also in particular decoctions prepared from guaiacum, sarsaparilla, and sassafras".[24] In the eyes of the representatives of official medicine they were *Kurpfuscher* (quacks) who cleverly exploited the public's fear of mercury or 'hydrargyrophobia' (Oscar Lassar) and thereby prevented many patients from receiving the correct kind of treatment. One should have no illusions as to the effectiveness of the remedies proposed by the anti-mercurialists, but the painful question was whether the use of mercury was so much more effective that its wholesome consequences could outweigh its many negative side-effects.

The negative side-effects were duly noted in medical handbooks. These included hazards like salivation, gastroenteritis, stomatitis, anemia, depression, teeth falling out, liver and kidney diseases, and even general mercurial intoxication. Specific modes of administration added specific risks. Oral administration endangered stomach and bowel in particular, inunction cures (*Schmierkuren*) could lead to contact dermatitis. Injections (*Spritzkuren*), both subcutaneous and intramuscular, were extremely painful and could lead to abscesses and systemic reactions. Although few patients could bear these injections, they were held by some specialists to be superior to an inunction cure, "which the physician can only prescribe,

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23 H. Eichhorst, *Handbuch der speciellen Pathologie und Therapie für praktische Aerzte und Studirende*, IV. Band: Krankheiten des Blutes und Stoffwechsels und Infektionskrankheiten, Vienna and Leipzig (Urban & Schwarzenberg), 1891, p. 604.

24 Ibid., p. 612. It is remarkable that the remedies and cures applied by the antimercurialist fringe of the medical profession around 1900 were in earlier periods of history standard recipes of *official* medicine. See O. Temkin, 'Therapeutic Trends and the Treatment of Syphilis before 1900', *Bulletin of the History of Medicine* 29 (1955): 309-16. On p. 312 Temkin observes: "I have the impression that the popularization of potassium iodide through Wallace and Ricord since 1838 made these decoctions [Temkin refers to "gaiacum, sarsaparilla and other 'sudorific woods'"] fade into near oblivion." Rather than fading into near oblivion, it seems that these 'decoctions', after falling from grace in the official medical world, could live on in the underworld of unofficial medicine.

but of which he cannot assure the correct implementation right to the end".[25] Compliance with the doctor's prescriptions could not be taken for granted. The public's fear of mercury, professionally dismissed as 'hydrargyrophobia', was not completely unfounded.

Although the official medical community fully agreed on the use of mercury as a remedy against syphilis, disagreement became paramount when the proper scheme of treatment was at issue. Disregarding the finer shades, we can distinguish two main schools in German dermatological and syphilological circles around the turn of the century. The first school consisted of the adherents of the so-called *chronic-intermittent method*, according to which antisyphilitic therapy should follow a fairly rigid schedule: once the diagnosis had been established, mercury treatments consisting of several inunction or injection cures should be applied at fixed intervals in time. Disappearance of the visible symptoms of syphilis was no reason to terminate the therapy. An outstanding proponent of this therapeutic method was Albert Neisser. The second school consisted of the adherents of the so-called *symptomatic-expectative method*, according to which mercury treatment was indicated only when symptoms were present. Alfred Blaschko was a prominent member of this school. Proponents of the first school were often accused of substituting a rigid schematism for the physician's art and judgement. The adherents of the chronic-intermittent method, by contrast, found a rationale for their rigid approach in the unpredictable course of syphilis: disappearance of symptoms was no sign of definitive healing. They thought that a failure to treat syphilis vigorously in the early stages would lead to a higher likelihood for tabes, paresis, and aneurysm later on. In a sense, the rigid schedule also bound the practising physician to a regimen and perhaps preserved him from vacillation toward his patients. For the latter would only be too eager to discontinue treatment after the visible symptoms had vanished.

The disagreement between the two schools could find no easy resolution. This reflected the unenviable dilemma physicians faced with regard to the treatment of syphilitics. They either risked pushing too far, or not far enough. The patient's health could be seriously endangered by mercury's harmful effects without being preserved from the later onset of tabes, paresis or aortic aneurysm. The physicians were only liberated from their desperate predicament after 1910, when Ehrlich's arsphenamine (Salvarsan) replaced mercury. With regard to the professional disagreement on the proper therapeutic scheme, Crissey and Parish observe: "(..) the matter had not been settled to the satisfaction of anyone when the discoveries of Ehrlich finally rendered the whole business irrelevant".[26] In later sections, however, we will see that the therapeutic disagreement existing in clinical circles has been a very relevant factor in the development of the Wassermann reaction; and that, for its part, this diagnostic test has also been instrumental in bringing about a certain *rapprochement*

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[25] F. Block, review of Max Joseph, Lehrbuch der Haut- und Geschlechtskrankheiten, *Deutsche medizinische Wochenschrift* 31 (1905), p. 606.

[26] Crissey and Parish, op. cit. (note 16), p. 360.

between the rival schools, if not a complete settlement of the matter. By the time that Ehrlich's Salvarsan would make the entire issue obsolete, the Wassermann reaction had already contributed to its resolution and won sufficient clinical acceptance to be used as the main validation test for the magic bullet.

Let us then turn now to a detailed analysis of the genesis and development of the Wassermann reaction.

3. The principle of the Wassermann reaction: complement fixation

Whereas the diagnostic and especially the therapeutic practices of dermatologists and other clinicians were organized around the mercurial treatment of syphilitic patients, the laboratory practice of serology was organized around the 'experimental system' of *immune hemolysis*.[27] In the course of the development of the Wassermann reaction (henceforth: WaR) both types of practices became more and more strongly connected to each other. Indeed, one may assert that it is only through such 'epistemic alignments' (Rouse) that some locally variant experimental manipulations with blood samples came to be informative at all about the disease entitity syphilis.

According to the historian of immunology, Arthur Silverstein, after its discovery just before 1900 the phenomenon of immune hemolysis was rapidly converted to a full 'experimental system', which opened new avenues for research and practical application.[28] This choice of terminology is significant because in Rouse's view a laboratory is precisely a locus for the construction of 'experimental systems' or 'phenomenal microworlds'.[29] The new experimental system of immune hemolysis was to become the vehicle for a lively debate between Bordet and Ehrlich on immune mechanisms and the nature of complement, and would lead to Landsteiner's discovery of the ABO blood groups in 1901-1902, to the identification of paroxysmal cold hemoglobinuria (PKH) as the first autoimmune disease, and to the development of a serodiagnostic test for syphilis.

Immune hemolysis is the phenomenon that the serum of animals (say guinea pigs) which have been injected with the erythrocytes (red blood cells) of another animal species (say sheep) is able to dissolve those foreign blood cells when mixed with them in the test tube. This visible 'lytic' effect (hence 'hemolysis') is based on the combined action of two factors: specific antibodies against the foreign erythrocytes (so-called 'hemolysins') and a non-specific

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27 A.M. Silverstein, 'The Heuristic Value of Experimental Systems: The Case of Immune Hemolysis', *Journal of the History of Biology* 27 (1994): 437-47.

28 Ibid.

29 Rouse, op. cit. (note 1), p. 101. In his later work (op. cit. [note 2], p. 129), Rouse uses the expression 'experimental system' as an equivalent to 'phenomenal microworld'.

factor called 'complement'. It was found by the Belgian immunologist Jules Bordet that the same 'complement', which is also present in the serum of untreated animals but can be neutralized by heating to 56° C, is also a necessary factor in other immune reactions involving different antibodies and 'antigens' (i.e., all those foreign substances including microbes or microbial products that provoke the production of specific antibodies when they enter a human or animal organism).[30] This suggested the setting up of an elegant two-stage test design using hemolysis as an indicator during the second stage for non-specifically measuring any mixture involving an antigen and its specific antibody. If one had a known antiserum, one could use it and the hemolytic indicator system to test for antigen. If no hemolysis occurs during the second stage, then the available 'complement' has apparently been used up or 'fixed' during the first stage, indicating the presence of a specific antigen with which the corresponding antibody in the serum must have reacted. Conversely, if a known antigen were used, one could likewise test for the presence of specific antibody. This is the principle of the so-called *complement fixation test.* It can be used either for antigen determination (*Antigennachweis*) or for antibody determination (*Antikörpernachweis*). In this respect the test design is perfectly symmetrical.

Bordet's complement fixation method was eagerly adopted by August Wassermann and his collaborators working at the Royal Institute for Infectious Diseases (Robert Koch's prestigious institute) in Berlin.[31] Wassermann and Bruck introduced a modification that would further extend the applicability of the test. They claimed to have shown for such diseases as typhoid, tuberculosis and meningitis that the test worked not only when suspensions of entire bacteria were used as antigen, but also when so-called 'bacterial extracts' were employed for this purpose (their experiments met with some criticism from other serologists[32]). The modification of Wassermann and Bruck was relevant for syphilis because the causative agent *Spirochaeta pallida* (now *Treponema pallidum*) had, from its discovery in 1905, resisted all attempts at cultivation.

In applying the complement fixation test to syphilis, Wassermann and Bruck employed as antigen, at least initially, extracts from organs that were rich in spirochaetes, in particular extracts from the livers of still-born syphilitic babies.[33] It was known that such livers

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30 J. Bordet and O. Gengou, 'Sur l'existence de substances sensibilisatrices dans la plupart des sérums antimicrobiens', *Annales de l'Institut Pasteur* 15 (1901): 289-302.

31 W. Kolle and A. Wassermann, 'Versuche zur Gewinnung und Wertbestimmung eines Meningococcenserums', *Deutsche medizinische Wochenschrift* 32 (1906): 609-12; A. Wassermann and C. Bruck, 'Experimentelle Studien über die Wirkung von Tuberkelbacillen-Präparaten auf den tuberculös erkrankten Organismus', *Deutsche medizinische Wochenschrift* 32 (1906): 449-54.

32 Especially Eduard Weil and his co-workers from the Hygienic Institute of the German University in Prague.

33 Eventually, it was found that it was not even necessary to use 'antigen' of syphilitic origin. This remarkable turn of events will be discussed in later sections.

literally swarmed with spirochaetes, which according to one commentator made syphilitic foetuses a "much sought-after article" (*gesuchter Artikel*) in 1905, "the year of the spirochaetes".[34] To obtain these research materials in the required quantity, Wassermann and his acollaborators had to mobilize an extensive network of contacts. In their first research paper they expressed their acknowledgements to no less than *thirty-six* persons who had provided them with syphilitic foetuses. Other materials (including sera) were obtained from the patients of Neisser's clinic and from (nonhuman) primates that were kept in Breslau.

The above exposition has presented only the principle of the complement fixation test and the WaR, but did not enter into the technical details of their practical execution. Of these, the quantitative aspects are most important. For one thing, the required quantity of complement had to be precisely determined by titration (if there were too much complement, it would still be available for the second stage despite the occurrence of a specific reaction during the first stage). More generally, accurate quantitative proportions of all 5 'reagents' involved in the test had to be established. Moreover, the WaR was also susceptible to many disturbing factors. To eliminate these possible sources of error, serologists had to operate with so-called 'controls'. For Fleck, a strong emphasis on the methodical necessity of 'controls' was a general characteristic of the serological thought style (63/84). In the actual implementation of the WaR, about 8 to 10 parallel tests had to be conducted in addition to the main test, which amounted to a very cumbersome task. This elaborate ritual of 'controls' was thought necessary to keep disturbing influences at bay.

4. Fleck's account of the development of the Wassermann reaction

In this section I will describe Fleck's view on the history of the WaR as a backdrop for my own interpretation in later sections. Fleck uses the example of the WaR to bolster his case for a sociological approach in the theory of knowledge. In his opinion, we should abjure the common individualistic point of view in epistemology.

Fleck points out that the decisive stimulus to apply the complement fixation method to syphilis was given by the well-known German official Friedrich Althoff, head of the Prussian Ministry of Education and Culture.[35] Althoff feared that Germany might fall behind France in the experimental research on syphilis and therefore stimulated Wassermann to work in this area. International rivalry in this domain was an important social motive, expressed through

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[34] H. Beitzke, 'Ueber Spirochaete pallida bei angeborener Syphilis', *Berliner klinische Wochenschrift* 43 (1906): 781-84.

[35] For more information on Althoff, see B. vom Brocke, 'Friedrich Althoff: A Great Figure in Higher Education Policy in Germany', *Minerva* 29 (1991): 269-93.

Althoff's mouth as a kind of 'vox populi' (68/90).[36]

The development of a serodiagnosis for syphilis was not the work of a single individual, Fleck asserts, but of an organized collective. The polemic which erupted in 1921 and in which several protagonists claimed the intellectual authorship of the WaR (Wassermann and Bruck among them), was therefore utterly futile in Fleck's eyes: the authorship essentially belonged to the entire collective and not to any particular individual (78/104).[37]

Fleck further observes that in the development of the WaR the emphasis gradually shifted from antigen determination to antibody determination. In their first two papers, published in 1906, Wassermann, Neisser, and Bruck declared that they had discovered a specific reaction between syphilitic antigen and syphilitic antibody.[38] The *primary* aim of their investigation, according to Fleck, was the detection of antigen in syphilitic organs and syphilitic blood. Antibody determination was, again according to Fleck, considered of *secondary* importance only (70/92). The quantitative results obtained may explain why the authors laid so much more stress on antigen than on antibody determination. Antigen determination was successful in 64 out of 76 confirmed syphilitic cases, but antibody determination only in 49 out of 257 cases (that is, in 19 percent). Small wonder then that the emphasis was put so much more on the former!

In 1907 several serological investigators (Landsteiner, Marie, Levaditi, Weil, Braun, and others) reported the curious fact that the WaR worked just as well when extracts from a normal (non-syphilitic) organ were used as 'antigen' instead of syphilitic liver extracts (73-74/97). The idea of the original authors that they had found a specific antigen-antibody reaction for syphilis was now rejected. Antigen determination, which initially had shown such promising results, had to be abandoned too. One can easily imagine the story to have ended here, but it took a surprisingly new track. That was largely because antibody determination, strangely enough, would enjoy considerable progress. Whereas the number of positive outcomes in cases of confirmed syphilis was at the start barely 20 percent, this percentage would eventually increase to about 70 or 90. According to Fleck, it was this increase that "represented the actual invention of the Wassermann reaction as a useful test" (72/95).

How did the increase come about? Fleck emphasizes that this turning point is largely shrouded in mystery: "The moment when this decisive turn occurred cannot be accurately

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[36] For his description of Althoff's role Fleck relies on a statement made by Wassermann in 1921. He notes, however, that Bruck presented a different version of the beginnings of the serodiagnosis of syphilis.

[37] The struggle over the intellectual property of the Wassermann reaction will be analyzed in Chapter VI.

[38] A. Wassermann, A. Neisser and C. Bruck, 'Eine serodiagnostische Reaktion bei Syphilis', *Deutsche medizinische Wochenschrift* 32 (1906): 745-46; A. Wassermann, A. Neisser, C. Bruck and A. Schucht, 'Weitere Mitteilungen über den Nachweis spezifisch luetischer Substanzen durch Komplementverankerung', *Zeitschrift für Hygiene und Infektionskrankheiten* 55 (1906): 451-77.

determined. No authors can be specified who consciously brought it about. We cannot state exactly when it occurred nor explain logically how it happened" (72/95). Nor is the testimony of those who had been directly involved very helpful in Fleck's opinion. They can tell us no more than that the technique had first to be worked out. It can be readily conceded that the test had to be "technically perfected". The reagents had to become more precisely matched by improving the titration methods. The reading of the outcome had also to be perfected to determine which degree of hemolysis inhibition represented a positive or a negative outcome. Fleck insists however that this technical perfecting of the instrument can be understood only if it is viewed as the work of an anonymous collective. It was largely a matter of finding a proper balance, mutually adjusting the various reagents and learning how to read the results (72/96). He suggests that a satisfactory trade-off between the required sensitivity and specificity of the reaction was also brought about by a kind of collective balancing act. Sometimes, when the reaction was oversensitive, the specificity of the test was too low ("there were too many positive results even with non-syphilitics"); at other times the sensitivity of the test was insufficient ("there are too many negative [results] even with syphilitics"):

> "The optimum intermediate position between minimum nonspecificity and maximum sensitivity had to be gradually established. This, however, is entirely the work of a collective consisting mostly of anonymous research workers, adding now 'a little more', now 'a little less' of a reagent, allowing now 'a little longer', now 'a little shorter' reaction time, or reading the result 'a little more' or 'a little less' accurately" (72-73/96).

The idea appears to be that the collective, by constantly 'tuning' the material procedure (cf. Pickering as quoted in the introductory section!), gradually tinkered its way toward success, until, supposedly, it finally reached the golden mean between insufficient sensitivity and too low specificity.

The important point for Fleck is that this gradual improvement is only thinkable as a process of gathering *collective experience.* He himself uses the metaphor of 'tuning' in this connection: "It is also clear that from these confused notes [the ambiguous results of the first experiments - HvdB] Wassermann heard the tune that hummed in his mind but was not audible to those not involved. He and his co-workers listened and 'tuned' their 'sets' until these became selective. The melody could then be heard even by unbiased persons who were not involved" (86/113). Fleck tends to view the serologists' collective not as a community of practitioners working at different institutional locations, but as one big team cooperating in "quasi-orchestral" harmony.

According to Fleck the intense collective work that eventually made the WaR practically useful proceeded "with disregard for theoretical questions and the ideas of individuals" (73/97) and was undertaken as a consequence of the special social importance of the syphilis question (77-79/ 102-04). The same social perception of urgency was at the basis of "insistent clamor of public opinion for a blood test" (77/103). Apparently, this clamour was insistent enough to secure eventually its own fulfilment.

5. The involvement of Albert Neisser

It is remarkable that Fleck has nothing more to say about the contribution of Albert Neisser (1855-1916), Germany's most prominent dermatologist at the time, to the development of the WaR than simply this: "Neisser offered the pathological material and his experience as a physician" (69/91). Neisser's involvement, as a non-serologist, in the formation of the serodiagnosis of syphilis from the very beginning, would appear to merit a much more thorough-going analysis. If we are interested in highlighting the importance of the 'clinical connection' for the development of the WaR, we cannot permit to ignore Neisser's role in this. It will turn out that Neisser's concerns were highly idiosyncratic and not at all representative of the average clinician, but that is of course no reason for passing in silence over his contribution. Rather, the fact that the 'clinical pole' in the development of the WaR was apparently not uniform and monolithic gives occasion to further reflection.

Although Neisser, as a 'physician', represented the clinical side in the cooperation with Wassermann and Bruck, his whole medical and scientific career seems to mock at Ottomar Rosenbach's formula *Artz contra Bakteriologe*. After all, as we mentioned above, Neisser had started his career in 1879 with the discovery of the 'gonococcus', the specific agent of gonorrhoea, before he became director of the Breslau dermatological clinic in 1882.

Rosenbach himself must have known that his formula 'physician versus bacteriologist' was less than adequate when applied to the medical dealings with syphilis, for in 1903 he published yet another polemical work, *Das Problem der Syphilis*, of which a revised edition appeared in 1906.[39] In it he criticized what he called the abuses of the "monistic way of thinking" which attempted to trace various kinds of affections back to a syphilitic origin and syphilis itself to a tangible microbic agent (he himself advocated a so-called 'energetic' approach, both for diagnosis and for therapy). His strongest denunciations were saved for what he called "the myth of the specific action of mercury and iodine". He commented sarcastically: "the only thing that is specific about the specific remedies against lues [=syphilis] seems to be that they will never, however often they may prove ineffective, lose their reputation".[40] By this criticism Rosenbach made himself into a mouthpiece of the *anti-mercurialists* who were an influential category outside the official medical world. As a staunch adherent of the chronic-intermittent method in mercury therapy, Neisser was clearly

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39 O. Rosenbach, *Das Problem der Syphilis und kritische Betrachtungen über ihre Behandlung*, second, enlarged edition, Berlin (Hirschwald), 1906 (reviewed in *Berliner klinische Wochenschrift* 44 [1907]: 577-78).

40 O. Rosenbach, 'Genügt die moderne Diagnose syphilitischer Erkrankungen wissenschaftlichen Forderungen', *Berliner klinische Wochenschrift* 43 (1906): 1157-60, on p. 1158.

implicated in Rosenbach's ridicule of the specificity of mercury and iodine.

For Rosenbach, the 'abuses' in the medical dealings with syphilis were the consequence of the prevailing "sway of the specialists" (*das Spezialistentum*). He strongly opposed the tendency to make the treatment of syphilis patients into a reserve for medical specialists; the general practitioner, the "physician without epitheton ornans", should also be active in this field. It thus seems that on the domain of syphilis there was not so much a dividing line of 'physician versus bacteriologist', but rather a division between specialists and general practitioners, or perhaps only between official medicine and quackery. Rosenbach was fully aware of the fact that his criticism of the "monistic way of thinking" in the diagnosis and therapy of syphilis enjoyed little support from the official medical world of the specialists. His was an outsider's criticism.

It will by now be clear that such a prominent representative of *das Spezialistentum* as Albert Neisser could hardly be suspected of hostility toward bacteriology and serology. Still we may wonder why he was interested in developing in particular a serological test for syphilis. Fleck fails to ask some simple questions: How did Neisser got involved and what was his motive for cooperating with Wassermann and Bruck? What could he possibly have expected from something like a "serodiagnostic reaction with syphilis"[41]? Perhaps Fleck did not ask these questions because he thought the answers to be obvious. We will see that they are not.

From the historical records it is not clear whether it was Neisser or Wassermann who took the initiative to contact the other and to enter into a closer cooperation. The statements of Wassermann and of his former co-worker Bruck, expressed during the fierce polemic on the authorship of the WaR in 1921, are diametrically opposed on this score (Neisser had died in 1916). According to Bruck it was Neisser who, during a visit to Wassermann, first envisaged the possibility to develop a serodiagnosis for syphilis. In that case it would have been the clinician who tried to 'enroll' the serologist and not vice versa! (So who 'enrolled' whom?) Wassermann countered Bruck's statement with the assertion that it was the Prussian official Friedrich Althoff who, in the final analysis, had given the decisive stimulus for the development of the WaR (Althoff had died in 1908).[42] Fleck relies on Wassermann's assertion to stress, as we have seen, the social motive behind the work on the serodiagnosis of syphilis. Neither Wassermann's nor Bruck's statement should be simply accepted at face value, however. Regrettably, Neisser's biographer, Sigrid Schmitz, does not clear up the matter, for she relies for her description of the contact between Wassermann and Neisser not on direct sources but on obituaries written by Bruck and others.[43] One can also speculate,

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41 The title of their first publication on the WaR.

42 A. von Wassermann, 'Zur Geschichte der Serodiagnostik der Syphilis (Bemerkungen zu den Bruck'schen Ausführungen)', *Berliner klinische Wochenschrift* 58 (1921): 1194-95, on p. 1195.

43 Schmitz, op. cit. (note 13), p. 49.

of course, that Althoff has exerted pressure on *both* investigators, Wassermann and Neisser, to pool their resources for the sake of Germany's greater role in the experimental research on syphilis. But even if this were the case, one could still ask what particular motives the investigators had to act upon Althoff's suggestion.

For Neisser as a 'clinican' such a motive appears rather easy to construe. After all, as Fleck describes, physicians found themselves in a diagnostic quandary "because of the pleomorphism of the syphilis symptoms" and this produced "a general and urgent 'demand for blood tests' as the means to identify this disease entity with precision" (6/11). And what purpose should a "serodiagnostic reaction with syphilis" serve if not a more precise identification of the disease entity? However, I think it was not simply a new diagnostic test that Neisser was looking for. From his previous involvement in the medical and scientific dealings with syphilis an entirely different motive for his contribution to the development of the WaR can be construed. Here follows my reconstruction.

Although Neisser had always been an adherent of a vigorous mercurial treatment of syphilis, its many undesirable side-effects had also motivated him almost from the start to look for a viable therapeutic alternative. He was therefore very much impressed by the so-called *serum therapy* used by Emil Behring in 1891-92 as a cure for diphteria. The success of this therapy was crucially dependent on the *strength* of the immune serum. In the shadow of Behring's fame, Paul Ehrlich had actually indicated the road to practical success. To achieve the highest possible immunity in the test animal for the sake of a potent antiserum, it was necessary to administer steadily increasing doses of diphteria toxin to the animal.[44] After Behring's success with diptheria, Neisser immediately tried to apply his own version of 'serum therapy' to syphilis. He inoculated 5 young prostitutes with cell-free serum from syphilitic patients, hoping thereby to prevent them from infection in their future occupational careers. He also inoculated 3 young women that were not active in prostitution. Despite Neisser's 'serum therapy' 4 of the 5 prostitutes subsequently contracted syphilis. He attributed these infections to their occupation and not to his inoculation, but at any rate it was clear that his attempt at immunization had failed. When Neisser published the results of his experiment with some delay in 1898, a veritable scandal - which became known as the 'Neisser affair' - broke out. Neisser was made into a scapegoat who had to pay for the abuses that were common in German hospitals (and the affair was also abetted by antisemitic sentiment). In the end he was officially rebuked and fined. Thanks to Friedrich Althoff's protection, the damage could be kept within limits.[45]

The whole affair made it clear to Neisser that henceforth experiments on human subjects, for the sake of finding a possible immunity against syphilis, would be blocked. He

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44 H. Satter, *Emil von Behring*, Bad Godesberg (Inter Nationes), 1967, p. 26.

45 B. Elkeles, 'Medizinische Menschenversuche gegen Ende des 19. Jahrhunderts und der Fall Neisser', *Medizinhistorisches Journal* 20 (1985): 135-48, on p. 139.

needed "another experimental animal".[46] In 1902 he tried to transmit syphilis to pigs, but without success. Other attempts to artificially infect various animal species by many different researchers had also failed. The idea that syphilis was a uniquely human disease which could not be transmitted to other animals began to take hold.

New perspectives were opened however in December 1903, when Metchnikoff and Roux of the Paris *Institut Pasteur* declared that they had succeeded in transmitting syphilis to two chimpanzees.[47] Soon it became apparent that the disease could also be transmitted to other primates. Neisser eagerly exploited the new research opportunities that were offered by the newly available 'animal models'. In the year 1904 he conducted extensive experiments in Breslau on 9 chimpanzees, 4 orang-utans, one gibbon and 7 macaques. One chimpanzee was injected with large quantities of serum from a fresh syphilitic patient at regular time intervals during eight months, to see whether he would acquire immunity against syphilis. No immunizing effect occurred.

For more definitive conclusions, however, Neisser thought that a research project on a much larger scale and stretching over a longer period of time would be absolutely necessary. There was also the problem that the experimental animals in Breslau hardly cooperated; many died from all kinds of affections even before the experimenter could observe the effects of his experiments. At the beginning of the year 1905 Neisser therefore organized a scientific expedition to the Dutch East Indies, paid from his own pocket, to continue his experiments on a much larger scale. He hoped that in the Tropics the mortality among captive primates could be reduced.

Neisser's hope would be disappointed. In less than one year, by the end of 1905, more than 900 primates (macaques, gibbons, orang-utans, etc.) had already been 'consumed' on his experiment station in Batavia. His attempts to achieve passive immunization did not exhibit any convincing results yet. But Neisser was still obsessed by the idea of a serum therapy for syphilis. He attributed the lack of success to the fact that the immune sera so far obtained were not yet sufficiently strong. In December 1905, on leave from his tropical expedition, Neisser declared in Breslau that it should be possible to enhance antibody production in the sera of infected animals, and thereby the protective power against syphilis, by "constantly repeated administration of the syphilitic virus".[48] After all, this had also been the key to Behring's success.

Neisser's involvement in the development of the WaR can be explained from this

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46 Ibid., p. 141.

47 E. Metchnikoff and E. Roux, 'Etudes expérimentales sur la Syphilis, 1. mémoire', *Annales de l'Institut Pasteur* 17 (1903): 809-21.

48 Report on a lecture by Neisser before a meeting of the Medical Section of the Schlesische Gesellschaft für vaterländische Kultur in Breslau, *Berliner klinische Wochenschrift* 43 (1906), p. 373.

background. It was not his first motive to get a diagnostic test for the sake of a more simple detection of syphilis, as Fleck would have it. Neisser's primary interest was to have a clearer understanding of what he perceived as the crucial *bottleneck* in his quest for a serum therapy: the *strength* of the immune serum. He hoped that a serological test might be instrumental in overcoming this bottleneck through a quantitative assessment of the strength of the antisera, and thereby lead the way to an effective serum therapy. During his leave in Germany, between the first and second Java expeditions, Neisser therefore cooperated with the Berlin serologists Wassermann and Bruck to develop what would become known as the Wassermann reaction. Syphilitic material from his patients in the dermatological clinic, but also antisera from the primates that were still kept in Breslau, formed his contribution to this cooperative venture. When he departed in November 1906 for a second expedition to Java (this time financed by the German Empire), he was accompanied by Bruck (who had changed sides after Neisser's rupture with Wassermann[49]). Bruck would apply the new serological test in the Dutch East Indies in the service of Neisser's quest for a serum therapy.

As it turned out, the WaR did not point the way to an effective serum therapy. It was indeed possible to stimulate antibody production, but Bruck found that the titre of the antiserum never exceeded a certain maximum value, however often the administration of syphilitic 'virus' were repeated. The bottleneck could not be overcome.[50] Many detailed questions about syphilis, such as the contagiousness of various syphilitic lesions, were elucidated by Neisser's research on primates. But the main purpose was not achieved. In November 1907, at the end of the second Java expedition, Neisser himself declared the bankruptcy of the whole enterprise: "We hoped for immunization methods. But what if there is no immunity at all? Of that I'm wholly convinced now."[51]

I hope the above reconstruction has made it sufficiently plausible that, from the outset, Neisser's interest in a serological reaction for syphilis was primarily motivated by therapeutic rather than diagnostic considerations. First and foremost, the test was supposed to free him from the deadlock into which his quest for an antisyphilitic serum therapy had led him. The tenacity with which he pursued his project over several years until the bitter end bears witness to Neisser's perseverance and the vigour of his therapeutic motive. All this does not of course imply that Neisser would develop no interest in the diagnostic possibilities of the

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49 The estrangement between Wassermann and Neisser probably dates from August 1906. One reason for the rupture between them, as transpires from correspondence published by Wassermann, was that the latter wanted to test the practical uselfulness of the serological reaction in several German clinics, whereas Neisser wanted to see this work executed exclusively in his own clinic and on his Java experiment station (A. von Wassermann, 'Zur Geschichte der Serodiagnostik der Syphilis', *Berliner klinische Wochenschrift* 58 [1921], p. 467). By deserting to Breslau, Bruck became Neisser's foremost serologist.

50 C. Bruck and M. Stern, 'Die Wassermann-A. Neisser-Brucksche Reaktion bei Syphilis', *Deutsche medizinische Wochenschrift* 34 (1908): 401-04, on p. 403.

51 Cited in Schmitz, op. cit. (note 13), p. 31.

new serological reaction once it had been discovered.

6. From antigen to antibody determination

According to Fleck, antigen rather than antibody determination constituted the primary aim of Wassermann, Neisser, and Bruck in their first two publications. A close reading of those papers, however, will not substantiate this claim. What is rather striking is that the authors stress the *symmetry* of the reaction; they declare repeatedly that the reaction can be used in principle both for antigen and for antibody detection. One can hardly blame the authors for wanting to make the most of the very promising results obtained with antigen determination, but it is also notable that they were not the least discouraged by the more unfavourable outcomes of antibody determination. From their experience with the field of biological diagnostics they knew that it was not unusual to get disappointing results initially. Such bad results could often be expected to improve in due course.[52] The unfavourable results obtained with antibody determination did not therefore prevent the investigators from proclaiming as their conviction that they had really found a specific antigen-antibody reaction for syphilis. This is of course the cardinal point that was ignored by Fleck: to uphold the specificity of their reaction, the investigators had to uphold, for theoretical reasons, its symmetry too. If the reaction would not work both ways, its specificity would become in doubt (as indeed it became later on when antigen determination was rejected as spurious!). Fleck's tendency to consider the development of the WaR exclusively from the practical and technical aspect has probably been responsible for this oversight.

We do however find a pronounced preference for antigen determination, at the expense of antibody determination, in the early serological work coming from the Breslau dermatological clinic after the rupture between Neisser and Wassermann. As it was no longer a matter of establishing the existence of a specific antigen-antibody reaction in syphilis, but of making it practically useful, there were good clinical reasons for this particular emphasis.

From a clinical point of view, a positive outcome of the serological test for antibody determination would not provide much relevant information. It would mean only, or so it was thought at the time, that the patient had once in his lifetime been infected with syphilis spirochaetes, without warranting the conclusion that these were still active in his body. A positive outcome for antigen determination, by contrast, was held to indicate the active presence of spirochaetes in the patient's body. Such information would have far greater clinical utility. In December 1906 Neisser expressed his personal expectation that the new serological reaction would be "diagnostically useful and capable of filling a very important lacuna in our diagnostic abilities: to wit, effecting a separation between latent and cured

52 A. Wassermann, A. Neisser, C. Bruck and A. Schucht, op. cit. (note 38), on p. 476-77.

cases".[53] It was, in Neisser's view, only antigen determination that could fill this gap. We may add that the diagnostic lacuna mentioned by him was especially troublesome for the adherents of the chronic-intermittent method of antisyphilitic mercury treatment. Antigen determination thus held the promise to free the practising physician who followed this treatment scheme from the dilemma of either pushing too far or not far enough. So here we see that Neisser's interest in antigen determination was also informed by the particular therapeutic doctrine he held. An additional, more 'material' reason for this interest was to be found in the availability in Breslau of many primates, which, after intense treatment with human syphilitic material, could supply strong antisera needed for antigen determination. In this respect Breslau had a competitive edge with regard to the other dermatological centres in Germany.

Once again, however, Breslau had moved into a blind alley. Antigen determination did not survive the curious discovery, reported in 1907 by several investigators, that extracts from non-syphilitic organs worked just as well as 'antigen' as did syphilitic liver extracts (73-74/97). This finding also destroyed the theoretical basis on which the WaR had originally been designed. The precise nature of the reaction now became something of a scientific mystery. Henceforth various theoretical proposals would be put forward to solve this riddle. I will not pursue this theoretical issue here. What is astonishing is that the WaR as a practical serological test survived the destruction of its theoretical foundation. This survival can be attributed to the upgrading and revaluation of antibody determination which was already underway when antigen determination turned out to be spurious.

It was largely due to the work of Julius Citron, a serologist and erstwhile collaborator of Wassermann, that antibody determination became useful and clinically relevant. His contribution was not limited to the introduction of increased serum dosage, from 0.1 cc to 0.2 cc of patient serum, as Fleck suggests (72/96). The turning of the WaR into a practically useful test was not just a matter of serologists perfecting the test instrument, or, in Fleck's image, "tuning their sets". Citron's work demonstrates that much more was involved.

As a serologist Citron was associated to the Second Medical Clinic of the Charité hospital in Berlin - it is important to note that this was *not* a specialized clinic for skin and venereal diseases. The patient population of this clinic had a composition that differed from that of the population of the Breslau clinic. It offered special challenges and opportunities to Citron.

Citron ran up against a quite remarkable phenomenon within a group of patients that all exhibited strong clinical indications of syphilis. Those patients who affirmed a syphilis infection in their anamnesis showed much less often positive antibody reactions than those who denied any syphilitic infection. The key to the solution of this riddle was to assume that *both* groups of patients had actually had an infection, but that only the first group, those who

53 A. Neisser, C. Bruck and A. Schucht, 'Diagnostische Gewebs- und Blutuntersuchungen', *Deutsche medizinische Wochenschrift* 32 (1906): 1937-42, on p. 1938.

recognized this fact, had been subjected to mercury treatment. In this way Citron discovered the important fact that intense antisyphilitic therapy has an influence on the outcome of antibody determination. Incidentally, this influence might also explain the disappointingly low figures for antibody determination reported by Wassermann, Neisser, and Bruck in their second publication, for as Citron pointed out in July 1907: "the material [= the patients - HvdB] of the Breslau dermatological clinic, which underlies the first statistics, must for the most part have been subjected to a specific treatment".[54] The early figure of 19 percent positive outcomes in a total of 257 confirmed syphilitic cases happened to refer to a nonrepresentatitive sample of patients that had been intensely treated with mercury (remember that Neisser was an adherent of the chronic-intermittent method!). If we take this circumstance into account, the alleged increase in the accuracy of antibody determination from barely 20 percent to about 70 or 90 percent becomes less dramatic than Fleck suggests.

As a serologist Citron had some difficulty to convince his clinical colleagues at the Charité of the pertinence of the outcomes of the WaR. He found an effective way to deal with what has been referred to in the introductory section as the 'dilemma of application': "the outside experts must be able to convince participants - who, as insiders, might expect to 'know best' - that they *do not* know best". Transferred to our context the dilemma could be stated thus: If the outcome of the WaR agreed with the clinicians' own judgement, it would tell them nothing new; if it disagreed with their judgement, they would doubt its validity and reliability. Citron saw an escape from this vicious circle. In cases of disagreement between laboratory and clinic the matter could sometimes - after the patient's death - be brought before the pathologist and be decided on his obduction table. As Eric Cassell observed in 1986: "The autopsy room used to be the central theater where disputes about the diagnosis of difficult diseases were resolved (..)".[55] On the obduction table the tell-tale signs of a syphilitic affection could often be established. Citron used the strategy of mobilizing allies (Latour).

But what exactly did a positive outcome of the WaR using antibody determination mean? It was Citron's most important contribution to the serodiagnosis of syphilis that he gave a new clinical meaning to antibody determination. Initially he had endorsed the common view shared by Neisser and Bruck, that only antigen determination was capable of indicating the presence of active spirochaetes, but later on he changed his opinion. His change of heart can be quite accurately located in time. On 18 July 1907 Citron still asserted:

"The study of antigen determination would be no less important, because from it we can infer with

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[54] J. Citron, 'Ueber Komplementbindungsversuche bei infektiösen und postinfektiösen Erkrankungen (Tabes dorsalis etc.) sowie bei Nährstoffen', *Deutsche medizinische Wochenschrift* 33 (1907): 1165-71, on p. 1169.

[55] E.J. Cassell, 'Ideas in Conflict: The Rise and Fall (and Rise and Fall) of New Views of Disease', *Daedalus* 115 (1986): 19-41, on p. 27.

> certainty the persistence of the disease, whereas the antibodies refuse us an answer as to whether the pathogenic virus is still active (*fortwirkt*) or not".[56]

On 28 October 1907 Citron declared however:

> " [..] in general it may be held to be correct that the presence of antibodies indicates the presence of active syphilis, and, conversely, that the disappearance of antibodies indicates the onset of complete latency or perhaps even the healing of lues. I'm standing here in a certain opposition to Neisser, Bruck, and Schucht, who from the presence of antibodies want to conclude no more than that at one time or another a luetic infection has taken place".[57]

Citron appeared to have forgotten that he himself had propounded the same view only three months earlier! His main reason for abandoning this view now was that he had encountered patients whose date of infection was more than four decades ago but who still showed a high antibody content in their serum. Citron considered it highly implausible that the human body would keep antibodies for so long without use. He therefore thought it more reasonable to assume that in such cases active spirochaetes were still present and continued to stimulate, directly or indirectly, antibody production. For the time being this was only Citron's personal opinion. In due course it would become the view shared by most serologists and clinicians. This shift in the clinical meaning of antibody determination was a crucial episode in the development of the WaR.

7. The Wassermann reaction in a clinical context

In December 1908 Fritz Lesser, serologist at a private dermatological clinic in Berlin, addressed what he called "the cardinal question in the whole serodiagnostics of syphilis", namely "the question whether a positive Wassermann reaction [using antibody determination - HvdB] merely indicates that the person concerned has once had syphilis, or whether it proves extant disease".[58] It was not possible to answer this question by immunological experiment, Lesser asserted. Only a combination of serology and clinical experience, backed by pathological anatomy, could provide an answer. Lesser opted for the second alternative that "in case of a positive serum reaction the syphilitic virus is still active"[59], largely because

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56 Citron, op. cit. (note 54), p. 1171.

57 J. Citron, 'Die Serodiagnostik der Syphilis', *Berliner klinische Wochenschrift* 44 (1907): 1370-73, on p. 1371.

58 F. Lesser, 'Weitere Ergebnisse der Serodiagnostik der Syphilis', *Deutsche medizinische Wochenschrift* 35 (1909): 379-83, on p. 382.

59 Ibid., p. 383.

of the fact that prolonged antisyphilitic therapy with mercury and iodine is able to influence the outcome of the WaR, that is, to turn it from strongly positive to weakly positive and eventually to negative. This change in the outcome of the WaR was often accompanied by the disappearance of the visible symptoms. Of course, the fact that mercurial therapy is able to influence the outcome of the WaR only counts as an argument in favour of the validity of this serological test if you are already convinced that mercury is more or less effective as an antisyphilitic remedy. For the antimercurialists at the fringe of the medical profession Lesser's argument cut no ice. By the same token it was destined to receive a sympathetic hearing from those clinicians who prescribed mercurial treatments, either according to the chronic-intermittent or the symptomatic-expectative method. At last, the WaR appeared to supply the long-awaited proof that mercury really worked! The test could also be used to guide therapy. Lesser even went so far as to consider the WaR as an "individually adapted yard-stick for therapeutic intervention". The new aim of antisyphilitic treatment would be to let the symptoms disappear *and* to achieve a negative WaR, in order thereby to avert the onset of late-syphilitic disorders in later life, for as Lesser remarked: "Over the head of every syphilitic person with a positive reaction hangs as a Sword of Damocles tabes or paralysis".[60]

At about the same time, in January 1909, the well-known dermatologist Alfred Blaschko expressed his confidence in the practical usefulness of the WaR. This support was the more significant because it came from a clinician who had previously taken a rather skeptical attitude toward the WaR. What had changed his mind was the impressive evidence, accumulated since the second half of 1907, of 1400 serological inquiries (initially carried out by Julius Citron) on almost 1000 cases of his own private clientele. Beside the possibility to influence the outcome of the WaR by prolonged antisyphilitic treatment, there was also found a general parallelism between the reaction and the manifestations of the disease. These facts made it highly probable also for Blaschko to assume that a "positive reaction indicates the existence of foci of syphilitic disease (*syphilitischer Krankheitsherde*) in the organism".[61]

As an opponent of the chronic-intermittent method, which he designated as a "method that strikes completely blindly" (*dieser ganz blind dreinschlagende Methode*), Blaschko was however more cautious than Lesser in drawing conclusions with regard to antisyphilitic therapy. He agreed that a negative reaction, in addition to the disappearance of symptoms, constituted a new aim for therapy: treatment should in general be continued until the reaction became negative. However, he immediately qualified this rule. It would of course be absurd, he added, to pour endless quantities of mercury into patients just to achieve the aim of a negative reaction. The physician should always keep an eye to the general condition of the

60 Ibid.

61 A. Blaschko, 'Ueber die klinische Verwertung der Wassermannschen Reaktion', *Deutsche medizinische Wochenschrift* 35 (1909): 383-90, on p. 389.

patient as a living person, his tolerance for mercury, possible undesirable side-effects etcetera. At times treatment should be temporarily discontinued if only to allow the patient some breathing space. According to Blaschko, the physician's art (*ärztliche Kunst*) was precisely to know how to tack and to shift; it would be bad to replace this art with a rigid, dead schematism.[62]

Thanks to the WaR, however, the old opposition between the adherents of the chronic-intermittent method and of the symptomatic-expectative method was already losing much of its pungency. The former method could strike less 'blindly' now. Its rigid schematism could be relaxed. Formerly, the orthodox proponents of this method would prescribe an antisyphilitic treatment consisting of, say, 8 cures, each cure consisting of 30 Hg inunctions (or 30 soluble Hg injections; or 15 insoluble Hg injections). On the basis of extensive statistical material measuring the effects of the number of cures on obtaining a persistently negative WaR, Fritz Lesser showed in January 1910 that a total number of 4 cures would be almost as useful as, and of course far less inconvenient than, the rigidly prescribed number of 8 cures.[63] Moreover, each separate cure could be more flexibly scheduled. Instead of fixing a certain quantity of mercury to be administered to the patient, it would now be possible to use the WaR as a criterion (*Massstab*) for the duration of the cure. According to Lesser, a positive reaction could be considered as an indicator for all symptoms, both of the visible eruptions on the skin and of the invisible affections of internal organs (revealed by pathological anatomy). Indeed, a positive WaR could itself be considered as "the most constant symptom of syphilis". Antisyphilitic treatment was to be oriented to the practical aim of achieving a (persistently) negative WaR. This was only the penultimate aim, but it served as a practical surrogate for the ultimate aim of warding off the dangers of tabes, paresis, aortic aneurysm and other cardiovascular disorders.

Here we may pause to note that Lesser, as Citron before him, very ably dealt with the so-called 'dilemma of application', in particular with regard to those clinicians who followed the chronic-intermittent method. As Mulkay, Pinch, and Ashmore write: "(..) the outsiders [read: serologists] must convince practitioners [read: clinicians] of the inadequacy of existing practices without generating undue hostility and without thereby jeopardizing practitioners' collaboration in recreating in the practical realm the conditions required for the successful implementation of the outsiders' recommendations".[64] This is what Lesser did. He first enlisted the clinicians' support for the WaR by demonstrating with its aid the effectiveness of mercurial therapy. Then, in a second move, he undertook to convince them of the "inadequacy of existing practices" by using the WaR to demonstrate the supererogatory and

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62 Ibid.

63 F. Lesser, 'Die Behandlung der Syphilis im Lichte der neueren Syphilisforschung', *Deutsche medizinische Wochenschrift* 36 (1910): 116-21, on p. 119.

64 Mulkay et al., op. cit. (note 8), p. 233.

overly rigid character of some existing treatment schemes.

While the use of the WaR as a criterion in antisyphilitic treatment led to a relaxation of the chronic-intermittent method, it also changed the symptomatic-expectative method. Although Blaschko did not want to consider a positive reaction itself as a symptom, by December 1908 he was already willing to accept it as a ground for initiating an antisyphilitic treatment, *even in the absence of visible symptoms*.[65] For the adherents of the symptomatic-expectative method the WaR therefore led to an enlargement of the indications for therapy.

In both ways, the serodiagnosis of syphilis brought about a certain convergence of the two treatment schemes and thus a rapprochement between the rival therapeutic schools within the profession. The official medical world could close its ranks against the antimercurialists and other quacks. Practising physicians gained in credibility toward their patients, for it would now be easier to argue the necessity of a treatment in the absence of visible symptoms.

The new near-consensus in the profession was duly expressed in the 1911 edition of the handbook of Kolle and Hetsch on bacteriology and infectious diseases:

> "The majority of clinicians and serologists nowadays takes the view that a positive reaction is a sign of active foci of spirochaetes (*aktiven Spirochaetenherden*) with syphilis".[66]

This passage speaks of clinicians and serologists, and rightly so, for as I have tried to show in this chapter, the development of the WaR into a practically useful test for detecting syphilis has actually been a joint achievement of the laboratory and the clinic.

8. A full-blown and well-established scientific fact?

By 1910 "the fact that the so-called Wassermann reaction is related to syphilis" (XXVII/2) was generally considered as well-established. In the preceding sections I have given a detailed account of the efforts of serologists to persuade clinicians of the value of this serodiagnostic test by capturing and translating their interests and forging 'epistemic alignments' between serological and therapeutic practices. My historical analysis was brought to a provisional conclusion at the point where those efforts were crowned with some success. This was however by no means the end of the story of the 'genesis and development' of the WaR. It is not at all easy to locate an exact end-point in time at which this process can be said to have reached completion. Fleck notes that the WaR had set an enormous avalanche in motion. At the time of his writing (1935) more than ten thousand scientific papers had

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65 Blaschko, op. cit. (note 61), p. 389.

66 W. Kolle and H. Hetsch, *Die Experimentelle Bakteriologie ind die Infektionskrankheiten*, Volume II, Third edition, Berlin-Vienna (Urban & Schwarzenberg), 1911, p. 657.

already been published on this particular subject: "There certainly cannot be many similar specialized problems which have so many papers devoted to them" (81/108). As yet this stream showed no signs of slowing down.

Fleck sees no difficulty in this vast accumulation of scientific papers. For him it simply indicates the sheer size of the collective effort devoted to the serodiagnosis of syphilis. It is taken as support for his theory which states that a scientific fact can be established only by collective work. In this case, however, I'm inclined to turn Fleck's vision on its head. If so much collective energy apparently had to be spent on this particular subject matter, then there may be something peculiar about the scientific fact that Fleck selected as his example for epistemological investigation. Perhaps this fact was not such a well-established fact after all. In other words, the very size of the collective effort may be reason for suspicion.

As I see it, the basic source of the difficulty is to be found in the early 'decoupling' of theoretical and practical questions which occurred after the theoretical conception from which Wassermann and Bruck had proceeded turned out to be untenable. In the absence of a convincing and generally accepted theoretical interpretation of the nature of the WaR, Fleck could define the 'factuality' of his chosen example only in terms of practical success:
"The theory of the reaction as well as the historical and psychological circumstances surrounding its conception are of less practical importance. *If the relation of the Wassermann reaction to syphilis is a fact, it became a fact only because of its extreme utility owing to the high probability of success in concrete cases*" (72/95; italics in the original).

It is doubtful whether such a 'dumb fact' of practical success, devoid of a satisfactory 'theory of the reaction', may count as a full-blown example of a 'scientific fact'. Let us draw on a comparison with a different domain. Pickering describes the production of facts in the field of elementary particle physics as resulting from a *three-way interactive stabilization* between material procedures, interpretive accounts (the theory of the apparatus) and phenomenal accounts.[67] What was conspicuously slow in coming in the case of the WaR, at least during the first four or five decades of the 20th century, was a convincing 'interpretive account' of the reaction (Fleck's "theory of the reaction") which would have made sense in the light of prevalent theories of immunology. For this reason the potentially three-way interactive stabilization process necessarily degenerated into a two-way process between material procedures and phenomenal accounts (that is, in this case, positive or negative test results indicating the presence or absence of syphilis). This two-way interactive stabilization, as we have seen in the preceding sections, was brought about by the forging of 'epistemic alignments' between serological and clinical practices. Perhaps the interaction between serologists and clinicians was of such extraordinary importance in the formation of the WaR as a practically useful test precisely because no generally accepted interpretive account was to fill the gap left by the original interpretation of the reaction.

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[67] A. Pickering, *The Mangle of Practice: Time, Agency & Science*, Chicago (The University of Chicago Press), 1995, p. 86.

Not that proposed interpretations were in short supply. Part of the avalanche of scientific papers mentioned by Fleck was intended to solve the puzzle of the WaR.[68] A key piece for the solution of this puzzle, however, would become available only in 1942 when Pangborn isolated the substance against which the serum of syphilitics reacted and identified it as a phospholipid, which she dubbed *cardiolipine*.[69] Even then the puzzle was not fully solved.

A large part of the avalanche of papers consisted of proposals for modifications and 'simplifications' of the WaR and of the closely related flocculation tests (which did not employ complement fixation but were based on the same 'Wassermann antibodies' or 'reagins' as figured in the WaR). This endless stream of modifications raises doubts as to whether the WaR ever achieved the degree of standardization as is sometimes suggested (and as might be thought required for the relationship with syphilis to qualify as a full-blown fact!). For Rouse, Fleck's account is precisely exemplary as an analysis of how standardization can be attained: "Virtually every kind of scientific achievement can be standardized and can thereby circulate outside its original more specific context [..]. Sometimes standardized facts are just the correlates of standardized procedures. The correlation between syphilis and a complicated transformation of the blood is a fact that was at first manifested in a very limited context and gradually, through repetition and practical refinement of the procedure, was extended into new domains".[70] Fleck emphasizes that the thought collective "standardized the technical process with genuinely social methods", namely "through conferences, the press, ordinances, and legislative measures" (78/104; the first method refers primarily to the League of Nations conferences on the serodiagnosis of syphilis held in 1923, 1928, and 1930), but this standardization effort must have been an endless uphill battle in view of the numerous local variations and modifications that were generated time and again. Thus at the League of Nations conference of 1928 held in Copenhagen 7 different modifications of the WaR - in addition to 9 different flocculation tests - were evaluated, while two years later, at the 1930 Montevideo Conference, again 7 different modifications were assessed, with only one modification (the socalled Harrison-Wyler method) being represented on both occasions.[71] One should therefore not overestimate the

68 In Chapter VI I will give a brief exposition of some current interpretations of the nature of the WaR, inasmuch as these are relevant for an analysis of the struggle over the 'intellectual property' of this serodiagnostic test. See also Fleck (80-81/106-108).

69 B. Zalc, 'Some Comments on Fleck's Interpretation of the Bordet-Wassermann Reaction in View of Present Biochemical Knowledge', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, pp. 391-405.

70 Rouse, op. cit. (note 2), p. 116. Referring to Fleck (53/72), Rouse also highlights the reverse movement to standardization, to wit that "each laboratory develops its own variations on the standardized scheme" (p. 110). My point is that it is highly questionable in the case of the WaR whether such a "standardized scheme" could be said to exist in the first place.

71 C.D. Westermann, *Over Niet-Specifieke Positieve Reacties op Syphilis*, Amsterdam (Thesis), 1945.

degree of standardization achieved by this "genuinely social method".

The foregoing discussion has adduced some considerations which may call the appropriateness of Fleck's example into question. It is doubtful whether the correlation between the WaR and syphilis qualifies as a full-blown example of a scientific fact in view of the absence of a commonly accepted interpretation of the reaction and the lack of proper standardization. The force of these considerations depends on the 'grammatical' criteria employed in speaking about scientific facts, but as such they do not affect the presumed practical usefulness of the WaR. So what about the latter?

Like most of his contemporaries, Fleck was convinced of the practical reliability of the WaR. As the English pathologist and bacteriologist William Bulloch stated in 1938, the WaR was regarded as "a test of deadly accuracy".[72] In technical discussions on the accuracy of diagnostic instruments it is customary to distinguish between the *sensitivity* of a test (defined as the conditional probability that someone having the disease will show up with a positive outcome) and its *specificity* (defined as the conditional probability that someone not having the disease will show up with a negative outcome). Before the Second World War, the WaR was generally taken to be accurate and reliable in the sense that it was seen as combining an adequate sensitivity with a very high degree of specificity.[73] A positive result on the WaR was normally considered sufficient reason for initiating a vigorous antisyphilitic treatment.

Was such confidence in the high accuracy of the WaR warranted? Of course, from a constructivist point of view it is not allowed to ask this question, but I can hardly resist the temptation of committing the sin of 'Whig history' in this particular case, if only because this violation of principle offers a dramatic relativization of the notion of 'practical success'.

The period after the Second World War gained new insights into the accuracy of the WaR, because so-called direct 'treponemal tests' like the treponema immobilisation test (TPI) and the FTA-fluorescein treponemal antibody test came to be available.[74] In contrast, the older WaR and flocculation tests were now designated as 'non-treponemal tests' because they used as 'antigen' a substance that was not directly related to the treponema (or spirochaete). Another circumstance was that extensive epidemiological data obtained by using the WaR had become available as a consequence of decisions taken in the 1930s in several US states to introduce compulsory pre-marital syphilis testing and of mass screening of US soldiers during the Second World War. This large-scale testing effort had yielded incredibly high levels of

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72 W. Bulloch, *The History of Bacteriology*, New York (Dover), 1979 [1938], p. 283.

73 Fleck also implicitly assumes such a very high degree of specificity, but does not provide numerical data. When he discusses the increase in the success rate of the WaR (from 20 percent to 70-90 percent in cases of confirmed syphilis), he gives percentages that relate to the sensitivity of the test.

74 My account is based on the excellent analysis in I. Löwy, 'Testing for a Sexually Transmissible Disease, 1907-1970: the History of the Wassermann Reaction', in V. Berridge and P. Strong (eds.), *AIDS and Contemporary History*, Cambridge (Cambridge University Press), 1993, pp. 74-92.

positive results among some sections of the population. The newly available 'treponemal tests' confirmed the scepticism raised by these epidemiological data among some American physicians. The WaR turned out to be much less accurate than had been supposed earlier. According to Walsh McDermott, the specificity of the test was such that when used for mass screening, it could easily result, in some populations, in more than 50 percent of 'false positives'.[75]

A brief technical elaboration may be in order here. The predictive value of a test (that is, the probability of actually having the disease given a positive test result), and therewith the chance of 'false positives', depends not just on its sensitivity and specificity but also on the prevalence of the disease among the population. Using Reverend Thomas Bayes' famous theorem, we can illustrate this with a simple calculation.[76] Suppose the specificity of the test is 98% and its sensitivity 80% (not such unfavourable assumptions, one might think), then if the prevalence is put at 10% the predictive value of the test will be 81.6% (with a corresponding chance of false positives amounting to 18.4%). If under these same assumptions the prevalence is put at 5%, however, the predictive value will be reduced to 68% (with a corresponding increase in the chance of false positives to 32%!).

There is no question, in the view of modern critics, that the US mass campaigns of the 1930s against syphilis must have caused a lot of unnecessary harm. As Allan Brandt notes: "Many individuals during the 1930s suffered the consequences of the toxic syphilitic treatments although they were never infected; in some cases, these individuals were barred from marriage because of an incorrect Wassermann reading. They suffered the stigma associated with a disease they never had".[77]

Of course, this whole exercise in retrospective wisdom is anathema to the genuine constructivist. For a constructivist, 'practical success' (like truth) cannot be more than what is taken as such in a certain social group. Let's therefore forget about the entire Whig history above! But, perhaps, we are still allowed to ask a different question: *Why* was the WaR commonly *taken* as practically successful among serologists and physicians (and among the general public as a whole) before the Second World War? What were the mechanisms for 'maintaining the faith'?

Ilana Löwy points out that, paradoxically, it was precisely the delicacy and technical

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[75] McDermott, op. cit. (note 20), p. 144: "Of all those people yielding positive reactions, only about one-half were actually syphilitic".

[76] The following formula is based on Bayes' Theorem:

Predictive value

$$= \frac{\text{prevalence X sensitivity}}{\text{prevalence X sensitivity} + (1 - \text{prevalence})(1 - \text{sensitivity})}$$

[77] A.M. Brandt, *No Magic Bullet*, New York and Oxford (Oxford University Press), 1987, pp. 152-54.

complexity of the WaR that could be used to maintain confidence in the accuracy of the test and to neutralize possible criticism: "The belief in the high specificity of the Wassermann reaction was always based on the assumption that the test was properly executed. On the other hand, the fact that the original Wassermann test was technically complicated made possible maintaining the faith in the specificity of the method by attributing all the inexplicable results to laboratory errors".[78] A special version of Harry Collins' '*experimenter's regress*' can be seen to be operative here, in the sense that competence in performing the technical procedure was judged in part on the basis of the desired outcome. This is clearly expressed in the following statement by the Rockefeller Institute bacteriologist Hideyo Noguchi: "[I]t should be suspected that when one obtains a high percentage of positive reactions in non-syphilitic cases one is not doing the test properly".[79]

It is significant that Fleck too uses the delicacy and technical complexity of the WaR as grounds for granting the test the benefit of the doubt. He mentions "the poor results obtained even by excellent research workers at the [..] Wassermann conferences held under the auspices of the League of Nations" (97/127), but sees no reason to doubt the accuracy of the WaR. Instead, Fleck alludes to the necessity of "quasi-*orchestral practice*" (cf. Pickering's paraphrase quoted in the introductory section) in performing the test and adduces the disturbance caused by a change in personnel as sufficient explanation for the poor results.

Even more problematic is Fleck's evident partiality against Wassermann's early critics. He stresses that each serologist must acquire *experience* in the practical application of serological techniques in general and of the WaR in particular before he will be able to obtain reliable results and then remarks: "A state of this kind [to wit, a state of experience] is what the first critics of the Wassermann reaction lacked" (96/126). Elsewhere, Fleck refers to the so-called "ultramethodics with a personal undertone" that Wassermann's assistant Meier demanded from the first critics of the WaR and which in his (Fleck's) view nicely illustrates the "personal factor in the emerging truth" (177 note 1/113 note 1). Fleck is clearly taking sides here with Wassermann and his likes against the critics. It is interesting to note, however, that one of those early critics, Eduard Weil, was fully aware of the potential of Meier's requirement for immunizing the WaR against all possible criticism: "An agreement on the question whether the WaR may on occasion also be positive in other diseases or whether [..] there exists an absolute 'specificity' can never be attained, because the adherents of the first group will always be accused of not mastering the 'ultramethodics with a personal undertone' demanded by Meier".[80]

Fleck's *parti-pris* in this case can hardly be reconciled with the principles of symmetry

[78] Löwy, op. cit. (note 74), p. 78.

[79] Cited in Löwy, op. cit. (note 74), p. 79.

[80] E. Weil, 'Das Problem der Serologie der Lues in der Darstellung Wassermann's', *Berliner klinische Wochenschrift* 33 (1921): 966-970, on p. 968.

and impartiality that are constitutive of modern constructivism, or with his own wish to develop a "non-egocentric" theory of knowledge. The mechanisms for 'maintaining the faith' that were effective on his contemporaries also operated on him. As Löwy observes: "[..] even such an unorthodox critic of science as Fleck did not suspect that the Wassermann test [..] was far from being well established as a 'medical fact' as it was believed in the 1920s and 30s".[81]

9. Afterthoughts and conclusions

What does it mean to view science as practice? In the foregoing sections I have argued that Fleck's account of the development of the WaR, so much admired by the adherents of a science-as-practice approach, suffers from serious shortcomings and limitations. His vision of the serologists' collective as one big cooperative team tinkering its way to success, as it were, by 'tuning' the material procedure is a rather autistic view that ignores the importance of the 'clinical connection'. In my judgement, Fleck overestimates the increase in the success rate of the WaR (or more exactly in its sensitivity) which has been achieved by such tinkering and tuning (if only because the initial figure of about 20 percent may have been unduly low owing to the special characteristics of the 'patient material' used in the early experiments!). His analysis also passes over what the serologist Fritz Lesser called "the cardinal question in the whole serodiagnostics of syphilis", to wit, the question of whether antibody determination could be used to detect extant disease or merely a one-time infection with syphilis. This crucial question, as I have tried to show in my alternative historical acount, was to be resolved not by serologists autistically "tuning their sets" but through close interaction between serologists and clinicians.

I believe that in some respects my analysis of the development of the WaR comes closer to the requirements of a science-as-practice approach than Fleck's account. Joseph Rouse in particular has emphasized the importance of taking account of how practices are connected to one another for understanding scientific knowledge:

> "Practices are always interconnected, never existing in isolation from one another, in ways that fundamentally affect the ongoing development of any particular practice. A typical practice needs other practices to enforce its norms, provide its necessary equipment and resources, educate and train its practitioners, confer significance on it or undercut its previous significance [..]. In order to understand how scientific knowledge is situated within practices, we need to take account of how practices are connected to one another, for knowledge will be established only through these interconnections".[82]

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[81] Löwy, op. cit. (note 74), p. 75.

[82] Rouse, op. cit. (note 1), pp. 156-57.

Thus in my analysis I have paid special attention to the interconnections between the laboratory practice of serologists, organized around the 'experimental system' of immune hemolysis, and the diagnostic and especially the therapeutic practices of dermatologists and other clinicians, organized around the mercurial treatment of syphilitic patients.[83] It was only by attending to the special problems and dilemmas with which clinical specialists were confronted in dealing with their patients that serologists could make the WaR into a practically useful test. Only in this way could the experimental manipulations with blood samples that are designated by this name become informative about the disease entity syphilis.

According to Rouse's 'posthumanist' conception, practices are more than sequences of human activities: "practices are not just patterns of action, but the meaningful configurations of the world within which actions can take place intelligibly, and thus practices incorporate the objects that they are enacted with and on and the settings in which they are enacted".[84] I want to leave it undecided here whether this indeed offers an attractive conception of practices, but I agree that any concrete analysis of a scientific practice should attend to the material and institutional setting in which and to the objects on which it is conducted. Let me just cite here some illustrations from my historical account of the development of the WaR which graphically show how a scientific practice may be crucially dependent on the special properties of the 'objects' or 'raw materials' with which it has to deal. In a biomedical context, what figures prominently among such objects is 'patient material' (*Krankenmaterial*), as it is often designated somewhat disrespectfully.[85] Thus, as the serologist Julius Citron pointed out, the disappointing initial results obtained with antibody determination could be partly explained by the peculiar nature of the 'material' from the Breslau dermatological clinic, i.e. by the fact that Neisser's syphilitic patients had been heavily treated with mercury. If only the choice of 'patient material' had been a little bit more fortunate, Citron

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83 It must be noted that Pickering distances his approach from the study of *practices* (in the plural form). He focuses on *practice* (emphatically in the singular form), which he understands as the work of cultural extension and transformation in time. 'Practices', that is, on his definition, "specific, repeatable sequences of activities on which scientists rely in their daily work", are considered as components of an existing scientific culture and as not concerned with its extension in time. See Pickering, op. cit. (note 67), p. 4. The problem is that Rouse does not make this distinction. His notion of 'practices' includes more than just patterns of activities and is also characterized by a dimension of temporality. This is not the place, however, to sort out the differences between these two authors.

84 Rouse, op. cit. (note 1), p. 135.

85 Perhaps this usage was especially current among German medical researchers in the early 20th century. In his book on technology, Heidegger made a passing comment on this particular usage (I leave the passage untranslated): "[..] [G]ehört [..] nicht auch der Mensch, ursprünglicher noch als die Natur, in den Bestand? Der umlaufende Rede vom Menschenmaterial, vom Krankenmaterial einer Klinik, spricht dafür". See M. Heidegger, *Die Technik und die Kehre*, Pfullingen (Günther Neske), 1991, p. 17.

implied, the results would have looked more promising from the start. The 'patient material' from the Berlin clinic to which he himself was affiliated had quite different characteristics and thus offered different opportunities for serological work. Other kinds of raw material also played crucial roles in the development of the WaR. Initially, the spirochaete-swarming livers of stillborn luetic babies - "a much sought-after article", according to H. Beitkze[86] - constituted the source of 'antigen' for Wassermann, Neisser, and Bruck. They had ample access to this resource thanks to a large network of contacts (in their first research paper they expressed their acknowledgements to no less than *thirty-six* persons who had provided them with material). It is significant that other investigators who had a much less privileged access to syphilitic livers subsequently found out that extracts from normal (non-syphilitic) livers would be equally usable. Likewise, Neisser's artificially infected primates in Breslau lost their value as suppliers of strong luetic sera when the serodiagnosis switched from antigen to antibody determination. In science as in economic life the value of given resources may be highly unstable.

The foregoing discussion also illustrates that access to raw materials may be regulated by *social factors*. (Citron's appeal to the pathologists of the autopsy room - after the death of the patient - to settle disputes with clinicians over the proper diagnosis offers another example. This option was open to him because of the circumstance that around 1900 poor patients were admitted to German hospitals on the condition that in the event of death their corpses would be available for post-mortem examination![87]) Social factors are supposed to be included in Rouse's conception of practices, but it is not entirely clear to me exactly how and where they fit in. He stresses that the social dimension of a practice is "radically open" and polemicizes against SSK constructivists on the grounds that they do not properly recognize this circumstance:

> "Social constructivist interpretations of practices fail to take adequate account of this openness of the social dimensions of practices. When they insist that social relations or interests are explanatory, they foreclose the possibility that those relations or interests, or even their characterization as social, may be what is at issue in the continuation of the practice".[88]

What is the force of this principled objection? Rouse maintains that social factors and interests, far from being the unproblematic explanans for which the SSK constructivists take them, are characterized by plasticity. He cites the interconnectedness of practices as a further argument against the SSK position, because this interconnectedness appears to imply that the boundaries of social communities, and thus the identities of their members, are always

86 See note 34.

87 See Elkeles, op. cit. (note 45).

88 Rouse, op. cit. (note 1), p. 141.

ambiguous and contested.[89] Rouse holds that the difference between members and non-members, and even between agents (subjects) and non-agents, is established within practices. This difference is thus neither objectively nor socially determined: "What matters is interconnected and ongoing practical interaction with the world".[90]

The issues raised by Rouse are too large to be exhaustively discussed here. The problem with his argument, in my view, is that it is too principled and too fundamentalistic. It has rather nihilistic consequences in that it removes all fixed anchoring points for an explanatory undertaking (indeed, he abjures any explanatory intent). I also believe that the interconnectedness of practices does not provide a knock-down argument against social explanations. In fact I'm quite willing to concede a limited though essential explanatory role to 'interests' precisely in explaining the interconnection of practices, or the creation of 'epistemic alignments' between them. One should think here primarily of those 'interests' which derive from the scientists' previous investments in their skills and competences. As SSK constructivist Steven Shapin observes:

> "[Scientists are] quite able to weigh in the balance the courses of action offered to them and the investments they have acquired in their skills and competences. One should say that they have an 'interest' in those skills and work-routines, an 'interest' in encouraging or enlisting in courses of action which promise to give scope and value to their skills and routines. Given a basically calculative model of the actor [..], 'interests' seem quite hard and durable enough to figure in a job of explanatory work".[91]

As interests of this kind are intimately related to the *work* performed by scientists, one might imagine them to deserve a prominent place in any science-as-practice approach.[92] Let us look now at how 'interests' figured in a more concrete way in the development of the WaR.

As I have extensively argued, the dermatologist Neisser was at first primarily motivated by a therapeutic (and not a diagnostic) 'interest' in the development of a serological test for syphilis. He had literally invested extraordinary amounts of work (and money!) in his dream project to search for a serum therapy against syphilis, and the prospective test was supposed to free this project from the deadlock in which it had entered. What about Wassermann's 'interest' in the development of the serodiagnosis of syphilis? If we assume that it was Neisser who proposed to him to work on this subject (as Bruck suggested in 1921), then it is not difficult to see that Wassermann had an 'interest' to enlist in this proposed course of action, because a successful complement fixation test for syphilis would surely demonstrate

89 Ibid., p. 145.

90 Ibid., p. 146.

91 S. Shapin, 'Following Scientists Around', *Social Studies of Science* 18 (1988): 533-50, on p. 546.

92 "Given a community of competent users, there is no way to *talk your way* into possession of the relevant skill" (Shapin, ibid., pp. 545-46).

the importance of the modification of using so-called 'bacterial extracts' that he and Bruck had introduced into Bordet's method (in its usual version the method could not be employed because the causative agent of syphilis had resisted all attempts at cultivation). In other words, this was certainly "a line of action which promise[d] to give scope and value to their routines and skills". As for the clinicians' diagnostic interest in a serological test for syphilis, I have argued that this should be seen in the light of the strong commitments of most of them to some scheme of vigorous mercurial treatment of syphilitic patients. For the clinical specialists, their therapeutic schemes constituted a major 'investment' to be protected and enlarged, and to be defended against the attacks of anti-mercurialist 'quacks'. A test which merely indicated that a patient had once in his lifetime been infected with syphilis spirochaetes, would hardly have been of interest to most clinical specialists. It would be different for a test which enabled them to separate cured from latent cases. For this reason Neisser and his collaborators in Breslau, after the failure of the search for a serum therapy, concentrated their energy on the ill-fated antigen determination and not on antibody determination. Eventually, thanks to the efforts of Julius Citron, Fritz Lesser, and others, the conviction gained ground that the clinical interest in detecting *extant* disease could be served by antibody determination itself. One of the most convincing arguments vis-à-vis clinicians was that prolonged mercurial therapy would influence the outcome of the WaR; the test was embraced because it appeared to provide the long-awaited proof that mercury was effective after all. As the WaR (using antibody determination) proved useful in monitoring the treatment of confirmed cases of syphilis, it was incorporated into existing therapeutic schemes. In the process it brought about a *rapprochement* between rival therapeutic schools among specialists, thus enabling them to close their ranks against the anti-mercurialists at the fringe of official medicine and to gain credibility and authority towards their patients. The creation of 'epistemic alignments' between serological and clinical practices thus included attending to the clinicians' interests. *Contra* Rouse, we can conclude that the interconnectedness of practices constitutes no argument against interest explanations. Having defended the legitimacy of interest explanations, I must immediately add some important qualifications. In the typical SSK approach, interests are seen as sufficiently hard and durable to be used for explanatory purposes (cf. the above-quoted passage from Shapin). It would seem, however, that this relative 'hardness and durability' of interests is not a matter of *a priori* principle but is susceptible to empirical variation.[93] Let us briefly review our historical case in this respect. How 'hard' and 'durable' were the interests that were involved in the development of the serodiagnosis of syphilis? We have seen that Neisser's

93 Pickering criticizes "the canonical SSK literature on the interest model" for its lack of interest in "how interests themselves change in practice". He admits that there are situations where interests remain constant through practice (it is on these situations that SSK studies have focused), but denies that such situations are representative. Thus in his view interests cannot be regarded as "unmoved movers", but are themselves subject to the "mangle of practice" and liable to redefinition. See Pickering, op. cit (note 67), pp. 64-65.

early *therapeutic* interest had to be given up when his search for a serum therapy ended in failure. Subsequently, his *diagnostic* interest in a test capable of separating cured from latent cases led to what later turned out to be the 'wrong' choice for antigen determination. Ironically, this interest would eventually be satisfied by antibody determination. I have also declared that the clinical specialists were strongly committed to their different therapeutic schemes of mercurial treatment, but this commitment (or 'interest') was not unalterable either. In fact, under the influence of the WaR, the adherents of the chronic-intermittent method were willing to implement a relaxation of their rigid schedule, whereas the proponents of the symptomatic-expectative method accepted a broadening of indications. Both schools thereby furthered their *common interest* in closing the ranks against the anti-mercurialists and in enhancing their authority vis-à-vis the general public. It is precisely for the changes in therapeutic schemes induced by the incorporation of the WaR that we can say that the serologists not just captured but also *translated* the interests of the clinicians. This translation was not simply a linguistic matter of 'interest-talk'; it involved a restructuring of the clinicians' field of action possibilities.[94]

The upshot of this long argument is that interests can indeed play a limited explanatory role in accounting for the activities of scientists, but cannot be regarded as "unmoved movers", to use Pickering's expression. Putting them in such a role would be to erect a grand metaphysical scheme. On the other hand, there is no reason for a principled rejection of interest explanations either. Even if interests may not emerge unchanged from scientific practice, it does not follow that they cannot be used to explain the choices investigators made when they embarked upon certain lines of action. The problem calls for a thoroughly pragmatic assessment.

At the end of this chapter I must mention two issues that could not be treated here, but that will be discussed in other chapters. First, it may have surprised the reader that our analysis of the development of the WaR did not involve any extended discussion of the so-called serological thought style, whereas the notion of thought style appears to be a central concept in Fleck's theory. The reason is that this serological thought style, as described in Fleck's first chapter on the WaR (59-64/79-84), hardly plays any role at all in his subsequent account of the genesis and development of this serodiagnostic test. Admittedly, Fleck writes that every serologist performing the WaR must participate in the thought style to perceive the relation between syphilis and blood as a definite form (96/127), but in this context the required participation means no more than that the serologist must have acquired comprehensive experience before he can obtain reliable results. There is no allusion here to the serological thought style consisting of a set of precepts as defined earlier. (Actually, the WaR does not exemplify the serological thougt style because it constitutes an *anomaly*; it flatly contradicts the element of 'serological specificity'!) We will have occasion to discuss

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94 Of course, this field would be even more drastically restructured after 1910, when Ehrlich's Salvarsan started to replace mercury.

Fleck's description of the serological thought style, and the status of the concept of thought style in general, in more detail in Chapter VIII.

The second issue that was only broached but not thoroughly discussed in this chapter is Fleck's rather extreme 'collectivism'. This issue is basically at stake in the analysis of the struggle over the intellectual ownership of the WaR and is therefore reserved for the next chapter.

CHAPTER VI

MERTON VERSUS FLECK
THE STRUGGLE OVER THE INTELLECTUAL OWNERSHIP OF THE WASSERMANN REACTION

1. Introduction

In 1979 the University of Chicago Press published the English translation of Ludwik Fleck's long-neglected monograph *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Basel, 1935), which had only been saved from oblivion thanks to Thomas S. Kuhn's brief reference to it in the preface of his famous book *The Structure of Scientific Revolutions* (1962) as "an essay that anticipates many of my own ideas".[1] The English translation had been edited by the historian of science Thaddeus J. Trenn and the well-known sociologist Robert K. Merton. The latter had also mobilized financial resources, made arrangements with the publisher and persuaded Kuhn to contribute a foreword to it.[2]

Although Merton has thus been instrumental in bringing out a new English edition of Fleck's monograph, one need not unduly psychologize to assume that he must feel at least somewhat ambivalent about this pioneering contribution of the Polish bacteriologist and self-taught sociologist of science which his co-editor Trenn deems to be of "overwhelming" relevance for current research in the sociology, history, and philosophy of science.[3] After all, the approach adopted by Fleck differs fundamentally from Merton's own version of the sociology of science, which constituted the dominant if not exclusive 'paradigm' from the 1950s well into and 1970s.[4] There is, moreover, an undeniable similarity between Fleck's work and that of contemporary constructivists, several of whom had entered their professional careers in the late sixties or early seventies by subjecting the then dominant Mertonian paradigm to scathing criticism. At that time, invoking Kuhn, men like Mulkay, King, Barnes and Dolby criticized Merton's one-sided interest in the social and institutional aspects - particularly the normative structure or 'ethos' and the reward system - of science and took

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1 T.S. Kuhn, *The Structure of Scientific Revolutions*, Chicago (University of Chicago Press), 1962, pp. Vi-VII.

2 L. Fleck, *Genesis and Development of a Scientific Fact*, edited by Thaddeus J. Trenn and Robert K. Merton, Chicago (University of Chicago Press), 1979, p. XIV.

3 Ibid., p. XIV.

4 An authoritative exposition of the Mertonian paradigm can be found in Norman Storer's introduction to the collection of essays published as R.K. Merton, *The Sociology of Science: Theoretical and Empirical Investigations*, edited by N. Storer, Chicago (University of Chicago Press), 1973.

exception to his apparent neglect of its substantive content.[5] In his monograph Fleck had criticized the Durkheimian sociologists of knowledge in a rather similar vein for their treatment of the substance of scientific knowledge as exempt from sociological analysis (47/85). The 'post-Kuhnian' critics also questioned Merton's description and treatment of the 'scientific ethos'.[6] As we can see in retrospect, in due course a new orthodoxy emerged from these criticisms: the Strong Programme and other varieties of constructivism. It was to be expected that after the appearance of the English translation of his monograph, Fleck would be greeted and embraced as a worthy precursor and pioneer of modern constructivism.[7]

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5 The charge that Merton excluded the substantive content of scientific knowledge from sociological consideration may seem surprising in view of the fact that in his early doctoral dissertation *Science, Technology and Society in Seventeenth-Century England* (published in 1938) he had extensively analyzed the shifting 'foci of attention' of 17th-century English science, as influenced by military and socio-economic factors. This preoccupation with matters of cognitive substance in science is however much less prominent in Merton's post-war contributions to the sociology of science which actually provided the building blocks for the so-called Mertonian paradigm. One might even hazard to say that the early dissertation does not really fit the mature paradigm. As supportive evidence for this we have the testimony of Thomas Gieryn who has declared that when he became Merton's research assistant and read the early dissertation he "was struck immediately by the contrast between the book and then-current research (1975 or so) in the sociology of science by Merton and his close collaborators Harriet Zuckerman, Jonathan Cole, and Stephen Cole" (see T.F. Gieryn, 'Distancing Science from Religion in Seventeenth-Century England', *Isis* 79 [1988]: 582-93, on p. 582). Merton's spouse and close collaborator, Harriet Zuckerman, has also affirmed that, for all his interest in the possible influence of social, cultural and economic factors on 'problem choice' in science, Merton never went so far as to endorse the constructivist claim that the findings of science are socially determined. See H. Zuckerman, 'The Other Merton Thesis', *Science in Context* 3 (1989): 239-67, on p. 261. One could thus still maintain that, in this rather special sense, for Merton the content of science remains exempt from sociological scrutiny. Further confirmation for this assertion is finally provided by a recent article of Merton's pupil Stephen Cole, who bluntly states: "Up until the 1970s, sociologists of science did not examine the actual cognitive content of scientific ideas, as they believed that these were ultimately determined by nature and not a product of social processes or variables". See S. Cole, 'Voodoo Sociology: Recent Developments in the Sociology of Science', in P.R. Gross, M. Levitt, and M.W. Lewis (eds.), *The Flight from Science and Reason*, Baltimore and London (The Johns Hopkins University Press), 1997, pp. 274-87, on p. 274.

6 M.J. Mulkay, 'Some aspects of cultural growth in the natural sciences', *Social Research* 36 (1969): 22-52; M.J. Mulkay, 'Norms and Ideology in Science', *Social Science Information* 15 (1976): 637-56; M.D. King, 'Reason, Tradition and the Progressiveness of Science', *History and Theory* 10 (1971): 3-32; S.B. Barnes and R.G.A. Dolby, 'The Scientific Ethos: A Deviant Viewpoint', *Archives Européennes de sociologie* 11 (1970): 3-25. Kuhn has dissociated himself from these strident critics of Mertonian sociology and declared that their line of criticism was "seriously misdirected". See his Preface to *The Essential Tension*, Chicago (University of Chicago Press), 1977, pp. XXI-XXII.

7 "We do possess one fine pre-war work in the sociology of knowledge tradition which considers in detail the emergence of an accepted set of scientific doctrines and techniques. Ludwik Fleck's *Genesis and Development of a Scientific Fact* (1935), recently rescued from oblivion, has rapidly become recognized as a major contribution", B. Barnes and D. Edge (eds.), *Science in Context: Readings in the Sociology of Science*, Milton Keynes (Open University Press), 1982, p. 65.

In view of this situation, one would naturally be anxious to know Merton's considered views about the significance and merit of Fleck's pioneering work. He has commented more extensively on the latter's contribution to the sociology of science in the introduction to the Polish translation of his book *Social Theory and Social Structure* (1982) - as if to make it more difficult for most of his readers to satisfy their natural curiosity! I will take the liberty to cite from this source.[8] In this introduction Merton declares that Fleck's work "is of more than merely 'historical interest' for all of us interested in the increasingly evident linkages between the history, philosophy, and sociology of science". He also recognizes the similarity of Fleck's approach to modern forms of constructivism, but simultaneously attempts to distance it from what he sees as the extremely relativist and subjectivist tendencies of the latter:

> "Fleck's theoretical orientation finds elliptical expression in the title of his monograph which audaciously refers to the genesis *and development* of a scientific fact. In that view, facts are not instantly and permanently fixed, passive recordings of an external reality; they are constructed and consolidated through social processes involving individuals within a 'thought collective'. This sounds reminiscent - or better, anticipatory - of more recent ideas on the 'social construction of knowledge' and it is. But Fleck avoids the radical relativism of much contemporary theorizing along these lines in recognizing that along with the social interactions between individuals in the collective there is also, for him, "the objective reality (that which is to be known)"."

Referring to Fleck's notion of 'passive linkages' or 'passive couplings', Merton remarks that these elements represent the objective component of knowledge and concludes: "It is not the case, as a radical epistemological relativism would have it, that anything and everything goes". As I argued in Chapter II, Fleck's appeal to 'objective reality' and his use of the notion of 'passive linkages' prove a much less robust defence against antirealist positions than might appear at first sight and than Merton has taken them to be. On the other hand, I also believe that most forms of modern constructivism are less subjectivist and relativist than Merton evidently holds them to be. Few if any contemporary constructivists would actually be willing to endorse Feyerabend's slogan 'Anything goes!'. So the differences with Fleck may have been somewhat overstated.

On the more positive side, Merton notes Fleck's emphasis on the unanticipated consequences of cognitive and social action, especially in the latter's description of the Wassermann reaction as the unforeseen and unintended outcome of the interactions among the members of the serologists' collective (69/91). This special emphasis resonates well with

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8 As I am not fluent in Polish, I will cite from a copy of the original English text of the introduction which Merton was so kind to send me in response to a request for commenting on an earlier version of this chapter.

Merton's own early recognition of the importance of the "unanticipated consequences of purposive social action" for understanding the dynamics of social processes.[9]

What I find slightly surprising about Merton's remarks on Fleck in the introduction to the Polish translation of his book, is that he refrains from giving any negative comment on the distinctly 'collectivistic' bent of the latter's sociological approach, as several other commentators have objected to the "extremely anti-individualistic standpoint" assumed by Fleck[10]. From the point of view of Mertonian sociology, which holds a more balanced perspective on the relationship between individual and collective, there would be ample reason to take exception to Fleck's 'collectivism'. Fleck is however not unique in this regard. Since the times of Bacon, as Merton observes elsewhere, views on the progress of science and technology have fatefully alternated between two extremes, conceived as disjunctive possibilities: "*either* the social theory of discovery *or* the 'heroic' theory [...] For want of an alternative theory, we have been condemned to repeat the false disjunction between the heroic theory centered on men of genius and the sociological theory centered on the social determination of scientific discovery".[11] Apparently, Fleck also has become the victim of this 'false disjunction'. In his eagerness to avoid the pitfalls of the individualistic point of view in epistemology, he seems to have plunged himself right into the other, equally implausible, extreme (cf. 76/102).

The consequences of Fleck's 'collectivism' become apparent when we examine his description and analysis of the genesis and development of the Wassermann reaction as a diagnostically useful test for detecting syphilis (the 'scientific fact' to which the title of his monograph refers). Initially, Fleck points out, this test showed a bare 20 percent of positive outcomes in cases of confirmed syphilis, but this percentage would eventually increase to about 70 or 90. He considers this increase, which made the Wassermann reaction into a practically useful test, as the result of collective labour in which the ideas of individuals were largely irrelevant: "Collective experience thus operated in all fields related to the Wassermann reaction until, *with disregard for theoretical questions and the ideas of individuals*, the reaction became useful" (73/97; emphasis added). According to Fleck, the principal actors in the drama (*die Heroen der Handlung*, as the German original calls them) cannot tell us how it happened, "for they rationalize and idealize the development" (76/101). He holds their defective epistemology, based on an individualistic point of view, responsible for this alleged inability. In Fleck's view, this defective epistemology lies also at the root of the fierce dispute over "authorship and contribution to the discovery of this extremely important reac-

9 R.K. Merton, 'The Unanticipated Consequences of Purposive Social Action', *American Sociological Review* 1 (1936): 894-904.

10 R.S. Cohen and T. Schnelle, 'Introduction', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, p. XI.

11 Merton, op. cit. (note 4), p. 352.

tion" (176/101 note 24) which erupted in 1921 between several of the protagonists (among them August Wassermann and his former collaborator Carl Bruck), or "the lively polemics between, and personal protestations by, the various workers involved" (69/90). Fleck uses the occasion of this struggle over the *intellectual property* of the Wassermann reaction (for that is how we can describe it in Mertonian terms) to criticize each of the participants. He accuses them of using such unscientific notions as "lucky accident" or "the intuition of a genius" (76/102). If only they would adopt *his* superior sociological epistemology, Fleck seems to imply, then surely they would cease to fight over such futile matters as the intellectual ownership of a scientific discovery. The sociological significance of such disputes, apparent from a Mertonian perspective, has completely eluded him. Paradoxically, the 'collectivistic' bent of his sociological approach has led him to criticize the participants in this conflict rather than to treat such a dispute as a proper object for sociological analysis in its own right.[12] In line with his approach, Fleck considers science as an exclusively *cooperative* venture, whereas the scientific enterprise is better seen as one of *competitive* cooperation.[13]

In contrast to Fleck, Merton has tried to steer a careful middle course between the extremes of the individualistic (or 'heroic') and the collectivistic (or 'social') views. I think there is real virtue in this attempt. One of its lasting merits, in my opinion, is the recognition of frequently occurring conflicts over intellectual property (in the usual form of priority disputes) as a phenomenon that is characteristic of the institution of science and that should be seriously studied in its own right rather than politely ignored (as is often done by 'individualistic' writers) or condemned as utterly futile (as is often done by 'collectivistic' writers). Despite all 'post-Kuhnian' criticism of Merton's sociology of science in general and of his reconstruction of the 'scientific ethos' in particular, this valuable element deserves to be

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12 Elsewhere Fleck states the view that it is the democratic duty of every individual scientist to recede into the background, to "withdraw his own individuality into the shadow [..] in the service of the common ideal" (144/188). The editors (that is, presumably, Merton) of the English translation of Fleck's work give the following critical comment on this: "Yet scientists typicallly engage in competition, and priority disputes are not infrequent, which would suggest that Fleck's democracy of science remains incomplete" (163). A similar criticism could be made of Edgar Zilsel's thesis that with the founding of institutions like the Royal Society and the Académie des Sciences the modern pattern of scientific progress through cooperation has emerged: "Knowledge is no longer the business of men of letters, anxious for personal fame, or of disputatious scholastics: the modern western notion of scientific research has been arrived at" (E. Zilsel, *Die sozialen Ursprünge der neuzeitlichen Wissenschaft*, Frankfurt am Main [Suhrkamp], 1976, p. 150). Modern scientific research, based on cooperation, Zilsel maintains, has abandoned the quest for personal fame so characteristic of humanistic *literati*. But, then, why do we so often meet with priority disputes in the subsequent history of science? Just like Fleck, Zilsel sees the cooperation but not the competition present in the scientific endeavour.

13 The phrase 'competitive cooperation' is used by Merton in his essay on the scientific ethos. See Merton, op. cit. (note 4), pp. 273-74. Compare David Hull: "Science is both a highly competitive *and* a highly cooperative affair" (D. Hull, *Science as a Process*, Chicago and London [University of Chicago Press], 1988, p. 286).

preserved and, if possible, expanded. This is not to say that Merton's analysis is fully satisfactory. In the following sections I will argue that the Mertonian framework should be stretched by incorporating constructivist insights, so as to enable a more adequate treatment of disputes over intellectual ownership in science. A reconsideration of Fleck's case of the formation of the Wassermann reaction, and in particular of the conflict over intellectual property to which this discovery (or invention) gave rise, will provide the opportunity to assess the fruitfulness of these ideas.

2. Priority disputes and intellectual property

Although Merton had made his début as a sociologist of science in 1938 with the publication of his dissertation on *Science, Technology and Society in Seventeenth-Century England*[14], it was his 1942 essay on the so-called 'scientific ethos' - the normative framework for the conduct of science - which would provide the basic ingredient for the future Mertonian paradigm.[15] Compared to his pre-war work, in which he had analyzed the influence of Puritanism on 17th-century science and the relationship between science and technology, the focus is now much more on science as a relatively autonomous social institution, or in other words on 'pure' or 'academic' science. The normative foundations of *this* kind of science were formulated as *the* 'scientific ethos'. It took several more years before the outlines of the Mertonian paradigm became clearly visible. Important contributions were Merton's work on priority disputes (1957) and on 'multiple' or simultaneous discoveries (1961). These contributions constituted the heart of the Mertonian paradigm, because they forged a link between the normative structure of science (as described in the 1942 essay on the scientific ethos) and its reward system (to which later studies would be dedicated). Drawing the different threads together, Merton was finally able to offer a unified analysis of the functioning of science as a specific 'social system', in line with the explanatory schemes of functionalist sociology in general. This advance in theoretical coherence, however, went *pari passu* with a marginalization of the older theme of the relations between science and technology.

In his 1942 essay on the normative structure of science Merton defined the scientific ethos as "that affectively toned complex of values and norms which is held to be binding on the man of science".[16] In that early essay he specified four different norms comprising this ethos: communism, universalism, disinterestedness and organized skepticism (which together

14 R.K. Merton, *Science, Technology and Society in Seventeenth-Century England*, New York (Harper and Row), 1970 [1938].

15 R.K. Merton, 'The Normative Structure of Science', in Merton, op. cit. (note 4), pp. 267-78.

16 Merton, op. cit. (note 4), pp. 268-69. The formulation obviously predates the modern sensitivity to gender issues.

form the appropriate acronym CUDOS). He later added the values of originality and humility to this complex. For our purposes here, the norm of communism and the values of originality and humility are the most relevant.

The norm of communism implies that the results of scientific research are published within a reasonable time and assigned to the community. "Secrecy is the antithesis of this norm; full and open communication its enactment".[17] The substantive findings of science constitute a common heritage; they do not enter into the exclusive possession of the discoverer or his heirs. The rights of 'intellectual property' are extremely curtailed: "property rights in science become whittled down to just this one: the recognition by others of the scientist's distinctive part in having brought the result into being".[18] If one thinks about it, this is a very strange and curious notion of 'property'; it has indeed, as Merton notes in a later book, a "distinctive anomalous character".[19] One might even wonder whether the term 'property' is justifiably used at all in this context; wouldn't it be more appropriate to speak of a right of recognition of 'intellectual paternity' (or 'intellectual maternity' or, gender-neutrally, 'intellectual parenthood'[20]) rather than of a right of 'intellectual property'? In a footnote Merton gives some examples of the language used by scientists in speaking of their work to establish that "the notion of property is part and parcel of the institution of science", but these examples are not fully convincing.[21] True, they show that a property-*related* idiom is sometimes used by scientists, but they do not demonstrate the presence of the notion of 'intellectual property' in the precise, minimal sense in which it is used by Merton.

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17 Ibid., p. 274.

18 Ibid., pp. 294-95. Compare the description on p. 273.

19 R.K. Merton, *The Sociology of Science: An Episodic Memoir*, Carbondale and Edwardsville (Southern Illinois University Press), 1977/1979, p. 48.

20 It is not easy to meet the contemporary demands of political correctitude by introducing a more even-handed or gender-neutral terminology, because the associated connotations of established usages often run counter to this attempt. Thus Wilhelm Roentgen can be designated as 'the father of X-rays' without difficulty, but Marie Curie is just the mother of Irène Curie, not 'the mother of radio-activity'. Or as Merton observes: "to 'father' a science connotes being its unique or principal begetter while to 'mother' a science, should the metaphor ever be instituted, would at first probably connote looking after the fledgling with tender loving care". See Merton, op. cit. (note 19), p. 114.

21 Merton, op. cit. (note 4), pp. 294-95, note 19. Some of these examples may also raise doubts about the strictness of the norm of communism and about the alleged 'minimal' character of property rights in science. In Merton's first example, Ramsay is asking Rayleigh's "permission to look into atmospheric nitrogen" on which Rayleigh had been working. If Rayleigh is still supposed to have the right for giving such permission, then clearly property rights in science are not as drastically curtailed as is suggested by Merton. In fact, this question points to the wider problem of "protection of exploitation rights" or "the principle of fairness in exploitation of results", which, according to Jerry Ravetz, "has received no mention in the classic literature on the sociology, or ethics, of science". See J.R. Ravetz, *Scientific Knowledge and its Social Problems*, Harmondsworth (Allen Lane the Penguin Press), 1973 [1971], pp. 255-56.

Do we have to conclude, then, that Merton has invented this particular notion of intellectual property all by himself? No, his view reflects established usage in science from the end of the 18th century. An early instance (perhaps the earliest) in which this notion is employed in an eminently Mertonian sense is a letter written by Antoine-Laurent Lavoisier to Joseph Lakanal, deputy of the Convention, dated 18 July 1793. In this letter, Lavoisier is reacting to projected schemes for a common organization of scientists and artisans at a time that the continued existence of the Académie des Sciences is politically at risk. The most relevant passage of the letter reads as follows:

> "The scientist [*le savant*] works only out of dedication to science and to enhance the reputation which he enjoys. Once he has made a discovery, he will exert himself to publish it, and his aim has been achieved when he has secured his property, when it has become established that it is really his".[22]

Lavoisier contrasts this with the typical motivation of a technician or artisan (*l'artiste*) who acts out of an expectation of material benefit and tries to keep his discoveries and inventions to himself.

My admittedly speculative conjecture is that the notion of 'intellectual property' in science in its eminently Mertonian sense, as paradigmatically expressed in Lavoisier's letter to Lakanal, has originated during the Revolutionary period in France. At that time new patent and copyright laws, replacing the Ancien Regime system of royal privileges, sought a new legal basis in man's supposedly natural property rights in relation to his thoughts.[23] Thus Article 1 of the Patent Law of 1791 solemnly stated that every discovery or invention in all branches of industry was the property of its 'author'. The same principle was also adopted as the basis for a new copyright law (regulating *le droit d'auteur*), prepared by a committee that was presided by Lakanal.[24] It is not altogether impossible that Lavoisier borrowed the concept of intellectual property from the sphere of patent and copyright legislation, and adapted it to the domain of 'academic' science. In this transposition the rights of intellectual property must have become drastically curtailed.[25] Emanating from revolutionary France,

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22 A.-L. Lavoisier, *Oeuvres de Lavoisier*, Paris, 1868, Volume IV, p. 615. I owe this reference to C. Boers, H. Koningsveld and J. Mertens, *Natuurwetenschap in de samenleving*, Wageningen, 1976, p. 17. They rightly call attention to the significance and relevance of Lavoisier's letter for discussing the 'Mertonian' norms of science. Background information on the historical context of Lavoisier's letter is provided by R. Hahn, *Anatomy of a Scientific Institution: The Paris Academy of Sciences 1666-1803*, Berkeley (University of California Press), 1971, Chapter 8: Closing the Academy, especially p. 236.

23 F. Savignon, 'The French Revolution and Patents', *Industrial Property* 28 (1989): 391-400.

24 P. Roubier, *Le droit de la propriété industrielle*, Vol. I, Paris (Recueil Sirey), 1954, p. 68.

25 In a footnote in his dissertation Merton had written: "The frequency of disputes concerning priority [..] constitutes an interesting problem for further research. [..] The entire question is bound up with the rise of the concepts of plagiarism, patents, copy-rights and other institutional modes of regula-

the notion of intellectual property also gained currency in German idealist philosophy (Hegel, Schopenhauer) and science. In 19th- and early 20th-century German science, the notion of *geistiges Eigentum* is usually employed in a Mertonian sense.[26] A well-known example of early use in Germany (21 December 1823) is drawn from Goethe's *Conversations with Eckermann*:

> "Questions of science are very frequently career questions. A single discovery may make a man famous and lay the foundation of his fortunes as a citizen [..]. Every newly observed phenomenon is a discovery, every discovery is property. Touch a man's property and his passions are immediately aroused".[27]

To judge from the last sentence, Goethe must have understood very clearly what priority disputes in science were all about.

If priority disputes turn about matters of intellectual property, the objective *occasion* for their occurrence, according to Merton, is constituted by the fact that many discoveries in science are 'multiple' discoveries (discoveries made by more than one person independently of each other at about the same time) rather than 'singletons'. Goethe, for example, had been personally involved in at least two 'multiples'.[28] The regular appearance of multiple discoveries in human history is a phenomenon that has repeatedly been noted. The observation of this phenomenon has often inspired strongly deterministic and 'collectivistic' views. Thus it is held that discoveries occur because their time has come, not because some scientific genius

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ting 'intellectual property' [..]". See Merton, op. cit. (note 14), p. 169. Although Merton has followed up the first part of this youthful suggestion with a delay of 20 years in his 1957 essay on 'Priorities in Scientific Discovery' (see Merton, op. cit. (note 4), pp. 286-324), he has never heeded the second part of it. I think that his increasingly onesided focus on the norms of 'pure' science (rather than the relations between science and technology) has been responsible for this omission. Anyway, the period of the French Revolution should be a 'strategic research site' for examining this matter more searchingly. For an earlier historical period, Rob Iliffe has analyzed how the conventions and procedures of the Royal Society for establishing priority failed to resolve the conflict between Huygens and Hooke over the invention of the balance-spring watch. See R. Iliffe, '"In the Warehouse": Privacy, Property and Priority in the Early Royal Society', *History of Science* 30 (1992): 29-68.

26 One example. Emil Behring wrote about the priority struggles in which he was involved: "So habe ich nur mein geistiges Eigentum zu wahren gesucht, das aber auch mit aller mir zu Gebote stehender Rücksichtslosigkeit" ("In that manner I have tried only to secure my intellectual property, albeit with all the ruthlessness I could muster"). Cited in H. Satter, *Emil von Behring*, Bad Godesberg (Inter Nationes), 1967, p. 20.

27 Cited in E.J. Hobsbawm, *The Age of Revolution: 1789-1848*, New York (Mentor Book), without year [1962], p. 327.

28 His discovery of the intermaxilliary bone in the human skull, reported to a friend in 1784, had been anticipated by Félix Vicq d'Azyr in 1780. The second discovery, the theory that the skull is made of modified vertebrae, was independently arrived at by Lorenz Oken. For Goethe's negative comments on the latter's presentation of the discovery, see A. Schierbeek, *Goethe als Natuuronderzoeker*, Amsterdam (Meulenhoff), 1944, p. 55.

happens to be around. The counterfactual claim is advanced that if Newton, Lavoisier, Darwin and Einstein had never lived, the discoveries which bear their names would still have been made at just about the same time. 'Great men' are therefore seen as dispensible.[29] In 1922 the American sociologists William Ogburn and Dorothy Thomas introduced a quantitative method in the study of multiple discoveries.[30] They (that is, Dorothy Thomas) compiled a list of 148 discoveries and inventions made independently by two or more persons. Further research, they stated, should be able to extend their list considerably. When Merton took up the study of 'multiples' many years later, he first tried to continue the quantitative approach of Ogburn and Thomas.[31] Later he chose a different tack and proposed the bold hypothesis that "all scientific discoveries are in principle multiples, including those that on the surface appear to be singletons".[32] Many alleged singletons have turned out to be duplications after the belated publication of the notebooks of famous scientists like Cavendish or Gauss. But Merton's main point is that in actual practice scientists act on the operative assumption that discoveries in science are potential multiples: if they don't take prompt measures, scientists believe, they will be forestalled by others. (Such a rush to ensure priority can be clearly seen in Watson and Crick's successful attempt to be the first, before Linus Pauling, to elucidate the structure of DNA.[33]) Merton also resists the 'collectivistic' conclusion that is usually drawn from the existence of multiples. If all discoveries are (in principle) multiples, this would not mean that all talents are reduced to the same level. "The greatest men of science", Merton notes, "have been involved in a multiplicity of multiples" (Lord Kelvin, for example, has been involved in at least 32 multiples!).[34] In this manner he tries to steer a middle course between the 'individualistic' (or 'heroic') and 'collectivistic'

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29 In an early essay Fleck also drew attention to the phenomenon of multiple discoveries to defend a 'collectivistic' view: "The relative independence of the cognized from the individual is well illustrated in the fact that different individuals frequently make the same discovery or invention simultaneously but independently from one another". See L. Fleck, 'On the Crisis of "Reality"', in Cohen and Schnelle, op. cit. (note 10), p. 50.

30 W.F. Ogburn and D. Thomas, 'Are Inventions Inevitable? A Note on Social Evolution', *Political Science Quarterly* 37 (1922): 83-98.

31 R.K. Merton, 'Singletons and Multiples in Science', in Merton, op. cit. (note 4), pp. 343-71. On p. 364 he reports having examined, together with Elinor Barber, 264 cases of multiples in detail in the context of a methodical study of multiples. The Merton-Barber data on multiples have been further analyzed in D.J. de Solla Price, *Little Science, Big Science*, New York and London (Columbia University Press), 1971 [1963], pp. 66 ff.

32 Ibid., p. 356.

33 Merton's essay 'Behavior Patterns of Scientists', written in 1968, reflects on *The Double Helix*, James Watson's famous account of this discovery. See Merton, op. cit. (note 4), pp. 325-42.

34 Merton, op. cit. (note 4), p. 367.

(or 'social') theories of discovery, granting that all scientific discoveries are multiples or potential multiples while still leaving room for the work of genius.[35]

The ubiquity of multiples is one of the two pillars of Merton's work on priority disputes. The regular occurrence of simultaneous and independent discoveries constitutes the objective *occasion* for disputes over priority; it is however the normative structure of science (the 'scientific ethos') which provides the *cause* or *grounds* for such quarrels. As already said, priority disputes turn about matters of intellectual property. Science as an institution is concerned with the advance of ('certified') knowledge; the more knowledge is advanced, the better, hence the high premium set on *originality*. In the institutional system of science original contributions, of course after having been published and thus made available to the community, are exchanged for recognition and esteem and all the possible rewards which may go with these. Such rewards range from *eponymy* (e.g. Halley's comet, Boyle's law, Brownian movement, the Wassermann reaction), medals, fellowships, membership of prestigious organizations to ennoblement (in the United Kingdom) and, of course, the Nobel Prize. All these rewards constitute a finely-graded system of stratification in science. The basic point, in Merton's words, is that "honorific recognition by fellow-scientists" is "the coin of the scientific realm".[36] Since the early 1960s, with the emergence of the computerized Science Citation Index, this informal social process of bestowing recognition has to some extent become formalized and instrumentalized.[37]

The great frequency of struggles over priority in the annals of science does not result from the psychological characteristics of individual scientists, but from "the institution of science, which defines originality as a supreme value and thereby makes recognition of one's

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35 Although I sympathize with Merton's attempt to steer a middle course, I think he has not been fully successful in its execution. From Merton's 'bold hypothesis' that all discoveries are multiples or potential multiples, it would seem to follow that 'great men' are indeed dispensible, however much he may deplore this conclusion. The formulation of this hypothesis can also be charged with being in principle immune to refutation and thus unsuitable for scientific investigation. Indeed this criticism had already been stated by Merton himself when he expressed the suspicion that it might constitute an "incorrigible" and "self-sealing hypothesis, immune to investigation" (Merton, op. cit. [note 4], p. 357). Merton does not actually need this 'bold hypothesis' in his study of priority disputes; the much less controversial assumption that multiples are not uncommon would be sufficient. (Constructivists might wish to advance the further criticism that on closer inspection many alleged multiples will turn out to be not exactly the 'same' discoveries.)

36 The idea of the 'credibility cycle', developed by Latour and Woolgar, is that this type of 'currency' can be *converted* into other advantages like higher salaries, research grants, additional assistants, material equipment etcetera. The accumulation of 'recognition and esteem' or rather 'credibility' in science resembles the accumulation of capital. Cf. B. Latour and S. Woolgar, *Laboratory Life*, Beverly Hills and London (Sage), 1979, Chapter 5.

37 Merton discusses the origins and (ab)uses of the Science Citation Index and its relationship to his sociological views in Merton, op. cit. (note 19), pp. 47-54.

originality a major concern".[38] Yet the institutional emphasis on originality is counterbalanced, to some extent at least, by the institutional endorsement of the value of humility. This value is expressed, for example, in the well-known epigram which Newton made his own:

> "If I have seen farther, it is by standing on the shoulders of giants".[39]

The value of humility also enjoins scientists to acknowledge their indebtedness to prior work and to insist on the limitations of their personal contributions and of scientific knowledge in general (cf. the almost perfunctory confessions of how little one knows). In the contest between recognized originality and humility, it is however the former that easily gains the upper hand: "Great modesty may elicit respect, but great originality promises everlasting fame".[40] The value of originality leads scientists (or others who defend their interests) to engage in priority disputes, but the value of humility exacts a psychological toll for that: those scientists will be plagued by mixed feelings for doing so.[41] The tension in the normative structure of science, between the values of originality and humility, finds expression in strong feelings of ambivalence towards conflicts of priority. Such conflicts are almost always experienced as unsavoury episodes. This also explains, Merton maintains, the resistance to the systematic investigation of these phenomena.

Later work by Merton and his school has examined the functioning of the reward system in greater detail. Is the institution of science fair in allocating rewards? Merton's pupil Jonathan Cole has investigated whether women scientists have an equal chance of acquiring recognition and esteem.[42] Merton himself, together with Harriet Zuckerman, has documented the operation of the so-called *Matthew effect* in science. They found that more credit tends to be given to those scientists who already have a reputation.[43] Or as the Gospel of

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38 Merton, op. cit. (note 4), p. 294.

39 See for an historical detective story on the origins of this epigram, R.K. Merton, *On the Shoulders of Giants: A Shandean Postcript*, San Diego (Harcourt Brace Jovanovich), 1985 [1965].

40 Merton, op. cit. (note 4), p. 308.

41 Emil Behring is exceptional in declaring that he enjoyed priority disputes: "Solche Kämpfe haben in der eintönigen Forscherarbeit immer etwas Erfrischendes" ("Such battles have always something refreshing about them in the monotonous labour of research"). Cited in Satter, op. cit. (note 26), p. 20.

42 J.R. Cole, *Fair Science: Women in the Scientific Community*, New York and London (The Free Press), 1979. For a more recent work on this subject see H. Zuckerman, J.R. Cole and J.T. Bruer (eds.), *The Outer Circle: Women in the Scientific Community*, New York and London (W.W. Norton & Company), 1991.

43 Merton, op. cit. (note 4), pp. 439-59. See also R.K. Merton, 'The Matthew Effect in Science II: Cumulative Advantage and the Symbolism of Intellectual Property', *Isis* 79 (1988): 606-23.

Matthew already stated: "For onto every one that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away that which he hath".

From the late 1960s onwards, the Mertonian paradigm received a barrage of criticism from a younger generation of 'post-Kuhnian' sociologists of science. This wave of criticism reflected both the decreasing hegemony of logical positivism within the philosophy of science and a growing dissatisfaction with functionalism in sociology. According to his critics, Merton's sociology of science was implicitly based on a positivist philosophy which considers discoveries as unproblematic occurrences. Kuhn's work, in particular his famous essay on the 'discovery' of oxygen, was cited to demonstrate the untenability of positivist views of discovery.[44] (If Fleck's monograph had been accessible at that time, critics could also have referred to his work!). M.D. King spelled out the implications of Kuhn's work for the study of priority disputes:

> "Merton, true to his positivism, does not allow that disputes might arise over the intellectual question of precisely *what* has been discovered when a discovery is made [..]. For him, the point at issue is the social and historical one: *who* discovered it and *when*? What is at stake is a social matter: namely, the individual scientist's property rights in discoveries, and the prestige that accrues to him as a 'propertyholder'. Priority disputes are social facts, requiring explanation in terms of other social facts. But what Kuhn's position implies is that for certain discoveries, at least, the intellectual debate over what has been discovered and the social dispute as to who discovered it are inextricably intertwined. The one issue cannot be resolved in isolation from the other: to concede priority to a discoverer is to acknowledge as authoritative his interpretation of the discovery or, in Kuhn's terms, to treat his work as paradigmatic".[45]

In other words, Merton's decision to exclude the content of science from sociological consideration may become a crippling handicap even in pursuing his favoured pastime, the study of priority disputes.

King's criticism must however be qualified and mitigated to some extent. In his essay on the history and systematics of sociological theory Merton showed himself to be keenly aware of the problematic nature of the 'sameness' of alleged examples of multiple discoveries and inventions:

> "It is no easy matter to establish the degree of similarity between independently developed ideas. Even in the more exact disciplines, such as mathematics, claims of independent multiple inventions are vigorously debated. The question is, how much overlap should be taken to constitute 'identity'? A careful comparison of the non-Euclidean geometries invented by Bolyai and Lobachevsky, for example, maintains

44 T.S. Kuhn, 'The Historical Structure of Scientific Discovery' [1962], reprinted in Kuhn, op. cit. (note 6), pp. 165-77.

45 King, op. cit. (note 6), p. 19.

> that Lobachevsky had developed five of the nine salient components of their overlapping conceptions more systematically, more fruitfully and in more detail".[46]

Despite the qualification of King's criticism that in the light of this passage is clearly called for, it still remains true, I think, that in the Mertonian study of priority disputes questions of scientific content are largely excluded from consideration. Merton appears to have followed the procedure of first deciding pragmatically as an analyst which inventions or discoveries exhibit sufficient similarity to be regarded as multiples (this will establish the list of multiples) and then concentrating on what King refers to as the 'social matter' of the priority disputes. Insofar as Merton's work does not show the 'inextricable intertwining' of the intellectual debate and the social dispute, King's criticism still stands.

What it might mean to show such 'inextricable intertwining' can be illustrated with a well-known example. Steven Shapin has sketched how the historical study of the notorious priority dispute between Newton and Leibniz, and their respective followers, over the invention of 'the' calculus might profit from a closer examination of the various cognitive (even metaphysical) issues involved and their links with political struggles at the English Court.[47] The important thing to note is that, although the Newton-Leibniz case counts as a classic example of a multiple invention and it is quite customary to speak of the invention of 'the' calculus, the contributions of both were not exactly equivalent. Leibniz's calculus was operationally different from Newton's and based on different metaphysical foundations. Those metaphysical foundations were politically relevant in early 18th-century England because they were held to entail different implications for the position of the king, a question over which Court Whigs (Newtonians) and Country Whigs were divided. The fierce battle of the Newtonians against the claims of Leibniz can be better understood from this background.

What has just been said about the Newton-Leibniz case, viz. that their respective contributions were far from identical, may also be valid for many other examples of multiple discoveries and inventions. The Ogburn-Thomas list therefore deserves critical scrutiny. Yehuda Elkana, who has examined the case of the allegedly simultaneous discovery of the energy conservation law in detail, has stated as a general conclusion: "[T]here are no identical discoveries made in full independence from each other: the problems posed are at least

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[46] R.K. Merton, *Social Theory and Social Structure*, New York (The Free Press), 1968, pp. 9-10. Merton refers to the historical work of B. Petrovievics on the Bolyai-Lobachevsky case and on the Darwin-Wallace case written during the 1920s and, further, to the more recent essay by Thomas Kuhn on the multiple discovery of energy conservation.

[47] S. Shapin, 'Licking Leibniz', *History of Science* 19 (1981): 292-305. For similar analyses of several other discoveries, see S. Schaffer, 'Scientific Discoveries and the End of Natural Philosophy', *Social Studies of Science* 16 (1986): 387-420. Schaffer highlights the strategic significance of 'discovery stories' for the emergence of disciplined sciences in the early 19th century.

slightly different from each other and so are the solutions".[48] If this general conclusion can be sustained, it provides additional support for King's view that the question at issue in priority disputes cannot be limited to: *who* discovered it and *when*, but has to be extended to: *what* has been discovered (and by *whom*).

Around 1970 Merton was not only criticized for his neglect of the content of science. Barnes and Dolby, and Mulkay even more, questioned the effective power of the norms and values of the scientific ethos to govern or guide the behaviour of scientists, and pointed out that these norms and values were repeatedly violated anyway.[49] The 'scientific ethos' could therefore not provide, they asserted, an effective set of rules for regulating the internal affairs of science. At the most it could be considered a suitable ideological repertoire to be used in external contacts with lay-people for furthering the interests of scientists. This issue, touching as it does on one of the fundamental problems of theoretical sociology, is too large to be discussed in this chapter.[50] Here I will deal only with a less radical line of criticism present in the contribution of Barnes and Dolby which is directly relevant to the subject of intellectual property.

Barnes and Dolby state that "the possessability of discoveries" represents a historically variable institutional form which reached its zenith in the professionalized science of the 19th century (and even then did not operate ideally) and has declined in importance ever since. They emphasize the inherent difficulties in attributing a discovery to one person and point out that "discovery as a property right is not as clear and natural as has been thought" [51] (as if Merton had ever asserted anything of this kind!). They also refer to emerging new patterns: "The tendency for research to be done by whole teams, no one member of which can

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[48] Y. Elkana, 'Is There a Distinction between External and Internal Sociology of Science?', in Cohen and Schnelle, op. cit. (note 10), pp. 309-16, esp. p. 314.

[49] Barnes and Dolby, op. cit. (note 6); Mulkay, op. cit. (note 6).

[50] Ultimately, this issue has to do with finitist objections to the 'empowering' of norms - cf. Barry Barnes's criticism of 'normative functionalism' as discussed in Chapter II. In his eyes, Merton's treatment of the 'scientific ethos' would surely qualify as an example of 'normative functionalism'. David Bloor also notes that "much British sociology of knowledge rejects the standard 'Mertonian' account of 'norms' in explaining, say, scientific behavior". See D. Bloor, 'Left and Right Wittgensteinians', in A. Pickering (ed.), *Science as Practice and Culture*, Chicago and London (The University of Chicago Press), 1992, pp. 266-82, on p. 273 (note). It would seem, therefore, that the gap separating Mertonianism and (moderate) constructivism remains unbridgeable at this basic level, despite my effort in this chapter to amalgamate insights from both traditions. Notice, however, that recently Barnes, Bloor and Henry have pronounced themselves much more favourably on Mertonian sociology than is common among constructivists and even state that Merton's implicit vision of the individual and of the relationship between individual and collective has much to recommend itself. They hold that a balanced view like Merton's is regrettably absent in many contemporary forms of constructivism. See B. Barnes, D. Bloor and J. Henry, *Scientific Knowledge: A Sociological Analysis*, London (Athlone Press), 1996, p. 114.

[51] Barnes and Dolby, op. cit. (note 6), p. 21.

be called the discoverer of anything produced, has not caused the tension that might have been expected".[52]

I think that this prediction of the impending demise of intellectual property may be somewhat premature.[53] There is indeed a secular historical decline in the relative frequency of priority disputes, as has been established by Merton himself.[54] Apart from that, it must be noted that there *are* cases in which team research has produced tensions with regard to intellectual property.[55] The question requires at least more empirical investigation before such confident statements can be made with any assurance.

The case to be discussed in the next section, namely the struggle over the intellectual property of the Wassermann reaction, is interesting precisely from this point of view: although Wassermann and Bruck initially belonged to the same 'team' which developed the serodiagnosis of syphilis, they later bickered between themselves over each other's share in the discovery (the battle was however not confined to former 'team' members because Wassermann's erstwhile rival and critic, Eduard Weil, also entered the fray). It is remarkable that Merton has nowhere analyzed such *intellectual property battles* (to have a term for the more general category which includes priority disputes) between members of one research team. What he has analyzed, in his later work together with Harriet Zuckerman, is the operation of the so-called *Matthew effect* both with regard to multiple discoveries and multi-authored articles (the latter normally coming from research teams).

52 Ibid., p. 22.

53 The position taken by Barnes and Dolby resembles that of Zilsel (see note 12). For the latter, the rise of cooperative scientific research during the 17th century entailed the decline of the Renaissance scholar's quest for personal fame. Now Barnes and Dolby venture a similar prediction for 'team research' in the 20th century.

54 Merton, op. cit. (note 4), p. 365.

55 A notorious example is the "scandal" (Fred Hoyle) of the discovery of pulsars in 1967 by the graduate student Jocelyn Bell, for which her boss Anthony Hewish received the Nobel Prize in 1974. See W. Broad and N. Wade, *Betrayers of the Truth: Fraud and Deceit in Science*, Oxford (Oxford University Press), 1985 [1982], pp. 143-49. Steve Woolgar has analyzed different 'discovery accounts' produced by the participants involved in this discovery and by others; see S. Woolgar, 'Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts', *Social Studies of Science* 6 (1976): 395-422. Another example is the dispute that erupted after the discovery of insulin in 1921-22 between the members of the 'team' (Banting, Macleod, Collip, and Best) at the University of Toronto. See M. Bliss, *The Discovery of Insulin*, London and Boston (Faber and Faber), 1988 [1982].

The following matrix indicates the coverage of, and the gap in, Merton's scientific interests:

	struggles over intellectual property	Matthew effect
multiple discoveries	+	+
multiple authorship	-	+

The case of the conflict over the Wassermann reaction has to be placed, at least in part, into the empty entry of this matrix. I note in passing that the name of this serological test illustrates the operation of the Matthew effect with respect to *eponymy*: while the first publication announcing the serodiagnosis of syphilis was headed by the names of Wassermann, Neisser, and Bruck, only the first-mentioned author was eventually honoured in the name of the reaction. This eponymous fact would become a strong trump card for Wassermann in the battle with his former collaborator and critic.

3. The quarrel over the Wassermann reaction

Fleck sees the genesis and development of the Wassermann reaction (to be abbreviated as WaR) as a process of *collective* experience in which the 'technical perfection' of this serological test was gradually realized. In his eyes the actual 'authorship' essentially belonged to the entire collective and not to any particular individual (78/104). It is therefore not amazing that he cannot sympathize with any of the protagonists who became embroiled in the struggle over the intellectual ownership of the serodiagnosis of syphilis: they all must have been wrong for the simple fact of having engaged themselves in such a futile dispute! What is more, Fleck believes that he is able to explain why the views of each of the participants are necessarily flawed and distorted from the character of the collective process itself in which the WaR was constituted. He declares in a note, after the disclaimer that he does not want to minimize the merits of a research scientist or even to discuss merits: "I have listed various views on authorship and contribution to the discovery of this extremely important reaction *only for epistemological purposes*: to show that everybody makes mistakes" (176/101 note 24, emphasis added; the last sentence reads in the German original: "um zu zeigen, dass sie alle danebengreifen", which is perhaps better translated as: "to show that they are all wide of the mark"). In my view it is rather naive to use the views expressed by those involved in this polemic as material for epistemological analysis without taking the stategic context of a dispute over intellectual ownership into account.

Fleck's description of the genesis and development of the WaR as a process of collective experience has an indisputably irrational taint about it. In their introduction to the collection

Cognition and Fact, Robert Cohen and Thomas Schnelle aptly summarize Fleck's account as follows: "Fleck [..] denotes the course of research as a zig-zag determined by accidents, false paths, and mistakes. Epistemologically, the original foundations of work are slowly transformed for the scientists - in retrospect, however, the collective knows nothing of this.[..] The shifts in the self-conceived contents of research occur in the collective unnoticeably for the individual".[56] In this connection Fleck holds a view which I would like to designate, for clarity's sake, as the thesis of the *retroactive epistemological distortion effect*: after the completion of their investigations the scientists involved are no longer able to adequately represent the complicated route which has led to the final result. They are even supposed to lose the understanding for the views which they themselves held previously.[57]

The prime example for such an extreme distortion effect provided by Fleck is Wassermann's account of the discovery of the serodiagnosis of syphilis which he presented in December 1920 before a meeting of the Berlin Medical Society.[58] It was this lecture, published on 28 February 1921, which would arouse indignant responses from his former critic Eduard Weil and his one-time collaborator Carl Bruck (Wassermann's former assistant Carl Lange also reacted, but in a less indignant mood) and thus unleash an acrimonious quarrel over intellectual property.

In his lecture Wassermann declared: "You will remember that, when I created the serodiagnosis of syphilis, I proceeded from the idea, and with the clear intention, of finding a diagnostically usable amboceptor [..]".[59] According to Fleck, this statement is utterly misleading. When in 1906 Wassermann and his collaborator Bruck, in cooperation with the Breslau dermatologist Albert Neisser, undertook to apply Jules Bordet's recently developed complement fixation test to syphilis, they were, again according to Fleck, *primarily* aiming at the detection of syphilitic antigen and only *secondarily* at the detection of syphilitic antibo-

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56 Cohen and Schnelle, op. cit. (note 10), pp. XXII-XXIII.

57 In the English translation of Fleck's monograph, the editors refer to the discussion of "the retroactive effect" in Merton, op. cit. (note 57), pp. 16, 17, 37 (See Fleck: 43 note). Although there is indeed a certain analogy with Merton's notion of "the retroactive effect", Fleck's thesis goes further in that it postulates more extreme distortions as a consequence of the retroactive effect. Merton refers to the retroactive effect of new knowledge "in helping us to recognize anticipations and adumbrations in earlier work" (p. 37). Fleck refers to the presumed retroactive effect of new knowledge, acquired through collective experience, in making previously held views unintellligible to the participants themselves.

58 A. von Wassermann, 'Neue experimentelle Forschungen über Syphilis', *Berliner klinische Wochenschrift* 58 (1921): 193-97.

59 Ibid., pp. 193-94. An amboceptor is an antibody which, according to Ehrlich's theory, has both an affinity for its homologous antigen and for complement. Amboceptors can therefore be used in a complement fixation test.

dies (amboceptors).[60] In their initial experiments the quantitative results of antigen detection were also much more impressive than those of antibody detection: a success rate of 64 out of 76 confirmed syphilitic cases against a poor figure of 49 out of 257 cases (that is, 19 percent). However, in the subsequent history of the serodiagnosis of syphilis the tables would be completely turned. It was shown in 1907 that antigen detection was altogether unsuitable for a diagnostic reaction. On the other hand, the results of antibody detection would be gradually improved in a collective process of 'technical perfection' until a practically acceptable success rate (70 - 90 percent) was achieved. According to Fleck, Wassermann projected, in his lecture of December 1920, the final result of this historical development into its initial stage. This distortion is conceived as an inevitable consequence of Wassermann's participation and involvement in the collective development process:

> "The ultimate outcome of this research thus differed considerably from that intended. But after fifteen years an identification between results and intentions had taken place in Wassermann's thinking. The meandering progress of development, in all stages of which he was certainly deeply involved, had become a straight, goal-directed path. How could it be otherwise? With the passing of time, Wassermann amassed further experience, and as he did so lost the appreciation of his own errors" (76/101).

Is Fleck's explanation plausible? Instead of giving a direct answer to this question, it is better to raise the preliminary question whether the 'distortion effect' for which an explanation is sought is actually of the kind described by Fleck. Had Wassermann at the end of 1920 really 'forgotten' his original views on the nature of the serodiagnosis of syphilis?

Fleck's account of the 'distortion effect' occurring in Wassermann's lecture of December 1920 is crucially dependent on his assertion that antigen detection rather than antibody detection was the *primary aim* of Wassermann, Neisser and Bruck in their first two publications on the serodiagnosis of syphilis. This assertion is contestable. Starting from the original intention of these authors, namely to apply a modified version of Bordet's complement fixation test to syphilis, there is no reason at all for such a onesided emphasis on antigen detection. The complement fixation test is in principle perfectly symmetrical: depending on the experimental setup, it can be used for either antigen detection or antibody detection. The modification that had been introduced shortly before by Wassermann and some of his collaborators - the use of so-called 'bacterial extracts' instead of suspensions of entire bacteria -, does not affect the symmetry of the test.[61] (It was practically relevant for the development

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60 In the following description of the development of the WaR there is some overlap with the previous chapter. Rather than having to refer back to this chapter several times, I have found it preferable to tell a more continuous and connected story here.

61 Eduard Weil and his collaborators at the Hygienic Institute of the German University in Prague had already criticized this modification of the complement fixation test, in particular Wassermann's claim that it was able, as shown in experiments with typhoid, tuberculosis and meningitis, to detect minute quantities of resolved antigen. This extreme sensitivity was highly suspicious in the eyes of Weil. From the very outset he was therefore skeptical of the claims made with regard to antigen

of the serodiagnosis of syphilis, because the causative agent had resisted, since its discovery, all attempts at cultivation. Wassermann and Bruck therefore used extracts from the livers of stillborn luetic babies, which were known to swarm with spirochaetes, as 'antigen' in their original trials.) The central claim of Wassermann, Neisser and Bruck in their first two publications was that they had established "the purely scientific fact" of a specific reaction occurring between luetic antigen and luetic antibody; at the same time they conceded that this reaction did not yet constitute a practically usable method (*eine Methode für den Praktiker*). It is thus understandable that the authors stressed the *symmetry* of the reaction: contrary to Fleck's assertion, they declared repeatedly that the reaction could be used in principle both for antigen and for antibody detection. The investigators were not the least discouraged by the unfavourable results obtained with antibody detection: "He who has witnessed and participated in the development of biological diagnostics, knows which contradictory results various authors have [..] initially obtained".[62] The cardinal point ignored by Fleck is that the investigators, to defend their claim that they had found a specific antigen-antibody reaction for syphilis, had to uphold its symmetry too. If the reaction did not work both ways (for antigen and for antibody detection), its specificity would have become in doubt.

We do however find a pronounced preference for antigen detection, at the expense of antibody detection, during a *second* stage in the development of the WaR when it was a matter, not of establishing its existence, but of making it practically useful. In the course of their cooperation, a disagreement arose between Wassermann and Neisser on the further practical development of the serological reaction for syphilis and its application to clinical practice. Neisser wanted to reserve this work, for the time being, exclusively to his own clinic in Breslau and to his experiment station in the Dutch East Indies.[63] Their ways parted in June 1906. Wassermann's collaborator Bruck took Neisser's side and moved from Berlin to Breslau, to become the latter's foremost serologist. It is in the early serological work coming from the Breslau clinic that we find a heavy emphasis on antigen detection. There were special clinical reasons for this particular emphasis[64], but anyway it is clear that Wassermann, after his rupture with Neisser, cannot be held responsible for it.

Antigen detection did not survive some curious discoveries that were reported in the year 1907. First it was established by several investigators (Weil and Braun, Michaelis, Fleischmann, Landsteiner, Müller and Pötzl) that in the serodiagnosis of syphilis luetic liver

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detection in the serodiagnosis of syphilis.

62 A. Wassermann, A. Neisser, C. Bruck, and A. Schucht, 'Weitere Mitteilungen über den Nachweis spezifisch luetischer Substanzen durch Komplementverankerung', *Zeitschrift für Hygiene und Infektionskrankheiten* 55 (1906): 451-77, p. 476.

63 In terms of Jerry Ravetz (see note 21) this conflict can be interpreted as turning on the 'exploitation rights' of a new scientific finding.

64 See Chapter V.

extracts could be replaced without injury by aqueous extracts from *normal* (non-syphilitic) organs. Soon after it was also found out by some researchers (Landsteiner, Levaditi, Porges and Meier) that, instead of aqueous extracts, *alcoholic* normal extracts could also be used for this purpose. The curious thing was that those normal extracts, whether aqueous or alcoholic, reacted with syphilitic but not with normal sera, as was indicated by the fixation of complement. While antigen detection had now to be abandoned, antibody detection, by contrast, was consolidated and much improved.[65] The elegant symmetry of the original idea, on which the WaR had been devised, was thus completely destroyed. Despite, or rather owing to its increased practical usefulness (for antibody detection), the nature of the WaR became something of a scientific mystery. Its existence constituted a veritable *anomaly* for immunology in the early decades of this century.

The riddle of the nature of the WaR would not be solved until after 1942, when Pangborn isolated and biochemically identified the substance against which the serum of syphilitic patients reacted. It was a phospholipid, which she dubbed *cardiolipine*.[66] Cardiolipine is present in the syphilis spirochaete (treponema) but is also a normal constituent of host tissue. Blessed with the benefit of hindsight, Bernard Zalc expresses his surprise "that none of the researchers involved in the field of syphilis diagnosis had imagined that [..] the antigen could be shared by the treponema and normal organ".[67] Without invoking this retrospective wisdom, it is perhaps more easy to understand why Wassermann and his contemporaries were so much at a loss in interpreting the nature of the WaR. At least there was no lack of theoretical proposals for solving the riddle. Because Fleck considers the development of the WaR exclusively from a practical and technical aspect, he dismisses "the theoretical questions and ideas of individuals" (73/97) as unimportant. Such a dismissive attitude is not conducive to a careful analysis of Wassermann's pronouncements of December 1920 and of the ensuing polemics to which they gave rise. It also turns the development of the WaR into an irrational and quasi-mysterious collective process in which "the shifts in the self-conceived contents of research occur [..] unnoticeably for the individual".[68]

The new findings of 1907 were anomalous for several reasons. They were at odds with the then dominant idea of *specificity*, according to which immune reactions had to be directed at the causative agents or their products. Given its reactivity with normal organ extracts, the WaR was apparently not directed at the syphilis spirochaete or its products. The solubility in alcohol of the socalled 'antigen' provided a further paradox. From this it was concluded

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65 See Chapter V.

66 B. Zalc, 'Some Comments on Fleck's Interpretation of the Bordet-Wassermann Reaction in View of Present Biochemical Knowledge', in Cohen and Schnelle, op. cit. (note 10), pp. 399-406.

67 Ibid., p. 405.

68 Cohen and Schnelle, op. cit. (note 10), pp. XXII-XXIII.

that the 'antigen' was not a protein but in all likelihood a lipid or 'lipoid'. The prevailing dogma at the time, however, was that antigens had to be proteins, for only proteins were considered sufficiently complex to be the bearers of biological specificity.[69] And if the socalled 'antigen' was not a real antigen, then the substance in the serum of syphilitic patients that reacted with this 'antigen' could not be a real antibody. Initially, Wassermann himself was among those who drew these conclusions from the new findings. For a time he even entertained the hypothesis that the socalled 'antibodies' of his reaction were syphilitic toxins.

A different, and in more than one aspect heretical, view on the nature of the WaR was developed by Eduard Weil and Hugo Braun in the years 1907-1909.[70] They did not reject the idea that the WaR involved real antibodies: according to them these were directed at 'lipoid' substances from the host tissue, which had been liberated through the action of the spirochaetes. This so-called 'autoantibody theory' challenged three immunological dogmas of the time: the idea of specificity, the 'protein dogma', and the idea of 'horror autotoxicus' (an organism will not produce antibodies against the substances of its own body).

There were also physico-chemical and 'colloidal' views on the nature of the WaR.[71] These resulted in new serodiagnostic tests for syphilis like the Meinicke and Sachs-Georgi reactions which did not employ complement fixation but were based on flocculation instead.

When Wassermann stated in December 1920, before the Berlin Medical Society, that it was his original intention to find "a diagnostically usable amboceptor", he was not necessarily suggesting, as Fleck appears to understand these words, that antibody detection rather than antigen detection had been his primary aim from the very beginning. Wassermann's formulation is much more subtle and evasive.[72] As the content of his lecture makes clear, he was not so much concerned with the practical execution of the WaR as with the elucidation of its nature. Wassermann referred to the general confusion with regard to his serodiagnostic reaction, saying that the various parties agreed on only one point: "one does not know, in which the WaR consists, what it is that one measures with it". On the basis of recent empirical investigations performed by himself, Wassermann believed that he was able to bring an end to the prevalent confusion, and also, incidentally - such at least is my reading

69 P. Mazumdar, 'The Antigen-Antibody Reaction and the Physics and Chemistry of Life', *Bulletin of the History of Medicine* 48 (1974): 1-21.

70 E. Weil and H. Braun, 'Ueber Antikörperbefunde bei Lues, Tabes und Paralyse', *Berliner klinische Wochenschrift* 49 (1907): 1570-74; E. Weil and H. Braun, 'Ueber das Wesen der luetischen Erkrankung auf Grund der neueren Untersuchungen', *Wiener klinische Wochenschrift* 22 (1909): 372-74.

71 P. Schmidt, 'Die Wassermann'sche Reaktion auf Syphilis - Eine Kolloidreaktion', *Zeitschrift für Chemie und Industrie der Kolloide ('Kolloid-Zeitschrift')* 10 (1912): 3-7.

72 After all, finding "a diagnostically usable amboceptor" can also be interpreted to mean finding "a specific antigen-antibody reaction", which in no way excludes its use for purposes of antigen detection.

of his pursuits -, to provide an interpretation of what had been discovered or invented which would enable him to defend his 'intellectual property' against foreign (especially French and Belgian) claims. This interpretation centered on the unique nature of the antibody, hence Wassermann's emphasis on "a diagnostically usable amboceptor".

In his lecture Wassermann explicitly referred to "the attempts to put *Bordet*'s name in the forefront with regard to this reaction [the WaR], which surfaced during the war years in France and Belgium".[73] It must have been still fresh in Wassermann's mind that the Belgian microbiologist Jules Bordet had received, in 1920, the Nobel Prize for Physiology or Medicine for the year 1919, for his discoveries relating to immunity. On several occasions Wassermann himself had also been a nominee for that prize. According to the French historian Claire Salomon-Bayet, who has examined the protocols of the Committee for Physiology or Medicine, "[the prize for 1919] might equally have been attributed to A. von Wassermann - as far as the Committee's protocol was concerned - had it been possible at the end of the War to recognize the pre-eminence of a German scientist at the same time as that of a Belgian scientist trained in Paris at the Pasteur Institute".[74] Wassermann's bitterness for the Nobel Prize that passed him by must have been exacerbated by the fact that the serodiagnostic reaction which in other countries bore exclusively his name came to be designated in France and Belgium (and also in Poland) as the *Bordet-Wassermann reaction.*[75] In defiance of Merton's norm of universalism, the institutional reward of *eponymy* is not completely free from nationalistic considerations.[76] Apparently, in those countries it was considered to involve only a small step to apply Bordet's complement fixation test to syphilis.[77] In view

73 Wassermann, op. cit. (note 58), p. 197.

74 C. Salomon-Bayet, 'Bacteriology and Nobel Prize Selections, 1901-1920', in C.G. Bernard, E. Crawford and P. Sörbom (eds.), *Science, Technology and Society in the Time of Alfred Nobel*, Oxford (Pergamon Press), 1982, pp. 377-400, p. 386.

75 Before the First World War, the serodiagnostic test for syphilis was simply designated as 'la réaction de Wassermann'. See E. Burnet, *Microbes et Toxines*, Paris (Flammarion), 1911, pp. 297-300.

76 Another example is Boyle's law, which in France is known as Mariotte's law. Merton has repeatedly drawn attention to the theme of 'national claims to priority' and the tension between 'scientific universalism' (in the assigning of credit) and 'ethnocentric particularism', particularly during times of international conflict. Then "[t]he man of science may be converted into a man of war - and act accordingly. Thus, in 1914 the manifesto of ninety-three German scientists and scholars - among them, Baeyer, Brentano, Ehrlich, Haber, Eduard Meyer, Ostwald, Planck, Schmoller, and Wassermann [NB! - HvdB] - unloosed a polemic in which German, French, and English men arrayed their political selves in the garb of scientists"; see Merton, op. cit. (note 4), p. 271. If Wassermann was among those who in 1914 placed German nationalism before scientific universalism, he was later repaid in kind by his Belgian and French colleagues.

77 In the collection *Cognition and Fact*, the French immunologist Bernard Zalc explains: "[I]n France and Belgium, this serological test is called the Bordet-Wassermann reaction to [emphasize] the fact that Wassermann's contribution to this serodiagnosis was to apply to syphilis the experimental procedure of complement deviation set-up a few years earlier by Bordet and Gengou", B. Zalc, op.

of this situation it is understandable that Wassermann had a strong motive to defend his 'intellectual property' against the claims of Bordet.

But how could Wassermann profile 'his' discovery against Bordet's method? After all, he and Bruck had indeed started from the latter's complement fixation test, albeit from a modification of it. But the modification, consisting in the use of so-called 'bacterial extracts' instead of suspensions of entire bacteria, was perhaps too slight to make much of a difference. Besides, the use of extracts from luetic organs was precisely the element in the original set-up which was later abandoned. So this would not make a strong case for the defence of Wassermann's intellectual property. He therefore took more recent views on the nature of the WaR as the basis for his defence. At the same time he was careful to construct a certain continuity with his original views.

In his lecture Wassermann reported about his recent inquiries into the nature of the WaR. In his opinion, the reaction between the serum antibody (which he now called the 'Wassermann substance') and the so-called 'antigen' (which he conceived of as a 'lipoid') consisted of the formation of a reversible complex or aggregate (which he designated as the 'Wassermann aggregate').[78] In his experiments Wassermann was able to precipitate the 'Wassermann aggregate', from which he could subsequently recover the two components. He further established that the Sachs-Georgi reaction was also based on the formation of the 'Wassermann aggregate'. This flocculation reaction therefore represented only a modification

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cit. (note 66), p. 399. As a real Frenchman, he sticks to this designation throughout. In Poland, too, the serodiagnostic test for syphilis was known as the Bordet-Wassermann reaction. Writing in Polish for a Polish audience, Fleck used the latter designation, while in his German monograph he complied with the attribution of eponymy current in German-speaking countries. It is claimed by the American medical science popularizer, Paul de Kruif, that before Wassermann and his co-workers, Jules Bordet had indeed attempted to apply the complement fixation test to syphilis. According to de Kruif, whose account is based on a personal conversation with Bordet in 1930, the Belgian microbiologist teetered on the edge of eternal fame because, super-careful as he was, he "made one test too many ...". He performed a control test with syphilitic serum and normal extracts. When hemolysis did not occur (as it theoretically should have done in the absence of prior complement fixation), Bordet concluded that the test could not be specific and dropped the whole business. The joke on Bordet was that normal extracts react with syphilitic sera *but not with normal sera*; they can therefore still be employed in a practically usable test in the place of syphilitic antigen. Wassermann and his co-workers eventually developed the serodiagnosis of syphilis because they were less careful than the super-careful Bordet. Or so de Kruif maintains. See P. de Kruif, *Men Against Death*, Hamburg/Paris/Bologna (The Albatross), 1934, pp. 204-22, esp. 219-20.

78 In a reply to his former assistant Carl Lange, who had poked fun at this terminology, Wassermann declared that these designations were chosen not for reasons of vanity but only because the name WaR was already in general use. See A. von Wassermann, 'Ueber die Antikörpernatur der Wassermannsubstanz', *Berliner klinische Wochenschrift* 58 (1921): 331-34. 'Self-eponymizing' is generally frowned upon in science, because it violates the value of humility. A hilarious exception that proves the rule is provided by the example of Stephen Stigler who formulated 'Stigler's Law of Eponymy'. (See S.M. Stigler, 'Stigler's Law of Eponymy', in T.F. Gieryn [ed.], *Science and Social Structure*, special issue of *Transactions of the New York Academy of Sciences*, series II, vol. 39, 1980, pp. 147-57). Stigler's Law of Eponymy exhibits self-exemplifying irony, because it states: "No scientific discovery is named after its original discoverer".

of the WaR. From the ability of the 'Wassermann aggregate' to fix or use up complement, Wassermann concluded that the 'Wassermann substance' possessed all the properties that are characteristic of amboceptors: "It is therefore a genuine amboceptor in Ehrlich's sense".[79] This is how Wassermann secured the continuity with his original views. It also explains why he stated, at the beginning of his lecture, that he was initially looking for "a diagnostically usable amboceptor" - the very words whose significance was misunderstood by Fleck.

But the 'Wassermann substance' was not just one antibody (or amboceptor) among many; it was a very special and unique kind of antibody: "with it for the first time an antibody against lipoids of human or animal organ cells was isolated".[80] And now Wassermann set out a theory on the origin of this special kind of antibody which closely resembled the 'autoantibody theory' that had been developed more than 10 years before by Weil and Braun, without even mentioning their names in this connection! Apparently, he could defend his intellectual property only by adorning his discovery with borrowed plumes.

Wassermann's determined effort to claim 'his' intellectual property by removing Bordet's name completely from the serodiagnosis of syphilis leaps to the eye in the following passages:

> "I have made use of an *instrument [Hilfsmittel]* constructed by Bordet [..], namely *complement fixation, to create something that was until then completely unknown.* To draw a comparison from chemistry, it is as if a chemist using Liebig's counter-current condensor found a fully novel chemical fact and one would therefore attribute the principal merit for this discovery to Liebig. [..]
> [..] the fact that it [the WaR] does not concern a reaction on parasites, but one directed at specifically affected host cells, would never have been found with Bordet's original experimental setup, but only with my setup using organ extracts. *Bordet's name has not the least to do with the reaction itself, which consists in the combination of the lipoid-amboceptor (Wassermann substance) and the lipoid-antigen into the reversible Wassermann aggregate.* Only in one single indicator for this phenomenon discovered and now also analyzed by me, has Bordet a part. If one chooses, for the recognition of this aggregate, a method different from complement fixation, then Bordet will be eliminated from the entire serodiagnosis of syphilis, - but not I, because without Wassermann lipoid-amboceptor and without Wassermann aggregate there is, until now, no serodiagnosis of syphilis, which is my intellectual product and property [*mein geistiges Produkt und Eigentum*] (emphasis in original)".[81]

As we can see, the use of a *modified version* of the complement fixation test plays only a subordinate part in Wassermann's argument (he wisely suppresses the fact that the 'organ extracts' were initially conceived as *bacterial* extracts); the full emphasis of the defence of 'his' intellectual property is on the presumed nature of the reaction. Having established that the Sachs-Georgi reaction involves the same 'Wassermann aggregate' as the WaR, he can

79 A. von Wassermann, op. cit. (note 58), p. 197.

80 Ibid.

81 A. von Wassermann, op. cit. (note 58), p. 197.

discount the complement fixation test as only one possible instrument or indicator, besides flocculation, for the recognition of this 'aggregate'. Therefore, Bordet can be eliminated from the "entire serodiagnosis of syphilis" (incidentally, Wassermann is simultaneously extending his property to cover also the Sachs-Georgi and Meinicke flocculation reactions!).

Wassermann's defence of 'his' intellectual property against the French and Belgian "attempts to put Bordet's name in the forefront" elicited in its turn strong responses from his fellow-countrymen Carl Lange, Eduard Weil, and Carl Bruck.[82]

Carl Lange, Wassermann's former assistant (who had however not been directly involved in the early stages of the development of the WaR), questioned the latter's claim that his recent empirical investigations had brought a definitive solution to the problem of the nature of the WaR. Lange could not discern anything new or remarkable in Wassermann's experiments. At any rate, they were insufficient to establish the conclusion that the 'Wassermann substance' really was an *antibody* against lipoids. The same substance was also able to react with colloidal gold or with mastix, and no one would infer from *these* facts that the substance represented an antibody.[83] In his rejoinder Wassermann contested the legitimacy of physico-chemical analogies for elucidating the nature of biological phenomena. He was also ruffled by the ironic tone of Lange's reaction: "I do not find this form very tasteful vis-à-vis a man who has created the whole field of the serodiagnosis of syphilis [..], in particular on the part of an author who for many years was allowed to work in my institute".[84]

In his reaction to Wassermann's lecture, Eduard Weil used the opportunity to pay off some old scores: "Only reluctantly do we dig up the old bone of contention, which we buried 11 years ago, because we were confronted, at that time, with an organization which did not treat scientific issues in the usual manner and against which we felt too weak".[85] He exposed the many distortions, misrepresentations and equivocations in Wassermann's historical account of the development of the WaR ("W. now spreads a thick veil over the actual course of events"). Two examples from Weil's reply. Wassermann was not just looking for "a diagnostically usable amboceptor", he was looking for a specific amboceptor against the syphilis spirochaete. Wassermann had also concealed from his audience that he himself had given up the antibody status of his substance after the 'antigen' proved soluble in alcohol. It is no disgrace to change one's views in a dynamic field of science, Weil remarked, "but

82 Bordet himself did not respond to Wassermann's lecture, at least not in the pages of the *Berliner klinische Wochenschrift*.

83 C. Lange, 'Entgegnung auf A. v. Wassermann's modifizierte Lipoidhypothese', *Berliner klinische Wochenschrift* 58 (1921): 330-31.

84 A. von Wassermann, 'Ueber die Antikörpernatur der Wassermann-substanz: Zugleich eine Richtigstellung der von Lange in dieser Wochenschrift veröffentlichten Entgegnung', *Berliner klinische Wochenschrift* 58 (1921): 331-34, p. 334.

85 E. Weil, 'Das Problem der Serologie der Lues in der Darstellung Wassermann's', *Berliner klinische Wochenschrift* 58 (1921): 966-70, pp. 966-67.

we have no appreciation for the fact that one conceals previously expressed views, once they have been refuted, or even goes so far as to disavow them completely".[86]

Weil also broached the issue of the part played by chance in the discovery of the WaR. In his lecture Wassermann had opposed a widely held view about his discovery which he paraphrased as follows: "From that time on [after the introduction of 'alcholic extracts'] the discovery was depicted in the literature more or less as if I had simply applied Bordet's method of complement fixation [..] to the field of syphilis, that the assumption from which I proceeded was false, but that I found something right [*etwas Richtiges*] by accident".[87] According to Weil, this would indeed give a truthful account of the development of the WaR, if one substituted 'a discovery of great practical importance' for 'something right' (cf. Fleck: 74-75/99).

In his rejoinder, Wassermann stated that he was ready to fulfil all of Weil's wishes as far as he was able to, but he did not answer any of Weil's concrete accusations of misrepresentation. He confessed "frankly" that he had often changed his theoretical views on the nature of the WaR. One possible wish of Weil could not be satisfied: "If [..] it were finally Weil's wish to show to the world that I, after all the false assumptions that I made, should not actually deserve to be the father of such an important discovery as the serodiagnosis of syphilis, then I cannot fully satisfy his wishes on this score, because for that he has to polemicize with fortune [*dem Schicksal*] and not with me".[88] Wassermann was of course referring to the eponymous fact that his name had become solidly associated with the serodiagnosis of syphilis.

In his first reaction to Wassermann's lecture, which initiated a series of increasingly acrimonious exchanges with his former principal, Carl Bruck also alluded to the vicissitudes of eponymy. According to Bruck, it was all very well that Wassermann opposed French and Belgian attempts to put Bordet's name in the forefront, but he should not have claimed the serodiagnosis of syphilis as his exclusive "intellectual product and property". Induced by this "egocentric" stand taken by Wassermann, Bruck felt in his turn obliged to defend his share, and that of the late Albert Neisser (who had died in 1916), in the development of the WaR. He thought he also owed it to Neisser's memory to reaffirm and recall that "the socalled Wassermann reaction" was "the result of the *joint* labour [*gemeinsamen Arbeit*] of A. v. Wassermann, A. Neisser, and C. Bruck". It was Neisser, according to Bruck, who by visiting Wassermann in Berlin in early 1906, had given the external stimulus [*äussere Veranlassung*] to initiate the serological investigations on syphilis and supported the endeavour through the supply of pathological material from his clinic. Bruck himself had been in charge

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86 Ibid., p. 970.

87 A. von Wassermann, op. cit. (note 58), p. 194.

88 A. von Wassermann, 'Bemerkungen zu den Ausführungen E. Weils', *Berliner klinische Wochenschrift* 58 (1921): 970.

of the practical execution of this task. After "a few months of solitary and strenuous work" had yielded positive results, it was Bruck who actually wrote the first article on the new serodiagnostic reaction (which was headed by the names of Wassermann, Neisser, and Bruck). Before he was allowed to leave Wassermann's department at the Institute for Infectious Diseases in Berlin to move to Breslau, he had to dictate all the technical details of the test (including its several controls) to Wassermann.

Bruck recalled that in the early period the serodiagnostic reaction was usually designated as the *Wassermann-Neisser-Bruck reaction* (or, abbreviated, *WNB-R*): "In the following years, however, the WNB-R gradually became a *Wassermann* reaction, as a consequence of considerations of convenience [*eine Bequemlichkeitserscheinung*] [..]".[89] Although it was customary to honour all the authors involved in serological discoveries (Bruck mentioned the Gruber-Widal, Sachs-Georgi and Weil-Felix reactions), for more than 10 years Bruck had not protested against the designation 'Wassermann reaction' out of "piety vis-à-vis my respected former teacher", who himself was not responsible for this eponymy. It was only Wassermann's recent usurpation of the entire intellectual property of the WaR, Bruck declared, that had induced him to reveal the historical truth about its actual genesis.

In his first rejoinder to Bruck, Wassermann reaffirmed his right to call himself the "intellectual father" of the serodiagnosis of syphilis, which indeed constituted his "exclusive intellectual product and property" [*alleiniges geistiges Produkt und Eigentum*]. He quoted from his earlier correspondence with Neisser to support his claim. In a letter dated 1 August 1906 (after the ways of both had already parted), Neisser had written to Wassermann: "But finally, dear friend, why don't you grant *us here* the satisfaction to develop the whole thing [*die Sache auszuarbeiten*], *after you have already acquired the honour, which is rightfully yours, to have inaugurated the entire procedure*". In the same letter Neisser had conceded to Wassermann, as "the one *whom we owe the decisive step in this matter*" (emphasized by Neisser himself), "a very special right to pursue these questions".[90] It is difficult to decide to what extent Neisser's statements support Wassermann's claim, for they may partly reflect conventionalized attributions. They are also interesting from the point of view of Ravetz's "principle of fairness in exploitation of results" [91], which Merton failed to incorporate in his description of the scientific ethos.

In his second reply, Bruck denied that he wanted to contest that it was Wassermann who had given the instigation [*Anregung*] to apply the complement fixation test to syphilis: "But it is a long way between the instigation and the working out of an idea, and I may claim the

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89 C. Bruck, 'Ueber die Entwicklung der Syphilisserodiagnose', *Berliner klinische Wochenschrift* 58 (1921): 464-67, p. 464.

90 Cited in A. von Wassermann, 'Zur Geschichte der Serodiagnostik der Syphilis', *Berliner klinische Wochenschrift* 58 (1921): 467.

91 See note 21.

merit for having executed the latter all by myself".[92] Now Bruck also used a different argument to counter Wassermann's tenacious insistence on his intellectual paternity of the WaR: "The *idea*, on which the serodiagnosis of syphilis was grounded and of which Wassermann so emphatically claims to be the intellectual father, has turned out to be *erroneous*. Through an *extraordinary stroke of luck*, I have discovered, during the practical execution of Wassermann's idea, a syphilis reaction, the nature of which is still not clear today".[93] If the principal merit for this discovery should be due to anybody or anything, Bruck concluded, then it should accrue to this stroke of luck. A similar view had also been expressed by Weil. For Fleck the appeal to chance is "of no scientific value" (76/102). The category of 'lucky accident' has no legitimate place in his sociological epistemology. In a moment I will return to this problem to see whether the factor of chance has been successfully eliminated by Fleck.

In his second rejoinder to Bruck, Wassermann dismissed the former's account of the genesis of the WaR as a fairy-tale from the Thousand and One Nights: "I casually threw out the idea that one might create a serodiagnosis of syphilis, then went out for a walk without caring further about the matter, had my poor young assistant exert himself to the utmost until one day he surprised me with the happy news: the serodiagnosis of syphilis has been discovered".[94] Wassermann went at great length to show that such idyllic conditions did not obtain in his department of Koch's institute. The method for the serodiagnosis of syphilis, he maintained, did not have to be elaborated by Bruck; it was already available in 1905 in the form of the modification of the complement fixation test involving the use of extracts obtained through agitation [*Schüttelextrakte*]. This modification was Wassermann's 'intellectual property', of which he could therefore dispose as he saw fit. He thus charged his collaborators with applying it to typhoid, or to meningitis. It was Bruck's "extraordinary stroke of luck" that he, and not one of his colleagues, had been assigned the job to apply the method to syphilis, a stroke of luck "which would become decisive in such a happy manner for Bruck's later career". Wassermann declared that he had never detracted from Bruck's merits as a collaborator in the development of the WaR. "But I say emphatically 'collaborator' [*Mitarbeiter*] [..] and not 'co-creator' [*Mitschöpfer*]. The serodiagnosis has been *created* [*geschaffen*] [..] exclusively by me". It was improper for Bruck, Wassermann stated, to try to reverse the roles between teacher and pupil. He should not have repaid good with evil.

It is notable that Wassermann uses the *hierarchical relationship* as it existed between him and Bruck to bolster his claims to the intellectual property of the WaR. Following his

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92 C. Bruck, 'Zur Geschichte der Serodiagnose der Syphilis', *Berliner klinische Wochenschrift* 58 (1921): 580-81, p. 581.

93 Ibid. (compare Fleck: 74/98).

94 A. von Wassermann, 'Zur Geschichte der Serodiagnostik der Syphilis', *Berliner klinische Wochenschrift* 58 (1921): 888-90, p. 888.

account, there seems to be no difference between disposing of one's intellectual property (over the modified complement fixation method, in this case) and assigning tasks to one's subordinates or *'Mitarbeiter'*. In such a hierarchical world it appears to be excluded by definition that a *Mitarbeiter* can ever become a *Mitschöpfer*.[95] It is also notable that Wassermann now defends his claim as to the intellectual property of the WaR exclusively on the basis of his modification of the complement fixation test, whereas he had earlier emphasized the special nature of the reaction against French and Belgian attempts "to put Bordet's name in the forefront". Wassermann did not have a consistent account that could be used in both contexts.

In his third and last reaction, Bruck contested that Wassermann's prior work on *'Schüttelextrakte'* should entitle him to call himself the 'creator' (*Schöpfer*) of the WaR: "For the socalled WaR is not a 'creation' (*Schöpfung*) at all, but a pure chance discovery, which, as we know today, has nothing whatsoever to do with bacterial extracts, but is based on the fact that certain bodily substances, which are also present in normal organs, happen to react with luetic sera in a characteristic way, and that this reaction by chance also happens to be demonstrable with complement fixation!".[96] Thus, against Wassermann's claim to have 'created' the WaR, Bruck reiterated his view that the principal merit for the discovery accrued to a stroke of luck.

In his final rejoinder, Wassermann observed that every important discovery in the past has had its critics who prefer to attribute the merit to luck and chance rather than to the discoverer. Chance may play a certain part, but, according to Wassermann, "in such lucky accidents Nature reveals herself only to him who uses a new method or makes an observation of which he recognizes the significance".[97] He was of course echoing Pasteur's famous remark that in science chance favours the prepared mind. Wassermann claimed to have created the method through which Nature would yield the secret of the luetic serum. This definitively settled, in his view, the question of the intellectual property of the serodiagnosis of syphilis.

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95 There is an interesting parallel with the Hewish-Bell conflict over the discovery of pulsars (see note 55). In response to Fred Hoyle's letter crediting Jocelyn Bell with the discovery, "Nobel laureate Hewish wrote *The Times*, saying in effect that Bell had been using his telescope, under his instructions, to make a sky survey which he had initiated. The possibility that the accidentally discovered pulsars were of human or alien origin was resolved under his direction", Broad and Wade, op. cit. (note 55), p. 148. Hewish also suggested that the next graduate student down the line would have made the discovery. "'Jocelyn was a jolly good girl but she was doing her job', says the Nobel laureate. 'She noticed this source was doing this thing. If she hadn't noticed it, it would have been negligent'." (Ibid., p. 148). Likewise, Wassermann suggested that he could have chosen any of Bruck's colleagues to have the job done.

96 C. Bruck, 'Zur Geschichte der Serodiagnostik der Syphilis', *Berliner klinische Wochenschrift* 58 (1921): 1194.

97 A. von Wassermann, 'Zur Geschichte der Serodiagnostik der Syphilis (Bemerkungen zu den Bruck'schen Ausführungen)', *Berliner klinische Wochenschrift* 58 (1921): 1194-95, p. 1195.

This series of acrimonious exchanges between Wassermann and Bruck was concluded with an editorial statement from the *Berliner klinische Wochenschrift*: "Therewith we consider this unsavoury [*unerquickliche*] discussion to be closed and do not want to withhold our judgement, in view of the tenacity with which Mr. Bruck suddenly believes to have to insist on property rights, that raising a priority claim 15 years after the discovery of the method concerned and 15 years of tacit agreement must make an impression which we now hope to see erased very soon - Ed. H.K. [= Hans Kohn]".[98]

From a Mertonian perspective it is significant that in this editorial comment the dispute is called an "unsavoury discussion". We can also see the editor actively exerting himself to define this struggle over intellectual property as an aberrant phenomenon by expressing his hope that the bad impression caused by Bruck might soon become obliterated. It is also remarkable that Bruck is accused of raising a 'priority claim': apparantly, priority disputes so much constitute the standard case of conflicts over intellectual property, that a deviant instance is also interpreted in its terms. Finally, the editor's *parti pris* in favour of Wassermann is very conspicuous, but this has probably much to do with the latter's high standing in the medical and scientific world of his time.

Let me finally turn to the question of the acceptability of the notion of 'chance' or 'lucky accident' in the epistemological analysis of discoveries and inventions. We have seen that Fleck rejects such notions as 'unscientific'. His sociological epistemology appears to assume a standpoint above that of each of the contending parties, by 'impartially' criticizing the views of both: "Some among the eyewitnesses talk about a lucky accident, and the well-disposed about the intuition of a genius" (76/102).

Fleck maintains that the recourse to chance is connected with an individualistic standpoint; by taking a "social point of view" it can be avoided. He uses a suggestive simile to convey his view: "It is an accident when a stone drops into a hole. But it is inevitable that dust should penetrate pores; it is blown about in the environment until it finally enters, but each individual particle comes to rest in its particular position only by accident" (78/104).

But we remember that Weil and Bruck invoked the notion of chance to account for the fact that something very useful eventually emerged from false initial assumptions. Can this 'miracle' be resolved by taking a social point of view, by considering the WaR as the outcome of some sort of collective effort? Fleck appears to give an affirmative answer to this question by presenting another comparison: "How does it come about that all rivers finally reach the sea, in spite of perhaps initially flowing in the wrong direction, taking roundabout ways, and generally meandering? There is no such thing as the *sea as such*. The area at the lowest level, the area where the waters actually collect, is merely *called* the sea!" (78/104). This comparison suggests that, given enough collective effort expended on finding a sero-reaction for the detection of syphilis, whatever comes out of it will be practically usable for

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98 *Berliner klinische Wochenschrift* 58 (1921): 1195.

this purpose, simply because it will be *defined* as such! (Under such conditions false initial assumptions are of course without consequence.) But this appears to be a *reductio ad absurdum* of Fleck's position. It is not amazing that he retracts from this position later on, for then he remarks that the WaR cannot be reconstructed "in its objective entirety" from historical and social factors (79/105). It thus appears that Fleck has not really eliminated 'chance' from his sociological epistemology. His criticism of the participants involved in the dispute over the WaR who used this notion is not justified.

4. Summary and conclusions

The argument of this chapter has been conducted on two fronts. I have tried to play off Merton's approach in the sociology of science against Fleck's treatment of the struggle over the intellectual ownership of the WaR in order to reveal the shortcomings of the latter's 'collectivism'. Simultaneously, however, I have attempted to show that the Mertonian paradigm itself needs extension and revision if it is to measure up to this challenge. An important lesson to be learnt from Merton is that priority disputes - and conflicts over intellectual property generally - are recurrent and interesting phenomena which deserve to be treated as a proper object of sociological analysis rather than being condemned as utterly futile or totally misconceived. The latter tack had been chosen by Fleck. Paradoxically, the 'collectivistic' bent of his approach had led him to polemicize against the participants involved in the conflict over the WaR rather than to analyze the salient features of the dispute.

The analysis of the struggle over the intellectual ownership of the WaR represents a foray into hitherto uncharted territory of the Mertonian paradigm. Until now Merton and his students have studied conflicts over intellectual property (in the customary form of priority disputes) between independent researchers involved in multiple discoveries and the differential allocation of prestige between such researchers and between the authors of multi-authored articles (the operation of the so-called Matthew effect), but not intellectual property battles between the members of a research team. The filling of this gap is of strategic value because some of Merton's critics, e.g. Barnes and Dolby, have suggested that the rise of team research in this century did not create the tensions that might have been expected on the basis of Merton's theory. My view is that closer examination of the historical record will probably bring to light more instances of tensions and conflicts than the critics allow. The least we can say is that on this issue the jury is still out.

Mertonianism and constructivism remain deeply divided over the efficacy of social norms and values (such as those comprising the 'scientific ethos') in regulating scientists' behaviour. Despite my attempts at integration and reconciliation, I have been unable to resolve *this* fundamental issue.

In exploring the white spot of the Mertonian paradigm and performing an in-depth analysis of the struggle over the intellectual ownership of the WaR, I have found it necessary to drop Merton's stricture on treating the content of science. Here I agree with Merton's 'post-Kuhnian' critics. In the case of 'team' discoveries too, the question of *who* has taken a creative part in the achievement is inextricably intertwined with the substantive question of *what* exactly has been discovered. Participants can argue their case only by taking a stand on both questions.

Fleck did pay attention to the views expressed by several protagonists embroiled in the quarrel over the WaR, but, as he said, "only for epistemological purposes". He attempted to show that their views exhibited characteristic retrospective distortions which reflected the nature of the collective process of gathering experience. Fleck cited the example of August Wassermann, who in his lecture of December 1920 could not but project the final outcome of the process back into the initial stages. Fleck maintained that new knowledge, acquired through collective experience, had a retroactive effect in that it made the participants lose the appreciation for their own previously held views. In offering this explanation Fleck lost sight of the strategic nature of the exchanges between the participants: each sought to press his own claims and combat those of others in a contest in which the intellectual property of the serological test for syphilis was at stake. Wassermann and, to a lesser extent, the other participants were thus made out as 'innocent' victims of the collective process of gaining experience.

In presenting an alternative interpretation of the exchanges of views between Wassermann and his opponents in the polemic of 1920-1921 I have taken the strategic character of the dispute more seriously into account. The opening salvo in the series of exchanges, Wassermann's lecture of December 1920, can be seen as a powerful reaffirmation and defence of the serodiagnosis of syphilis as 'his' intellectual property against the attempts in France and Belgium during the First World War to put Bordet's name in the forefront (in those two countries the serological test had been re-labelled 'the Bordet-Wassermann reaction'). To defend 'his' property, Wassermann had to 'profile' the seroreaction for syphilis vis-à-vis Bordet's complement fixation method. The first problem for Wassermann was that his initial views on the nature of the reaction were unsuitable for that purpose. He therefore took recourse to the so-called 'autoantibody theory', an interpretation that had originally been propounded by his former critic Eduard Weil, to underline the very special and unique properties of the serological reaction. All the while he carefully constructed a continuity with his former views by stating that it had been his original intention to find a "a diagnostically usable amboceptor" (which Fleck misread as stating that antibody detection rather than antigen detection had been his intention from the outset). We have seen that Wassermann's appropriation of the full intellectual ownership of the WaR aroused indignant reactions from, amongst others, his former critic Weil and his former collaborator Bruck. The second difficulty for Wassermann was that the substantive account of the reaction which he had employed to counter Bordet's claims could not be used against Weil and Bruck. With regard to Weil his ultimate 'argument' was *das Schicksal* or 'fortune' (the eponymous fact that in Germany and Central Europe his name had become solidly associated with the reaction).

Against his erstwhile collaborator Wassermann appealed to the hierarchical relations formerly existing between them which by definition excluded that a 'co-worker' could ever become a 'co-creator'. After a series of increasingly acrimonious exchanges, the editor of the *Berliner klinische Wochenschrift* sided with Wassermann and closed the "unsavoury discussion".

In their replies to Wassermann, Weil as well as Bruck appealed to the factor of chance (or an "extraordinary stroke of luck") to account for the fact that something very useful eventually emerged from false initial assumptions. In my view such an appeal to chance is quite acceptable and appropriate given the nature of the case, but Fleck found it unscientific to invoke this notion. He thought it possible to eliminate the chance factor by adopting a 'social' point of view and considering the WaR as the outcome of a collective effort. I have argued that this elimination is only apparent, unless one is willing to endorse the absurd conclusion that any outcome of a collective effort will be *ipso facto* useful.

Fleck's rejection of chance as a legitimate factor in the development of science betrays the collectivistic and deterministic tenets of his sociological theory of knowledge. Modern constructivists do not generally share this attitude. A keen awareness of the role of contingency and chance in human affairs may be a useful asset to question existing definitions of reality rather than to take them for granted. After all, constructivism lives from the realization that things are not necessarily the way they are but might have been different.

CHAPTER VII

FROM METHYLENE BLUE TO SALVARSAN: TEST ANIMALS, HUMAN SUBJECTS, AND CLINICAL TRIALS

1. Introduction

In a review of *Genesis and Development of a Scientific Fact*, the Dutch philosopher Gerard de Vries encapsulated the thrust of Fleck's programme of a sociologically-based comparative epistemology in a concise formulation: "Epistemology is the epidemiology of intellectual contact".[1] This is indeed an apt characterization of Fleck's view, for the upshot of his 'doctrine of thought styles and thought collectives' is precisely that scientific facts are the outcome of intensive social interaction between the members of a thought collective, defined as "a community of persons mutually exchanging ideas or maintaining intellectual interaction" (39/54-55).[2]

At times, however, Fleck hints that this conception of 'intellectual interaction' with its one-sided 'intellectualistic' emphasis on the exchange of *ideas* may be too restrictive. Summarizing the course of events leading up to a practically usable Wassermann test, he writes: "Skills, experience in the field, and ideas whether 'wrong' or 'right' passed from hand to hand and from brain to brain" (69/91). Interaction within a thought collective is not limited, therefore, to the exchange of ideas passing 'from brain to brain' but also includes the exchange of skills and experience passing 'from hand to hand'. (A similar point is made by Karin Knorr-Cetina in a somewhat different context, when she notes that "'conceptual interaction' is not merely 'conceptual'."[3]) One might even go further and also include the exchange of material resources like samples, raw materials, specimens, research objects, test animals, tools and instruments. This proposed extension finds some justification in Fleck's own text. Using the same example of the formation of the Wassermann test, Fleck notes that Albert Neisser's contribution was that he "[..] offered the pathological material" in addition to his experience as a physician (69/91). This 'pathological material' was derived from the syphilitic patients of Neisser's dermatological clinic in Breslau and also from his artificially infected apes and monkeys.

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1 G. de Vries, 'De besmettelijkheid van intellectueel contact', *Kennis en methode* 5(1981): 156-64, p. 162.

2 Compare David Gooding: "Natural knowledge is produced by people interacting sometimes with Nature and always with each other", D. Gooding, '"Magnetic curves" and the magnetic field: experimentation and representation in the history of a theory', in D. Gooding, T. Pinch, S. Schaffer (eds.), *The Uses of Experiment*, Cambridge (Cambridge University Press), 1989, p. 183.

3 K.D. Knorr-Cetina, *The Manufacture of Knowledge*, Oxford (Pergamon Press), 1981, p. 60.

Elaborating on Fleck, Gerard de Vries proposes to analyze the structure of thought collectives in terms of the relationships and dependencies obtaining between their members.

In academic areas, de Vries holds, researchers are dependent on other investigators in several respects, e.g. with regard to the supply of *ideas*; the provision of 'objects of research' or *materials*; the offering of *criticism*; and, especially nowadays, the *assessment of research proposals* in connection with funding decisions.[4] The problem with de Vries's proposal is that he still follows Fleck in delineating thought collectives primarily along disciplinary lines. Fleck designated the collective that realized the Wassermann reaction as the '*serological* thought-collective', although it transpires from his own historical description that non-serologists like Albert Neisser were also prominently involved. On the basis of her laboratory studies Karin Knorr-Cetina has criticized the strong fixation in science studies on the disciplinarily defined 'scientific community' as the social framework supposed to be relevant for the action of scientists. She offers the notion of *transepistemic* or *transscientific fields* to emphasize that laboratory scientists are entangled in a web of social relationships which extend beyond the boundaries of a scientific community. In her view, "the social integration [..] is based not upon what is shared [e.g. a common disciplinary background - HvdB], but upon what is *transmitted* between agents".[5] She speaks of *resource-relationships* between agents, a notion that also captures the types of dependencies distinguished by de Vries. From all this it is clear that Fleck's phrase 'exchange of ideas' (*Gedankenaustausch*) is not nearly comprehensive enough to cover the various mutual dependencies and interactions making up the epidemiology of scientific contagion.

One might reasonably argue that a broadening of the framework of analysis from an exclusive concern with *intellectual* interaction to the inclusion of the exchange of skills, experience, and material resources does not go far enough. Such an analysis is still focused on the sphere of *circulation* or exchange; a full-blooded constructivist approach also needs to attend to the sphere of *production*, in other words it will have to elaborate a view of how scientific results are being 'constructed', 'fabricated', 'manufactured', or 'produced', and it also has to investigate how the spheres of production and circulation are linked to each other. Fleck has some rudimentary insights to offer on the 'production' of scientific knowledge (his 'epistemology' therefore cannot be exhaustively characterized as an 'epidemiology'), but in recent years this subject has been more fully and consistently elaborated by Karin Knorr-Cetina in the tradition of ethnographic laboratory studies, and by Andrew Pickering, David Gooding and others who attempt to move beyond the sociology of scientific *knowledge* (SSK)

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4 G. de Vries, 'De ontwikkeling van wetenschappelijke kennis, sociologisch beschouwd', *Kennis en methode* 6(1982): 190-220, p. 212. De Vries remarks that for *non-academic* areas this list should probably be extended.

5 K.D. Knorr-Cetina, 'The Ethnographic Study of Scientific Work: Towards a Constructivist Interpretation of Science', in K.D. Knorr-Cetina and M. Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*, London/Beverly Hills (Sage), 1983, pp. 115-140, p. 133.

towards a detailed and systematic analysis of experimental *practice.*[6] Andrew Pickering, a principal exponent of this 'turn to practice' (or 'turn to experiment'), summarizes the new view of the production of scientific knowledge as follows:

> "Like any other kind of production, the production of knowledge requires resources: material resources such as money, manpower and machines; and conceptual resources such as ideas about how the hardware works, rules of thumb, theories, models and mathematical techniques. Expertise in the practical use of these resources is also needed, so expertise is itself a resource. [..] It makes sense, then, to think of the constructive practice of individual scientists as being *structured* by their positions within a field of resources".[7]

Probably because of his preoccupation with elementary particle physics, Pickering recognizes the importance of machines but fails to mention such things as raw materials or experimental animals among the material resources. These should be explicitly included when the biomedical sciences, as in the present case, are the focus of study.[8] An important issue in contemporary science studies is whether they can do justice to the role of material resources in scientific practice without abandoning their constructivist tenets, that is, without falling back into the position of naive realism.

Pickering conceives the constructive practice of scientists as a dialectic between resistance and accommodation. In the course of their daily pursuits scientific investigators will come up against all kinds of 'resistances' or unexpected problems, to which they can accomodate not just by revising the theory under consideration, but also by revising the interpretation of the working of the experimental apparatus or, more likely, by adjusting their instruments or changing their material procedures. They seek to achieve coherence between these different elements. In Pickering's view, scientific practice is shot through with chanciness and con-

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6 This trend is exemplified in two recent collections: D. Gooding, T. Pinch, S. Schaffer (eds.), *The Uses of Experiment*, Cambridge (Cambridge University Press), 1989; A. Pickering (ed.), *Science as Practice and Culture*, Chicago and London (The University of Chicago Press), 1992. See also I. Hacking, 'Philosophers of Experiment', in A. Fine and J. Leplin (eds.), *PSA 1980*, Vol. II (East Lansing: Philosophy of Science Association, 1989), pp. 147-56. Fleck's epistemological position has been compared to the 'pragmatic realism' advocated by Pickering and Gooding in J. Golinski, 'The Theory of Practice and the Practice of Theory: Sociological Approaches in the History of Science', *Isis* 81(1990): 492-505.

7 A. Pickering, 'Forms of Life: Science, Contingency and Harry Collins', *British Journal for the History of Science* 20 (1987): 213-21, p. 220.

8 Karin Knorr-Cetina also speaks of "a traffic of substances, materials and equipment" and of "an exchange of specimens, tools, and materials" within and between laboratories. See K. Knorr Cetina, 'The Couch, the Cathedral, and the Laboratory: On the Relationship betwen Experiment and Laboratory in Science', in A. Pickering (ed.), op. cit. (note 6), pp. 113-38, p. 128. For a stimulating analysis of the relevance of research materials for the conduct of scientific investigation, see A.E. Clarke, 'Research Materials and Reproductive Science in the United States, 1910-1940', in S.L. Star (ed.), *Ecologies of Knowledge: Work and Politics in Science and Technology*, Albany (State University of New York Press), 1995, pp. 183-225.

tingency. 'Resistances' can only emerge in a particular practice, relative to the goals, interpretations and material procedures of that practice. Successful accommodations to these resistances are similarly contingent: it is impossible to know in advance which revisions in material procedures or interpretations will restore coherence. Pickering also uses the term 'tuning' - "in the sense of tuning a radio set or car engine"[9] - for this accommodative work, which is clearly reminiscent of the way Fleck describes how Wassermann and his co-workers fiddled with the serodiagnostic test for syphilis to achieve a better quantitative match between the components of the reaction, thus enhancing the success rate of the test in cases of confirmed syphilis from barely 20 percent to 70-90 percent: "[..] from these confused notes [i.e. the unpromising initial results - HvdB] Wassermann heard the tune that hummed in his mind but was not audible to those not involved. He and his co-workers listened and 'tuned' their 'sets' until these became selective. The melody could then be heard even by unbiased persons who were not involved" (86/113). Pickering hastens to add the caveat that in scientific research - unlike with tuning a radio set - the character of the signal is not known in advance.[10] The use of the term 'tuning' can be further stretched and extended. Experimental practice involves a complex interplay of interdependent revisions of material procedures, interpretations and theories, in other words of material and conceptual 'tunings'. These may be accompanied by a concomitant 'tuning' and refiguring of social entities and relationships, involving work-styles, institutional structures, and more generally 'forms of life'.[11]

Even before Pickering turned from the sociology of scientific knowledge to the study of 'science-as-practice', Knorr-Cetina had already adopted in her ethnographic laboratory studies a constructivist approach which considers the products of science quite literally as the result of a process of fabrication.[12] A striking fact for the ethnographic observer visiting the laboratory was that in this particular setting natural scientists were nowhere confronted with 'Nature'. The raw materials with which scientists work are already preconstructed; laboratory mice and rats are specially bred[13]; substances and chemicals are obtained in

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9 A. Pickering, 'The Mangle of Practice: Agency and Emergence in the Sociology of Science', *American Journal of Sociology* 99 (1993): 550-89, p. 564; Pickering himself notes the resemblance with Fleck's use of the term 'tuning'. See also A. Pickering, *The Mangle of Practice: Time, Agency & Science*, Chicago and London (The University of Chicago Press), 1995, p. 22, note 35.

10 Ibid.

11 A. Pickering, 'Knowledge, Practice and Mere Construction', *Social Studies of Science* 20 (1990): 682-729, p. 707.

12 Knorr-Cetina, op. cit. (note 3); Knorr-Cetina, op. cit. (note 5).

13 Klaus Amann argues that laboratory mice have been so drastically transformed into 'epistemic objects' that the common name 'mice' which they share with the wild and domestic mice of our lifeworld is in fact highly misleading; actually they are a completely different kind of animal. See K. Amann, 'Menschen, Mäuse und Fliegen', *Zeitschrift für Soziologie* 23 (1994): 22-40, esp. 29-31.

purified form from industry or other labs; the water which runs from a special faucet is sterilized; in short, the laboratory is a locus of action from which 'nature' appears to have been excluded as much as possible.[14] Laboratories, according to a more recent formulation, can be seen as "made up of scientific *reconfigurations* of the natural in relation to the social order from which epistemic benefit can be reaped".[15] Most constructivist analysts, Knorr-Cetina holds, conceive of construction in terms of the moves and activities and negotiations of indiviuals. In her opinion, however, construction today is increasingly the work of *machineries of construction*, a fact that is easily overlooked in historically oriented studies of science with an individualist bias.

Against the background of the analytical concerns sketched above it is now possible to formulate the subject matter of this chapter. Its primary aim is to describe and analyze the intellectual, social, and material 'conditions of possibility' of Paul Ehrlich's laboratory practice of experimental therapeutics (or 'chemotherapy'), which in 1909 resulted in the discovery of *salvarsan* as an effective drug against syphilis. At first sight it might appear that this spectacular discovery, which is often depicted as the crowning achievement in the career of a scientific genius, is not amenable to a sociological analysis along constructivist lines. The objection could be made that Ehrlich's genius raises him above the 'epidemiology of intellectual contact'. Much more than itself having been influenced by the intellectual currents of his time, his independent fertile mind appears to have moulded the ways his contemporaries thought about immunological and other biological phenomena. Leonor Michaelis has even asserted that only in one respect was Ehrlich a product of his time, i.e. as being an 'apolitical animal', but that in all other respects "it would be more correct to consider Ehrlich's time as a product of his person".[16] When Ehrlich received the Nobel Prize in 1908 for his previous work on immunology, his one-time collaborator Wassermann declared that without the former's socalled *side-chain theory* the serodiagnostic test for syphilis would never have been found.[17] Ehrlich's inordinate influence around the turn of the century is also recognized by Fleck, when he observes that the notion of specificity, exemplified by Ehrlich's wellknown lock-and-key symbols, dominated the very depths of the science of serology (117/155). Given this *prima facie* independence of Ehrlich's thought from external influences, the attempt to understand his scientific pursuits as part and parcel of the 'epidemiology of intellectual contact' is not without interest. Ehrlich's achievement may indeed

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14 The above is a paraphrase of a passage from Knorr-Cetina, op. cit. (note 5), p. 119.

15 K. D. Knorr-Cetina, 'Strong Constructivism - from a Sociologist's Point of View: A Personal Addendum to Sismondo's Paper', *Social Studies of Science* 23 (1993); 555-63, p. 560.

16 L. Michaelis, 'Zur Erinnerung an Paul Ehrlich: Seine wiedergefundene Doktor-Dissertation', *Die Naturwissenschaften* 7(1919): 165-68, p. 165.

17 A. Wassermann, 'Paul Ehrlich', *Münchener medizinische Wochenschrift* 56 (1909): 245-47.

present a 'hard case' for Fleck's programme of comparative epistemology. We will see if this case can be accomodated. But this does not exhaust our interest in Ehrlich's work.

Section 2 discusses the trajectory of Ehrlich's career from his early student years until 1899, when the *Institut für Experimentelle Therapie* was erected in Frankfurt am Main to enable him to conduct his practice of experimental therapeutics. This section is a necessary prelude in so far as it deals with the development of Ehrlich's famous *sidechain* or *receptor theory*, which would become an important theoretical resource for the practice of experimental therapeutics. I will also use the opportunity to compare the 'logic' of Ehrlich's scientific career to that of Louis Pasteur's career. The latter has been analyzed by the French sociologist Bruno Latour.

In Section 3 I will describe and analyze the intellectual and in particular the material and social conditions of the practice of experimental therapeutics, first conducted in the *Institut für Experimentelle Therapie* and from 1906 also in the *Georg Speyer Haus*. Drawing upon material, conceptual and organizational resources from the German chemical (synthetic dyestuffs) industry and upon 'animal model systems' first introduced by French researchers - here we see a clear exemplification of Knorr-Cetina's 'transscientific field'! -, Ehrlich built a formidable *construction machinery* for doing experimental therapeutics. His laboratories instituted a drastic 'reconfiguration' of the natural and social order, a kind of reshuffling between chemical substances, experimental animals and human patients, or between industrialists, chemists, biological researchers and clinicians. The material practice introduced by Ehrlich also required a particular 'social tuning' in the form of a characteristic institutional structure, work-style, pattern of research management, and symbiotic relations with the chemical industry.

It is often claimed that Ehrlich's programme of experimental therapeutics represents a *rational* and *purposive* approach to the development of drugs. This claim flies in the face of constructivist science studies which generally emphasize the 'contingent' and 'opportunistic' character of research. Section 4 will therefore have a closer look at the allegedly 'rational' nature of Ehrlich's approach. It will also explore more fully the intellectual or theoretical aspects of his programme.

If - as constructivist authors emphasize - the objects dealt with in the laboratory are highly artificial and bound to conditions established in that special setting, then the *applicability* of the results obtained there to the 'real' or 'normal' world outside is not self-evident. As Knorr-Cetina and her co-workers state: "The transfer to the 'normal world' of an object articulated in the laboratory is therefore problematic, to which one may respond by adapting the 'normal world' to laboratory conditions, or by 'normalization' of the objects produced".[18] Elsewhere she notes that translating scientific accounts into practice and making

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[18] K. Knorr-Cetina (with the collaboration of K. Amann, S. Hirschauer, K.-H. Schmidt), 'Das naturwissenschaftliche Labor als Ort der "Verdichtung" von Gesellschaft', *Zeitschrift für Soziologie* 17 (1988): 85-101, p. 89. The first option mentioned - adapting the 'normal world' to laboratory con-

laboratory objects recur outside the laboratory requires hard *work* and cultural (social, political, economic and the like) *intervention*.[19] From this angle the clinical introduction of salvarsan may be of particular interest. In 1908 Ehrlich could still boast that his approach to biomedical research would largely do away with the need to experiment on human subjects. Through extensive animal experiments and in close cooperation with the chemical and pharmaceutical industry, he opined, the most 'optimal' drugs could be developed and selected, so that the final test on man would amount to no more, as it were, than proving the sum (*nur noch die Probe aufs Exempel*).[20] As it turned out, however, this optimistic appraisal was a gross underestimation of the problems that would afflict the clinical introduction of salvarsan. In fact, the final stage in the development of this chemotherapeutic drug would cause Ehrlich more trouble and headaches than all the preceding stages together. It absorbed all his energy in the last years of his life (1909-1916). In Section 5 I will present a more detailed analysis of the difficulties facing the clinical introduction of salvarsan and of the types of work and intervention that were needed to overcome them. In the concluding section I will return to the analytical issues raised above.

2. The trajectory of Ehrlich's career

The course of Ehrlich's scientific development has been likened to the career of that other genius and reputed hero of microbiology and immunology, Louis Pasteur.[21] Just like Pasteur, Ehrlich could not stick to any particular discipline, but moved freely from one area to another. When we consider his entire career, he appears to have travelled quite a distance from the histological staining of leucocytes to the discovery of salvarsan, just as Pasteur likewise seems to have moved a long way from crystallography to inoculation against hydrophobia. "Yet in both cases", as Robert Muir observes in his obituary of Paul Ehrlich, "step seems to follow step in natural sequence".[22]

ditions - has been popularized by Bruno Latour in his article 'Give Me a Laboratory and I will Raise the World', in K.D. Knorr-Cetina and M. Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*, London/Beverly Hills (Sage), 1983, pp. 141-170.

19 Knorr-Cetina, op. cit. (note 15), p. 559.

20 J. Schwalbe, 'Standesangelegenheiten: Sind Aerzte berechtigt, für ihre wissenschaftlichen Untersuchungen pharmazeutischer Präparate von den auftraggebenden Fabriken Honorar entgegenzunehmen?', *Deutsche medizinische Wochenschrift* 34 (1908): 1730-33, p. 1732.

21 R. Muir, 'Paul Ehrlich 1845-1915', *Journal of Pathology & Bacteriology* 20 (1915-16): 350-60. Cited in C.E. Dolman, 'A fifth anniversary commemorative tribute to Paul Ehrlich, with two letters to American friends', *Clio Medica* 1 (1966): 223-34, p. 223.

22 Ibid.

The trajectory of Pasteur's career has been analyzed by the French sociologist Bruno Latour in his monograph *Les Microbes*.[23] Here I will review Ehrlich's career up to its final 'chemotherapeutic' stage, which will be analyzed in more detail in later sections. A comparison of Pasteur's and Ehrlich's careers might be conducive to a better understanding of both.

Despite the varied nature of Ehrlich's investigations in a wide range of fields, it is not difficult to discern a fundamental insight underlying all his work. From his early student days on, as Ehrlich told Wassermann, he had been 'possessed' by the idea which would become his *Lebensgedanke*, the conviction that each of the living cells and organ systems of the body has specific chemical affinities to (or 'avidities' for) specific substances.[24] This idea had been impressed on Ehrlich's mind when one of his teachers showed him that in chronic lead poisoning the toxic element was concentrated in particular organs of the body. For Ehrlich the medical student it was already beyond doubt that chemistry, in particular organic chemistry, would hold the key to unlock the secret of biological phenomena. He also expected it to throw light on the mysterious action of drugs and to open the possibility of a deliberate search for new medicines.

During Ehrlich's student years in Strasbourg and other cities (1872-1878), the German synthetic dyestuffs industry was well on the way to its eventual domination of world markets; in due course, it would become also, in the words of Samuel Lilley, the "synthetic everything-else industry".[25] Following the example of his cousin Carl Weigert, Ehrlich turned the products of this precocious science-based industry into new investigative tools for medical science. They proved useful as staining agents facilitating the study of various tissues. In other words, dyes (just like lead) are excellent instruments for visibly demonstrating selective affinities. In his 1878 doctoral thesis, Ehrlich used existing chemical and physical theories developed for explaining the attachment of dyes to textile fibres to shed light on the processes involved in tissue and cell staining.[26] Clearly, he borrowed not only *material* but also *conceptual* tools from the emerging dye industry. Michaelis has identified Ehrlich's "preference

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23 B. Latour, *Les Microbes: Guerre et Paix suivi de Irréductions*, Paris, 1984 (Editions A.M. Métaillié). English translation: *The Pasteurization of France*, Cambridge, Ma. and London, 1988 (Harvard University Press).

24 A. Wassermann, 'Paul Ehrlich', *Münchener medizinische Wochenschrift* 56 (1909): 245-47.

25 S. Lilley, 'Technological Progress and the Industrial Revolution', in C. Cipolla (ed.), *The Fontana Economic History of Europe* Vol. 3, Glasgow, 1980, pp. 187-254, p. 243. Information on the history of the German synthetic dye industry can be found in J.J. Beer, *The Emergence of the German Dye Industry*, Urbana (University of Illimois Press), 1959, and A.S. Travis, *The Rainbow Makers: The Origins of the Synthetic Dyestuffs Industry in Western Europe*, Bethlehem USA (Lehigh University Press), 1993.

26 A.S. Travis, 'Science as a Receptor of Technology: Paul Ehrlich and the Synthetic Dyestuffs Industry', *Science in Context* 3 (1989): 383-408.

for colours" (*Vorliebe zur Farbe*) as the driving factor of his career.[27] He would never become unfaithful to his 'first love', as Wassermann expressed it.[28] At this early stage, Ehrlich did not yet establish direct contacts with the dye industry; he procured his samples of dyes through the intermediary of an obliging Freiburg dealer in dyes and drugs.[29]

After his university training, Ehrlich extended his staining techniques to the field of haematology. His findings acquired diagnostic relevance for the detection and differentiation of leukaemias and anaemias. Ehrlich himself, however, became dissatisfied with staining dead preparations and entered upon the study of "real biology" (Wassermann) by developing the technique of *vital staining*. Dyestuffs would be injected into the living animal "to shift the dyeing operation to the organism itself". By using dyes which would lose their colour during reduction and which would regain it during oxidation, Ehrlich was able to obtain clues concerning the 'oxygen avidity' of various tissue and organ systems of living organisms. For this purpose he employed two vital-staining dyes (alizarin blue and indophenol blue), each with a different propensity to turn to its colourless form. The results of his investigations were reported in his *Habilitation* thesis on *Das Sauerstoff-Bedürfniss des Organismus* (1885). In it Ehrlich presented a theoretical model of the supposed chemical composition of cellular 'protoplasm', which would be elaborated further in his later *side-chain* or *receptor* theory.[30] Ehrlich also established the selective staining effect of the dye methylene blue on nerve endings and tried its use for relieving pain in neuralgias. When later on the same dye was found to have a selective affinity for (human) malaria parasites, Ehrlich did not hesitate to give it a clinical trial by administering the dye to two malaria patients in 1891. Although the results looked promising, this particular line of research could not be pursued further because a suitable experimental animal on which to transmit the (human) malaria parasites was not available.[31]

It should come as no surprise that a man like Ehrlich with a basic natural science approach toward medical problems would become attracted to the aetiological tendency in medicine represented by Robert Koch's bacteriology. Ehrlich's first contribution to this new school was to devise a new staining method (1883) for Koch's recently discovered tubercle bacillus, thus rendering this discovery even more practically useful. Yet Ehrlich's finest period arrived around 1890 when bacteriological research turned its attention to the rather

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27 L. Michaelis, 'Die Bedeutung der Farbstoffe für Ehrlichs biologische Forschungen', *Die Naturwissenschaften* 2 (1914): 250-51, on p. 250.

28 Wassermann, op. cit. (note 17), p. 247.

29 Travis, op. cit. (note 26), p. 392 (footnote 6).

30 A detailed analysis of Ehrlich's *Habilitation* thesis is given in A.S. Travis, op. cit. (note 26).

31 This is still a major problem in present-day attempts to develop antimalarial vaccines. See R.S. Desowitz, *The Malaria Capers: Tales of Parasites and People*, New York and London (W.W. Norton & Co.), 1991, p. 226.

mysterious phenomena of (humoral) immunity. It was Ehrlich's investigations into the quantitative relationships between toxins and antitoxins which enabled a precise determination of the effective dosage of antitoxins and thus made Emil Behring's 'serum therapy' (passive immunization) against diphteria into a brilliant practical success, though it was the latter who would reap the pecuniary benefit from this venture through his commercial deals with the Hoechst Dyeworks Company.[32]

Although Ehrlich did not fare well in the financial arrangements between Behring and the Hoechst company (he had been given laboratory space, without salary, in Koch's Institute of Infectious Diseases in Berlin), his vital contributions to the realization of diphteria antitoxin production were duly recognized by Friedrich Althoff, a top-level civil servant (*Ministerialdirektor*) of the Prussian Ministry of Educational and Medical Affairs, and he was made director of the newly created *Institut für Serumforschung und Serumprüfung* in Berlin-Steglitz in 1896. In this capacity he developed complex quantitative techniques for standardizing and assaying the potency of antisera. He also devised, in 1897, his ingenious *side-chain theory* to account for the formation of antibodies and the specificity of their relationship with homologous antigens. Here I will briefly sketch the main outlines of this theory. In Ehrlich's theory, the cellular 'protoplasm' is depicted as a giant 'living' molecule possessing on its surface many different side-chains or receptors, each of them having specific chemical affinities to particular nutritive substances. Antigens, e.g. toxins or other foreign substances, may induce antibody formation when they happen to combine with particular side-chains of the protoplasm through particular chemical groups, the so-called 'haptophore' groups. Those side-chains are thus blocked in their normal functioning for the cellular metabolism, to which the protoplasm will react with replication of similar side-chains. Because this regenerative response is excessive ("Nature is prodigal"), there will now be a surplus of side-chains of this particular type. Some of these excessive receptors will be shed to the blood and henceforth circulate as antibodies with a specific affinity to the antigens which initially triggered the whole process. The characteristic feature of the side-chain theory is that it dispenses with the need to introduce special teleological mechanisms for explaining the apparently purposive character of the immune response. As Ehrlich emphasizes, according to his theory immunity is considered merely "a chapter of the general physiology of nutrition".[33] In subsequent years the theory became however rather complicated because of the need to postulate different types of receptors to account for the existence of different types of antibodies.

Ehrlich's side-chain theory drew its inspiration from ideas that were current in organic and dye chemistry. The very conception of protoplasm as a 'nucleus' with many 'side-chains' was constructed, as the terminology bears witness, in analogy with Kekulé's theory of the

32 H.A. Lechevalier and M. Solotorovsky, *Three Centuries of Microbiology*, New York (Dover), 1974, p. 223.

33 P. Ehrlich, 'Die Schutzstoffe des Blutes', in *Gesammelte Arbeiten zur Immunitätsforschung*, Berlin (Hirschwald), 1904, 515-54, p. 546.

benzene ring ('nucleus') allowing for different substituents ('side-chains'). Moreover, Ehrlich used the lock-and-key simile first introduced by the organic chemist Emil Fischer to elucidate the specificity of the antigen-antibody relationship. Finally, Ehrlich distinguished between different chemical groups within toxin and antibody molecules, each responsible for different functions, which was in agreement with ways of thinking prevalent in dye chemistry. Just as the dye chemist Otto Witt distinguished 'haptophore' and 'chromophore' groups within the dye molecule to account for its attachment to textile fibres and its colouring power respectively, Ehrlich distinguished 'haptophore' and 'toxophore' groups within the toxin molecule to account for its combining power with protoplasm receptors and its toxicity. On the basis of such borrowings, Travis has metaphorically characterized Ehrlich's theory as being itself a 'receptor' of the German synthetic dyestuffs industry.[34]

In the years after 1900 Ehrlich's side-chain theory gained much popularity in medical circles in German-speaking countries, although it was strongly opposed by the bacteriologists Max Gruber and Jules Bordet and by the Swedish chemist Svante Arrhenius. In Germany, the exposition and defence of his theory became, as Lewis Rubin notes, "something of a cottage industry".[35] August Wassermann and Carl Bruck were among those who took part in this enterprise, each writing a brochure on the side-chain theory. After the First World War, the theory largely fell into disrepute. When the so-called clonal selection theories of antibody formation emerged after 1955, however, Ehrlich's theory was recognized in retrospect as a respectable precursor.[36] More recently, Ehrlich has even been credited with anticipating the latest ideas on anti-antibodies and anti-idiotype immunoregulation.[37]

Around the turn of the century, Ehrlich was not only interested in unravelling the phenomena of immunity. He also tried to understand the pharmacological action of medicines. His ambition was to deliberately design drugs which would act effectively, and not just symptomatically, on the causes of diseases. The side-chain or receptor theory would ultimately help to fulfil this ambition. In 1899, through the efforts of Friedrich Althoff and the lord mayor of Frankfurt am Main, Franz Adickes, Ehrlich was given the directorship of the newly created *Institut für Experimentelle Therapie* in that city, which enabled him to pursue his new aims in addition to continuing the old function of state control of commercial antisera. The geographical proximity of two dyestuffs firms, Cassella and Hoechst, constituted an impor-

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34 Travis, op. cit. (note 26).

35 L.P. Rubin, 'Styles in Scientific Explanation: Paul Ehrlich and Svante Arrhenius on Immunochemistry', *Journal of the History of Medicine and Allied Sciences* 35 (1980): 397-425, p. 422.

36 A modern textbook states: "[..] Ehrlich proposed the first selective theory of antibody formation". See I. McConnell et al., *The Immune System*, Oxford (Blackwell Scientific Publications), 1981, p. 115.

37 A.M. Silverstein, 'Anti-antibodies and Anti-idiotype Immunoregulation, 1899-1904: The Inexorable Logic of Paul Ehrlich', *Cellular Immunology* 99 (1986): 507-22.

tant asset to Ehrlich. He was offered even more favourable opportunities to investigate the 'chemotherapy' of infectious diseases in 1906, when Franziska Speyer erected and endowed the *Georg-Speyer Haus*, adjacent to Ehrlich's Institute, in memory of her late husband (Georg Speyer had been a leading Jewish financier and philanthropist in the city of Frankfurt). Now everything was in place for a full-scale implementation of Ehrlich's chemotherapeutic research programme.

Pasteur's and Ehrlich's careers compared

As I remarked above, Ehrlich's scientific career has been likened to that of Louis Pasteur. Here I will try to bring into focus those features that were common to the careers of both. Just like Ehrlich, Pasteur moved freely from one area of investigation to the next, although his biographers have also noted Pasteur's obstinate tenacity to stick to his chosen course. (This trait corresponds to Ehrlich's self-confessed 'monomania', despite his astonishing versatility.[38]) What was in fact characteristic of Pasteur throughout his career, according to Bruno Latour, was his strategy of displacement (*déplacement*) and translation. Every time that one would have expected Pasteur to continue his investigations in the area of science in which he had just met with some success, he would take a sidestep and turn to a novel, more 'applied' problem area that would capture the interest(s) of a larger number of people. All the same, he would transform the (more) 'applied' problem into a 'basic' problem to be solved by means that had been acquired in the area which he had just left. Starting out his career with some esoteric problems in crystallography which were of interest only to a handful of chemical colleagues, Pasteur moved on to the heated controversy on the nature of ferments, then to the study of the economically important 'diseases' of beer, wine, and silk-worms, then to the problem of spontaneous generation, then to problems of veterinary medicine (anthrax, chicken cholera), and, finally, to human medicine. With each newly chosen subject, the circle of interested people would become wider. After Pasteur successfully vaccinated, in 1885, two boys who had been bitten by rabid dogs, an unending stream of thousands of people seeking similar treatment flocked to Paris. To help them and to support further research, a staggering amount of 2,586,000 gold francs were collected from the general public, enabling the founding of the *Institut Pasteur* (1888). "Has credibility ever been converted into capital so quickly in the history of the sciences?", Latour asks rhetorically.[39] Pasteur ended up as the deified 'Pasteur', the man of the century, who would give his name to streets in all the cities of France.

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[38] M. Marquardt, *Paul Ehrlich als Mensch und Arbeiter*, Stuttgart, Berlin and Leipzig (Deutsche Verlags-Anstalt), 1924, pp. 55-56.

[39] Latour, op. cit. (note 23), p. 113; *Pasteurization*, p. 101.

Bruno Latour offers a simple formula to explain the 'secret' of Pasteur's career: "he innovates by forming associations" (*c'est en associant qu'il innove*).[40] For the hagiographer thirsting after 'genius', this formula would not do; but it is sufficient to the historian or sociologist, Latour maintains. As a matter of fact, Latour nowhere hides his admiration for Pasteur's strategy which he calls really *géniale*.[41] Apparently, for Latour, the touch of genius is not in the scientific insights but in the progressive movement of displacement and translation.

Ehrlich's career exhibits a similar 'logic' to that of Pasteur's. As Travis writes: "He [Ehrlich] moved from one area of investigation to the next as soon as he had gone as far as he could go - given the materials, theories, and investigative instruments and methods of the day. At each move he was better armed with experience to tackle new problems".[42] As Ehrlich moved from one area to another, Travis observes, his powers of persuasion increased accordingly. Ehrlich would become "a master at persuading [..] audiences, especially those who would provide financial backing".[43] Latour's observation made with regard to Pasteur - that with each newly chosen problem area the circle of those interested would become wider - also appears to hold for Ehrlich's career.

Carl Browning, a former co-worker of Ehrlich's, draws attention to another salient aspect of Ehrlich's progressive movement through the 'credibility cycle'[44]: with each new area of inquiry, his investigations not just captured the interest of more people but also became more expensive. Shortly after the turn of the century, Browning observes, Ehrlich had accumulated enough 'credibility capital' to embark upon the final stage of his career:

> "Ehrlich's reputation had now gained the standing which made it possible to undertake his cherished plans for chemotherapy. This was the most expensive of his researches in the demands on materials and skilled assistance in chemistry and various departments of pure and applied biology".[45]

In the following sections I will examine these expensive researches more closely.

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40 Latour, op. cit. (note 23), p. 77; *Pasteurization*, p. 69.

41 Ibid., p. 79; *Pasteurization*, p. 71.

42 A.S. Travis, 'Paul Ehrlich: A Hundred Years of Chemotherapy 1891-1991', *The Biochemist* 13 (1990): 9-12, p. 9.

43 Ibid.

44 B. Latour and S. Woolgar, *Laboratory Life*, Beverly Hills and London (Sage), 1979, Chapter 5.

45 C.H. Browning, 'Emil Behring and Paul Ehrlich: Their Contributions to Science', *Nature* 175 (1955): 570-71, p. 571.

3. The practice of experimental therapeutics: intellectual, material and social conditions

Nothing succeeds like success, as the American saying has it. In a prosperous career, new and larger successes are achieved by building on the smaller successes of the past. This was valid both for Pasteur's and for Ehrlich's career. Beyond this truism, we did not offer much of an explanation in the preceding section. Latour's formula of 'innovation by association' is not really explanatory either. It is vulnerable to the same objection as was raised by Knorr-Cetina against the metaphor-and-analogy theory of innovation, to wit, that it is a 'theory of failure and mistake' as well as a 'success formula'.[46] As such, Latour's 'theory' does not discriminate between the conditions of success and of failure, between the 'good' and the 'bad' associations.

An example from Ehrlich's career will illustrate the point. In 1901, at the prodding of *Ministerialdirektor* Althoff and the lord mayor Adickes, the Theodor Stern Foundation was persuaded to finance a cancer research station at Ehrlich's Institute in Frankfurt. With the money went the obligation to thoroughly investigate the phenomena of cancer. Although some scientific results were achieved (e.g. the finding that mouse mammary tumours could be transplanted), in the end, after years of work on this subject, Ehrlich and his collaborators were unable to deliver the hoped-for medical goods. Probably for this reason Claude Dolman concluded: "This cancer work represented an unsought digression from his [Ehrlich's] main course (..)".[47] Undoubtedly, this judgement is retrospectively coloured by the known relative lack of success of Ehrlich's cancer work. But what if this research had led to a cure for cancer? Then perhaps Ehrlich's chemotherapeutic programme might have led nowhere, and would have been judged retrospectively as 'an unsought digression from his main course' ... At any rate, Ehrlich's 'association' with the Theodor Stern Foundation was no guarantee for a successful innovation in the area of cancer.

It has even been suggested that Ehrlich's involvement with immunology (which brought him the Nobel Prize in 1908!) also constituted a deviation from the main course of his career, or at least an interruption between his early work on the constitution, distribution and action of drugs and his later chemotherapeutic work properly so called.[48] It would be equally plausible, however, to consider Ehrlich's long immunological intermezzo as a kind of *incubation period* in which his side-chain theory took final shape and in which the preconditions for embarking on the next venture were created. Moreover, as long as expectations about the possibilities of 'serum therapy', nurtured by Behring's successes with diphteria and

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46 Knorr Cetina, op. cit. (note 3), p. 65.

47 C.E. Dolman, 'Ehrlich, Paul', in C.G. Gillispie (ed.), *Dictionary of Scientific Biography*, Vol. IV, New York, 1971, p. 297.

48 H. Sachs, 'Die Bedeutung Paul Ehrlichs für die biologischen Naturwissenschaften', *Die Naturwissenschaften* 4 (1916): 149-55, p. 149.

tetanus, soared high, the need for a 'chemotherapeutic' approach to infectious diseases would appear less urgent. (In Chapter IV we have seen that the Breslau dermatologist Albert Neisser was prompted by Behring's serum therapy to search for an immunization method against syphilis. Only in November 1907, after two expeditions to the Dutch East Indies, did he recognize the definite failure of his project.)

For Ehrlich, the miraculously selective work of antibodies in natural immune responses also provided a *challenge* to the ingenuity and powers of the synthetic chemist, a kind of ideal model which a chemotherapeutic approach could only hope to emulate. "If one would charge", he wrote in 1897, "a chemist with the task of finding an antidote against an alkaloid or other poison, which would constitute a chemically and physiologically indifferent substance, which would neither destroy the poison nor precipitate it to an insoluble state, yet which would nevertheless be able to neutralize any quantity whatsoever of this poison, he would surely dismiss such an assignment as a chimaera. All the same, the living organism is able to make light work of this task, frequently within the span of a few days and for a large number of poisons".[49] In contrast to many influenced by vitalistic doctrines, Ehrlich did not recognize an inseparable gulf between the "biotherapeutic" and the "chemotherapeutic" approach; his side-chain theory had offered a *chemical* conception of the action of toxins and antitoxins.[50] Finding some kind of artificial substitute for naturally occurring antibodies was thus in principle within the realm of the feasible, however exacting the realization of this goal might otherwise be. In this connection I have to remind the reader of the fact that the expression 'magic bullets' (*Zauberkugeln*), which for us has become almost synonymous with Ehrlich's salvarsan and its modern successors and symbolic for the reductionistic strivings of present-day biomedical research[51], was originally coined by Ehrlich to refer to antibodies.[52] These magic bullets, "which seek their target of their own accord", provided an exemplar for the synthetic drugs yet to be constructed.

At first Ehrlich did not believe that his side-chain theory, or more particularly his receptor concept, would also be applicable to the pharmacological activity of drugs. As Parascandola and Jasensky point out, "(..) it took him about ten years to apply his side chain

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49 Cited in E. Heymann, 'Zur Geschichte der Seitenkettentheorie Paul Ehrlichs', *Klinische Wochenschrift* 7 (1928): 1257-60, p. 1258.

50 P. Ehrlich, 'Ueber die Beziehungen von chemischer Constitution, Vertheilung und pharmakologischer Wirkung', in P. Ehrlich (Hrsg.), *Gesammelte Arbeiten zur Immunitätsforschung*, Berlin (Hirschwald), 1904, pp. 572-628, p. 574.

51 B. Dixon, *Beyond the Magic Bullet*, London (George Allen & Unwin), 1978.

52 P. Ehrlich, 'Ueber den jetzigen Stand der Chemotherapie', *Berichte der Deutschen Chemischen Gesellschaft* 42 (1909): 17-47, p. 21.

theory to the problem of drug action".[53] Only by 1907 was Ehrlich willing to accept the idea that synthetic drugs become attached to the protoplasm of the cell by special atom groupings ('receptors' or 'chemoreceptors') in the same way as toxin molecules did. One reason for his hesitation was that many drugs could be easily extracted from tissues by solvents, thus exhibiting a lack of firm bonding.[54] Another reason was that chemically defined drugs did not induce antibody production.[55] These doubts about the applicability of his receptor theory to the action of drugs did not prevent Ehrlich from launching his chemotherapeutic programme. Although he initially thought it unlikely that drugs entered into a 'chemical union' with the 'living protoplasm', he was nevertheless convinced that chemical affinities ('in the widest meaning of the term') were involved in the interaction between drugs and cells.[56] That was sufficient to embark on his programme. The results to be obtained would subsequently lead him to revise his view on the inapplicability of the receptor theory.

Synthetic chemistry and experimental biology

The strategy of experimental therapeutics to find out specific curative drugs for particular diseases required that a large number of different chemical compounds be tested on a large number of experimental animals in which the diseases in question had been articially produced. To us this might appear only too obvious, but in Ehrlich's time it wasn't. Ehrlich criticized current pharmacology (and toxicology) precisely for its almost exclusive use of experiments on *normal* (non-diseased) animals. Existing types of research, he held, could lead only to the discovery of symptomatically acting drugs like analgesics, antipyretics, anaesthetics, sedatives etcetera, but not to truly curative drugs acting on the causes of diseases. Ehrlich also noted that achievements in the pharmalogical area had been largely due to the initiatives of chemists. Already in 1898, in a lecture before the Association for Internal Medicine, he indicated a different approach:

> "A change for the better will occur only when purely biological views are adopted, that is, when the initiative is transferred from the chemist's workshop to biological laboratories. As physicians we should cease to content ourselves in such important questions with auxiliary roles like adviser or even assistant and demand that we be given pride of place in the domain that is most properly ours".[57]

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[53] J. Parascandola and R. Jasensky, 'Origins of the Receptor Theory of Drug Action', *Bulletin of the History of Medicine and Allied Sciences* 48 (1974): 199-220, p. 205.

[54] Ibid., p. 206.

[55] Ehrlich, op. cit. (note 50), p. 616.

[56] Ehrlich's suppositions on the precise nature of these chemical affinities are discussed in Parascandola and Jasensky, op. cit. (note 53), pp. 206-209.

[57] Ehrlich, op. cit. (note 50), p. 581.

These words have been interpreted to mean that Ehrlich's goal was "to free physicians from the threatened grip of the chemists".[58] To offer such an interpretation is to fall victim to the rhetoric employed by Ehrlich to impress a medical audience (when addressing an audience of chemists, as in his lecture before the German Chemical Society of 31 October 1908, his rhetoric was significantly different![59]). If anything, Ehrlich's experimental therapeutics had to rely even more on the support of the "chemist's workshop" for the production of large numbers of compounds and the systematic variation of their composition. The characteristic feature of his approach was the *combination* of synthetic chemistry with 'experimental biology' on a large scale, enabling extensive tests of many chemical compounds on many artificially infected animals. This cooperation between 'chemistry' and 'biology', it was hoped, would lead to the discovery of theoretical guidelines which could direct the search for therapeutic compounds. Only in that sense was the initiative to be taken away from the "chemist's workshop".

Work on experimental therapeutics[60] started in earnest in 1903. Because the Frankfurt Institute did not dispose of a chemical laboratory (as the later *Georg Speyer Haus* did), Ehrlich secured the cooperation of Arthur Weinberg, director of the nearby Cassella dyestuffs company, to supply him with the needed chemical compounds. The other required component of experimental therapeutics was an 'animal model system'. Suitable experimental animals are often not easy to come by. Remember that Ehrlich's early experiments with methylene blue on (human) malaria could not be followed up because this disease cannot be reproduced in animals! Ehrlich eventually settled on investigating various trypanosome infections in mice and rats. Trypanosomes are the protozoal agents of a number of tropical and subtropical diseases such as sleeping sickness (caused by *Trypanosoma gambiense*), the tsetse-fly cattle-disease *nagana* (caused by *T. brucei*) and the South American horse disease *Mal de Caderas* (caused by *T. equinum*). The causative role of these protozoal organisms, together with that of their respective insect vectors, had been established only recently. During the heyday of

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[58] J. Liebenau, 'Paul Ehrlich as a commercial scientist and research administrator', *Medical History* 34 (1990): 66-78, p. 78. Liebenau wrongly ascribes Ehrlich's pronouncements to the year 1904 (instead of 1898) and considers them as reflecting a sudden dissatisfaction on Ehrlich's part with the work going on in his Frankfurt Institute. This interpretation is wide of the mark.

[59] Cf.: "(..) throughout my scientific work I have exerted myself to make the insights and teachings of chemistry useful to medicine", P. Ehrlich, op. cit. (note 52), p. 17.

[60] The expression 'experimental therapeutics' was sufficiently ambiguous to include coverage of cancer research, bacteriological and hygienic research and the statutory control testing of commercial antisera for serum therapy, which were also conducted within Ehrlich's *Institut für Experimentelle Therapie* in Frankfurt. The term 'chemotherapy', though sporadically used before, was adopted in September 1906 as the official designation for the research to be implemented in the *Georg Speyer Haus*. Despite the different label, there is no essential difference with the 'experimental therapeutics' in the more narrow sense already conducted before that date in the Institute.

colonialism they enjoyed considerable medical interest.[61] An additional reason for Ehrlich to concentrate on trypanosome diseases was that they appeared beyond the reach of serum therapy. But the most important consideration was the availability of a suitable experimental animal.[62]

Certain species of trypanosomes (like *T. brucei* and *T. equinum*) can be easily transmitted to white mice. The parasites develop and multiply in the blood of the rodents until the latter, after 3 or 4 days, decease. The course of the infection can be exactly monitored by microscopic inspection of the blood. A drop of blood from an infected mouse can be used to reinfect another mouse. In this way trypanosome strains can be propagated and maintained in laboratories for many years. In short, mice infected with trypanosomes constitute an almost ideal 'experimental system' (Rouse).

It was not to Ehrlich's credit, but to that of Alphonse Laveran and Félix Mesnil of the *Institut Pasteur*, that trypanosome-infected mice were introduced as an experimental system. A former co-worker of Ehrlich's, Julius Morgenroth, was not chary in his praise of the French contribution:

> "This excellent experimental object (*Versuchsobjekt*), as is constituted by the trypanosome-infected mouse, was first employed for chemotherapeutic trials by Laveran and Mesnil; one may view this advance in experimental technique as a decisive turn in the development of experimental chemotherapy".[63]

Ehrlich received his first two rodents (actually they were rats, but the disease was soon transmitted to white mice) infected with the agent of *Mal de Caderas* (T. equinum) from the French researcher Professor Nocard in December 1902.[64] He immediately wrote down instructions to his collaborators for trying various chemical compounds on these experimental animals.

The management of scientific manpower

Besides chemical compounds and experimental animals a third type of resource was needed to realize Ehrlich's programme of experimental therapeutics: skilled manpower. A limited number of chemically and medically (or biologically) trained staff members of the Institute, and of the Georg Speyer Haus after 1906, could be set free to devote themselves exclusively

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[61] M. Worboys, 'The Emergence of Tropical Medicine: A Study in the Establishment of a Scientific Specialty', in G. Lemaine et al. (eds.), *Perspectives on the Eemergence of Scientific Disciplines*, The Hague/Paris (Mouton), 1976, pp. 75-98.

[62] P. Ehrlich, 'Chemotherapeutische Trypanosomen-Studien', *Berliner klinische Wochenschrift* 44 (1907): 233-36, 280-83, 310-14, 341-44, p. 234.

[63] J. Morgenroth, 'Die experimentelle Chemotherapie und das Problem der inneren Desinfektion bei bakteriellen Infektionen', *Die Naturwissenschaften* 1 (1913): 609-15, p. 611.

[64] M. Marquardt, op. cit. (note 38), pp. 71-72; Ehrlich, op. cit. (note 62), pp. 234-35.

to this task. Their number was supplemented by visitors from abroad who, attracted by Ehrlich's international reputation, volunteered to work in his laboratories: Reid Hunt, Christian Herter, and Preston Kyes from the United States, Carl Browning and Henry Dale from Great Britain, and Kyoshi Shiga and Sahachiro Hata from Japan, among others.[65]

Ehrlich kept himself in strict control of the activities of his personnel. Every morning, or even the night before, he wrote down detailed instructions to his several co-workers for their day's work on so-called "blocks" (which were copied in the copybook).[66] The progress of their work was also closely monitored. Ehrlich's motto was "uniform direction of research combined with as much independence as possible for individual researchers" (*Einheitliche Richtung der Forschung bei möglichst selbständigen Leistungen der Einzelnen*)[67], but in practice the emphasis was much more on the uniformity of direction than on the independence, or as Parascandola dryly remarks: "(..) Ehrlich did not want his coworkers to become too independent".[68] Dolman has characterized him as a "benign dictator".[69] Although Martha Marquardt, Ehrlich's secretary from 1902, described his relationship to his assistants as "jovial and friendly", she too had to admit that "he could act very energetically when his instructions were not obeyed exactly".[70] It is no wonder that the routine character of the prescribed chemical and biological investigations, combined with Ehrlich's close supervision, sometimes led to resentment among his staff.[71] However, his collaborators were not only supposed to reliably carry out his instructions but also to discuss the results with him. Ehrlich's formulations of theory, as his former coworker Carl Browning notes, were "often hammered on the anvil of his assistants' brains".[72] So the 'exchange of ideas' between Ehrlich and his staff, though rather asymmetrical and onesided, was not completely absent. Ehrlich also awarded his coworkers some credit for their work by granting them authorship or coauthorship of scientific publications.

Ehrlich's style of research management and his organization of scientifically trained labour were not entirely his own invention. This pattern of research organization had already been pioneered by the German synthetic dye industry after the founding of research laborato-

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65 Dolman, op. cit. (note 47), p. 298.

66 Marquardt, op. cit. (note 38), p. 39.

67 Marquardt, op. cit. (note 38), p. 44.

68 J. Parascandola, 'The Theoretical Basis of Paul Ehrlich's Chemotherapy', *Journal of the History of Medicine and Allied Sciences* 36 (1981): 19-43, p. 32, footnote 44.

69 Dolman, op. cit. (note 21), p. 224.

70 Marquardt, op. cit. (note 38), p. 42 and p. 45.

71 Ibid., p. 43 and p. 45.

72 Browning, op. cit. (note 45), p. 571.

ries.[73] The first research director of BASF, Heinrich Caro, had characterized the "endless combination game" in these laboratories of coupling numerous compounds as "scientific mass-labour" (*wissenschaftliche Massenarbeit*).[74] In 1909 Carl Duisberg, a director of the Bayer company and a future architect of the IG Farben, summed up the routinized and uncreative character of research work in the chemical laboratories in one blunt statement: "Von Gedankenblitz keine Spur!" (No trace of a flash of genius!).[75] The leading representatives of the industry held that new chemical products and processes were "corporate inventions" (*Etablissementserfindungen*), that is, they were the result of the entire research organization and could not be attributed to individual researchers. (Thus the latter's claims for higher royalties on patented inventions could be subverted.) The organization of research in Ehrlich's Frankfurt Institute and in the Georg Speyer Haus was modelled on the existing pattern of research management in the synthetic dye industry. Thus this industry provided Ehrlich not only with the material and conceptual tools he needed, but also with an organizational 'tool'.[76] Of course, the latter had to be adapted to the practice of experimental therapeutics. Ehrlich's specific contribution was that he extended and combined the 'chemical mass-labour' of the synthetic dye industry with 'biological mass-labour' in animal experimentation. In due course, the German chemical and pharmaceutical industry was to 're-borrow' in its turn Ehrlich's model for conducting experimental therapeutics when several dyestuffs firms introduced their own departments for chemotherapy. In 1913, Carl Duisberg summed up the requirements for chemotherapeutic research:

> "First, we need a fully equipped chemical laboratory, then a pharmacological institute with a staff of men trained in medicine and chemistry, *an abundance of animals to experiment upon*, and finally - the latest

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[73] E. Homburg, 'The emergence of research laboratories in the dye-stuffs industry, 1870-1900', *British Journal for the History of Science* 25 (1992): 91-111.

[74] H. Caro, 'Ueber die Entwicklung der Theerfarben-Industrie', *Berichte der Deutschen Chemischen Gesellschaft* 25 (1892): 955-1105, p. 1095.

[75] H. van den Belt and A. Rip, 'The Nelson-Winter-Dosi Model and Synthetic Dye Chemistry', in W.E. Bijker, T.P. Hughes and T. Pinch (eds.), *The Social Construction of Technological Systems*, Cambridge, MA (MIT Press), 1987, pp. 135-58. See also T.P. Hughes, *American Genesis*, Harmondsworth (Penguin Books), 1989, p. 183.

[76] As John Pickstone notes, the 'scientization of industry' and the 'industrialization of academic science' are only two sides of the same coin. In terms of his typology of 'socio-cognitive types' of science, technology and medicine (savant STM, analytical STM, experimental STM and techno-science), Ehrlich's work clearly represents the type of *techno-science*. See J.V. Pickstone, 'Ways of knowing: towards a historical sociology of science, technology and medicine', *British Journal for the History of Science* 26 (1993): 433-58, p. 453.

> development in the field - a chemotherapeutic and bacteriological department, equipped according to the ideas of Paul Ehrlich; all these must be in close connection with one another."[77]

The first director of chemotherapeutic research at the Bayer factories of Wuppertal-Elberfeld was Heinrich Hörlein. It was under his direction that important medicines like germanine, atebrine, and, in the 1930s, the sulfonamides (Prontosil) would be found. In an IG Farben publication of 1933 Hörlein noted that in Anglo-Saxon countries the word *teamwork*, originally at home in the world of sports, had entered into use to refer to the kind of scientific cooperation among researchers from different disciplinary backgrounds, as exemplified by Ehrlich's team. This *teamwork* was supposed to be directed by *the leader of the team*, "in whose hands all the different strands come together".[78] In this sense, Ehrlich had indeed been a team leader. No doubt, Hörlein tried to follow in his footsteps.

The symbiosis with the chemical industry

Without close relationships with the German chemical industry, Ehrlich's programme could hardly have been implemented. We have already seen that the dyestuffs firm of Cassella near Frankfurt provided Ehrlich with the chemical compounds for his initial investigations on trypanosome infections. For this purpose alone, much more than a hundred different substances were tried, many of them (e.g. the almost 50 substituted products of trypan red) prepared and tailor-made on Ehrlich's special instructions.[79] In October 1904, Cassella entered into a 'community of interests' (*Interessengemeinschaft*) with the larger firm of Hoechst, which was a first step in the concentration of economic power eventually leading to the formation of IG Farben in 1925.[80] Now Ehrlich had to deal directly with the Hoechst company, already known from his previous experience, for making arrangements over material and financial support in return for the company's right to take out patents on prospective new drugs. From the point of view of a neat separation of interests, this symbiotic relationship with Hoechst might appear rather dubious. As an official institution the *Institut für Experimentelle Therapie* was charged with the statutory control of antisera, of which Hoechst was the most important producer; on the other hand, this institute (and the Georg Speyer Haus

77 C. Duisberg, *Chemical News* 23 (1913), pp. 246-47 (emphasis added). Cited in Liebenau, op. cit. (note 58), pp. 77-78.

78 H. Hörlein, 'Medizin und Chemie', in IG Farbenindustrie Aktiengesellschaft, *Medizin und Chemie*, Leverkusen, 1933, pp. 7-25, p. 10.

79 P. Ehrlich, op. cit. (note 62), p. 235.

80 Beer, op. cit. (note 25), 130 and p. 145.

later) was to be subsidized by this same firm for conducting research on experimental therapeutics.[81]

Ehrlich defended the close cooperation with the chemical and pharmaceutical industry. He expounded his view in 1908, in a response to a survey by the *Freie Vereinigung der medizinischen Fachpresse* (Free Association for the Medical Press) on the question of whether physicians were allowed to accept fees from the commissioning chemical factories for their clinical and experimental investigation of pharmaceutical preparations.[82] In Ehrlich's view, a sharp distinction had to be made between experimental and clinical investigation. Experimental research on new medicines was a very costly affair, and only feasible if the chemical factories were willing to shoulder a large part of the expenses on materials, test animals and personnel:

> "The material and mental support of our chemical factories is largely indispensable for modern therapeutics, and it would therefore not be advisable to loosen this natural union".[83]

According to Ehrlich, the further development of experimental therapeutics held out the only hope of abolishing the abusive practice of introducing new medicines by experimenting on human patients. Restrictions on the acceptance of fees, Ehrlich declared, should not prevent the realization of this prospect.

4. The contingencies of a rational approach

It is a common view that Ehrlich's chemotherapeutic programme, guided by his sidechain or receptor theory, initiated a new era in pharmacological research, replacing the 'trial and error' and 'blind empiricism' of the old days with a 'rational' and 'purposive' search for causally effective drugs. Modern pharmacologists consider their attempts to deliberately design medicines with desirable properties on the basis of a knowledge of structure-activity relationships as a direct continuation of Ehrlich's work.[84] Ehrlich himself had proclaimed

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81 Liebenau, op. cit. (note 58), p. 73, suggests that "the government laboratory" was "unofficaIly endorsing Hoechst biologicals". He also states that "the Hoechst Company saw the Georg Speyer Haus as an extension of their own laboratory" (p. 77). Among the evidence adduced to support this statement is a file in the Hoechst Archive on (Albert) Neisser's expedition to the Dutch East Indies. It appears, however, that Liebenau has mistaken *Albert* Neisser, the Breslau dermatologist and a friend of Ehrlich's, for *Max* Neisser, a member of Ehrlich's staff at the Frankfurt Institute.

82 Schwalbe, op. cit. (note 20).

83 Ibid., p. 1732.

84 H.A.J. Struijker Boudier, 'De wortels van het geneesmiddel', *Wijsgerig Perspectief* 33 (1992/93): 171-75. This author considers the 'receptor concept' (a modern refinement of Ehrlich's notion) as paradigmatic for modern pharmacology.

that "the times of purely *empirical* therapies have passed" and that we should aim at "a truly *rational* application of medicinal remedies"[85] (emphasis added). Or to use his vivid simile: one should not grab blindly like a bear in the soup bowl if one wanted a good piece.[86] Ehrlich's advocacy of a rational and purposive approach to pharmacology may have been inspired by the ideal of '*zielbewusste Synthese*' or 'purposive synthesis' (Caro) in dyestuffs chemistry. By using knowledge on the relations between chemical structure and colour or other properties (e.g. Otto Witt's colour theory), dye chemists thought it possible in principle to deliberately design and construct new dye molecules with desired qualities in the chemical "drawing room" (with structural formulae representing "construction drawings").[87]

Many of his followers and admirers hold that Ehrlich indeed succeeded in adopting a rational and purposive approach to the development of drugs. Thus in 1914 Carl Bruck, who had been intimately involved in the development of the Wassermann reaction, emphasized the methodical and goal-directed character of Ehrlich's chemotherapeutic work by drawing the following comparison:

> "His actions can be compared to those of a marksman who exactly knows his weapon, the performances of his projectiles and the trajectories they must take and whose eye has always just one particular aim in view, which to reach is his final goal. At first the projectiles may err in the void, but the circle of their trajectories will become steadily narrower, the number of hits will increase, until finally, sent by the marksman's hand guided by observation and experience, a master shot in the bull's eye occurs".[88]

Bruck added that Ehrlich's lifework did not consist of *chance discoveries* (in 1921, in his polemic with Wassermann, he would characterize the discovery of the Wassermann reaction as just that kind of discovery![89]). In the same vein, Alexander Berg has asserted that Ehrlich's discovery of salvarsan was in no way to be attributed to chance, but was "the fruit of indefatigable laboratory work, guided by the idea of a great natural scientist".[90] Assertions to the effect that Ehrlich has put the development of new medicines on a rational and scientific basis are indeed legion.

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85 Ehrlich, op. cit. (note 62), p. 233 and P. Ehrlich, 'On partial functions of the cell: Nobel lecture', in *The Collected Papers of Paul Ehrlich*, ed. F. Himmelweit et al., vol. III: 183-94, p. 183.

86 Cited in Parascandola, op. cit. (note 68), p. 40.

87 Van den Belt and Rip, op. cit. (note 75).

88 C. Bruck, 'Salvarsan und Syphilis', *Die Naturwissenschaften* 2 (1914): 258-63, p. 258.

89 See chapter V.

90 A. Berg, 'Paul Ehrlich, 1854-1915', in H. Freund and A. Berg (eds.), *Geschichte der Mikroskopie: Leben und Werk grosser Forscher*, Vol. II, Frankfurt am Main (Umschau Verlag), 1964, 79-89, pp. 85-86.

The description that is usually given of Ehrlich's research is at odds with the general picture of scientific work that emerges from constructivist laboratory studies. Such studies repeatedly demonstrate the *opportunistic*, *contingent*, and *idiosyncratic* nature of research.[91] The question thus arises whether an overly rationalized and idealized image of Ehrlich's chemotherapeutic work has been drawn up or whether his pursuits are perhaps the exception to the rule. It is also possible, of course, that the vocabulary used - 'rational' versus 'empirical', 'methodical' and 'purposive' versus 'blind' and 'chancelike' - may be too rigid and polarized for accurate description. Keeping the latter possibility in mind we will have a closer look at Ehrlich's chemotherapeutic work.

Four critical observations

The first thing to be noted is that at the start of his chemotherapeutic programme in 1902/-1903, Ehrlich did not consider his receptor concept applicable at all to the pharmacological action of drugs. It took him some years before he changed his opinion in 1907. What eventually made him change his mind was the discovery of *resistance* of some trypanosome strains against particular substances. In itself, this discovery of resistance (*Arzneifestigkeit*) was the consequence of a 'resistance' in Pickering's sense. After repeated administrations of trypanocidal substances it sometimes turned out, rather unexpectedly, that trypanosomes could no longer be made to disappear from the blood of white mice by renewed administration. Ehrlich and his co-workers decided that the parasites, and not the rodents, had undergone an alteration and from then on used this phenomenon as a research tool for acquiring new insights in the action of drugs. They found out that they could raise resistant strains of trypanosomes at will, by repeated administration of suitably selected and increasing dosages of compounds which in normal quantities would kill off those microorganisms. This phenomenon of resistance exhibited a remarkable specificity. Trypanosomes that had been made resistant to the arsenical substance atoxyl would also show resistance to other arsenical compounds (e.g. acetyl-atoxyl) but not to other classes of compounds such as azo dyes (e.g. trypan red) or basic triphenylmethane dyes (e.g. fuchsine). True, it would not be impossible to raise strains with a threefold resistance (i.e. against each of the three classes of substances), but then each type of resistance had to be developed separately. To explain these empirical data Ehrlich postulated that the cellular protoplasm of non-resistant trypanosomes possessed different '*Angriffsstellen*' ('places of attack') or receptors for which specific classes of substances have particular affinities.[92] Drug resistance would ensue when particular receptors, which are normally in good working order, somehow develop a reduced affinity. So the receptor theory of drug action, which is often supposed to have guided Ehrlich's

[91] For an overview of the findings of lab studies, see Knorr-Cetina, op. cit. (note 5). Andrew Pickering has characterized the dynamics of scientific practice as 'opportunism in context'.

[92] Ehrlich, op. cit (note 62), p. 342.

chemotherapeutic programme from the outset, was only developed during the course of this very programme.[93] It was not yet available when the Georg Speyer Haus was opened in September 1906.

A second point concerns the choice of chemical compounds to be tested for pharmacological action. Ehrlich was always inspecting the long lists of new products published by the German chemical industry, ready to try in his animal experiments any new compound which showed some sign of promise. Yet the initial detection of a substance exhibiting some action, however slight, on a particular infection is, as Ehrlich himself emphasized, always and unavoidably "a matter of chance".[94] After finding such a 'lead', the pharmacological effect might be further enhanced by the systematic variation of its molecular structure, i.e. by introducing or eliminating various chemical groups.

A third point is a complication of the last one. The chance factor may reside not only in the specific substance to be tried, but also in the particular quality of the test animals (both the rodents and the trypanosomes) on which it is to be tried. The discovery of the curative effect of atoxyl is a telling case in point. This arsenical organic compound, first obtained by the French chemist Béchamp in the early 1860s by heating arsenic acid with aniline, was marketed at the beginning of this century by the Vereinigte Chemische Werke Charlottenburg. The product was recommended as a tonic for anaemias and chloroses, and also for treating skin diseases. Probably because arsenic preparations had already been previously employed on sleeping sickness and other trypanosomal diseases (by David Livingstone and David Bruce), Ehrlich decided in 1903 to test atoxyl on his parasites of *Mal de Caderas*. He found no effect at all in his experiments conducted with Shiga and therefore discarded the substance. Luckily, the compound was not entirely lost from his chemotherapeutic research programme, because in 1905 Thomas and Breinl of the Liverpool School of Tropical Medicine reported that atoxyl was able to eliminate trypanosomes from the blood of infected rodents. As a consequence, Ehrlich turned his attention again to atoxyl and those arsenical compounds which could be obtained by systematic modification of its molecular structure (after Ehrlich had first corrected the alleged structural formula of this substance). We know that salvarsan was actually at the end of the long road of molecular modification starting with atoxyl ... Ehrlich concluded afterwards that he had missed the potential of atoxyl on the first occasion in 1903, because the trypanosomes on which he and Shiga had tried the substance happened to be (naturally) resistant. He observed that the often unknown natural variation in virulence and resistance among different strains of parasites and the variation in sensitivity to the chemicals among different test animals always constituted a major liability in doing chemotherapeutic research. To prevent confusion arising from contradictory results in diffe-

93 Parascandola and Jasensky (op. cit., note 53) point out that the receptor theory of drug action was also indepently arrived at by the English physiologist John Newport Langley.

94 Ehrlich, op. cit. (note 62), p. 235.

rent laboratories, Ehrlich even suggested that every strain of trypanosome employed should be given a kind of 'passport' - a profile of resistances against the main types of trypanocidal substances.[95]

The example of atoxyl also gives us an opportunity to raise a fourth critical point with regard to the standard view on Ehrlich's rational approach. Bruck compared Ehrlich to a marksman "whose eye has always just one particular aim in view", but the fact remains that the end result of his chemotherapeutic programme - an effective medicine against syphilis -, had not been his original aim when he started. In his address delivered at the opening of the Georg Speyer Haus, Ehrlich still proclaimed that "[o]ur highest aim must always be the conquest of sleeping sickness [..]".[96] Somewhere along the route Ehrlich changed his aim from the conquest of sleeping sickness to the conquest of syphilis. Why this shift? The answer can be found in the general movement of the medical field. In the years after 1905 the substance atoxyl attracted considerable interest from several investigators, not only for its possible curative effects on sleeping sickness and other trypanosomal diseases (it was tested in this respect by Robert Koch, among others, in a highly publicized African expedition), but also for its possible effects on diseases caused by spirochaetes such as syphilis. The link between trypanosomes and spirochaetes had been suggested by Fritz Schaudinn after the discovery in 1905 of *Spirochaeta pallida* as the probable causative agent of syphilis.[97] Never mind that Schaudinn's speculations on the relationship between trypanosomes and spirochaetes appear as completely unfounded by modern standards; his contemporaries needed just this pointer to try new applications of atoxyl. Some physicians, e.g. the dermatologist Oscar Lassar, tested the substance directly on syphilitic patients.[98] The Berlin bacteriologist Paul Uhlenhuth and his collaborators took a more indirect road via animal experiments. First they established the effect of atoxyl on 'chicken spirillosis' (better called chicken spirochaetosis[99]), then on mice and rats artificially infected with the spirochaetes causing

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95 Ehrlich, op. cit. (note 62), p. 342.

96 Cited in Lechevalier and Solotorovsky, op. cit. (note 32), p. 453. Ehrlich also headed the German delegation to the first Sleeping Sickness Conference held in London in June 1907. See M. Worboys, 'The Comparative History of Sleeping Sickness in East and Central Africa, 1900-1914', *History of Science* 32 (1994): 89-102, p. 97.

97 See Chapter III.

98 O. Lassar, 'Atoxyl bei Syphilis', *Berliner klinische Wochenschrift* 44 (1907): 684-87.

99 "The name 'spirilloses' which is often used as a synonym [for 'spirochaetoses'] is out of place, because it could lead to the assumption that the causative agents of these diseases [i.e. spirochaetoses] are related to the known bacterial species of spirilla. The latter microorganisms have however nothing to do with the spirochaetes"; W. Kolle and H. Hetsch, *Die Experimentelle Bakteriologie und die Infektionskrankheiten*, Berlin and Vienna (Urban & Schwarzenberg), 1922 (6th edition), Vol. II, p. 792. The confusing designation of 'spirilloses' is also found in Ehrlich's writings, and in earlier editions of this handbook.

European relapsing fever. Their next step was to test atoxyl on monkey syphilis, but monkeys proved rather intractable. Uhlenhuth finally settled on the use of the rabbit as a suitable test animal, after the Italian Bertarelli had demonstrated that syphilis could also be transmitted to this animal species. After Uhlenhuth had found that syphilitic rabbits could be completely cured by injections of atoxyl, he started clinical trials on human subjects.[100] In the year 1907 quite a few investigators in several countries reported on the curative effect of atoxyl on human syphilitics, some of them also urging caution in its application because it could lead to blindness.[101] Ehrlich's decision to switch from the conquest of sleeping sickness to the conquest of syphilis was thus in line with contemporary trends in the medical field. In other words: the scientific genius was not impervious to the 'epidemiology of intellectual contact'. One may also hold that Pickering's phrase 'opportunism in context' is applicable here.

The need for new test animals

The reorientation of Ehrlich's chemotherapeutic programme towards syphilis and other spirochaetoses required new species of test animals and new qualifications of the scientific manpower competent to deal with them. At first Ehrlich relied on the cooperation of his old classmate from the Breslau *Gymnasium*, Albert Neisser, who was then still in the Dutch Indies for his research on simian syphilis. In Batavia, Neisser and his collaborators tested numerous arsenical preparations sent by Ehrlich on monkeys and apes.[102] The dermatologist maintained a regular correspondence with Ehrlich on the results obtained. Although Neisser continued to cooperate after the completion of his (second) tropical expedition in November 1907, Ehrlich also mobilized other resources. In November 1908 he visited Paul Uhlenhuth in Berlin and was much impressed by the latter's ability to cure syphilitic rabbits and chicken suffering from fowl spirochaetosis with a single injection of atoxyl. Ehrlich asked Uhlenhuth to supply him with one syphilitic rabbit, which he would duly receive - but only after a few months' delay! (After Ehrlich's death, Uhlenhuth claimed priority for having laid the foundation for the chemotherapy of syphilis.[103]) Ehrlich also corresponded with his Japanese friend and former co-worker, Professor Kitasato, of the Tokyo Institute of

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100 P. Uhlenhuth, *Die Bedeutung des Tierexperiments für die Medizin, besonders für die Erforschung des Wesens, der Erscheinung und Bekämpfung der Seuchen*, Freiburg in Baden (Speyer & Kaerner), 1929, p. 24.

101 Cf. F. Moses, 'Der heutige Stand der Atoxylbehandlung bei Syphilis, unter Mitteilung eigener Beobachtungen', *Berliner klinische Wochenschrift* 44 (1907): 929-31.

102 C. Bruck, 'Albert Neisser', *Die Naturwissenschaften* 4 (1916): 609-17, p. 612; A. Neisser, 'Ueber die Verwendung des Arsacetins (Ehrlich) bei der Syphilisbehandlung', *Deutsche medizinische Wochenschrift* 34 (1908): 1500-1504.

103 On this priority claim see L. Lendle, 'Zur Geschichte der Begründung der Chemotherapie am Beginn des 20. Jahrhunderts', *Medizinhistorisches Journal* 4 (1969): 24-40.

Infectious Diseases, asking for an assistant experienced in transmitting spirochaetes to experimental animals. In the spring of 1909 Sahachiro Hata, who was thoroughly acquainted with rabbit syphilis, arrived at the Georg Speyer Haus in Frankfurt. Now Ehrlich had at his disposal a scientific assistant with the qualifications required to deal with the recently acquired test animals, even apart from the legendary 'oriental patience' which was useful in performing routine jobs. Hata was charged with testing a large series of compounds for their effects on relapsing fever, fowl spirochaetosis and rabbit syphilis. Before long, in June 1909, he found that the compound with the number 606 (that is, the sixth compound in the series commencing 600) or 3,3'-diamino-4,4'-dihydroxy arsenobenzene had excellent curative properties in relation to its toxicity. This was the substance that would later be called salvarsan or arsphenamine.

The chemical design of drugs

The above description shows that Ehrlich's chemotherapeutic programme, though conceived as a 'rational' undertaking, was nevertheless in its actual realization dependent on various contingent, opportunistic and circumstantial factors. We have not yet examined, however, that crucial part of the undertaking which lays most claim to being rational and purposive: the chemical design of new drugs. Granted that the finding of a promising 'lead molecule' (like atoxyl), as Ehrlich himself admitted, is largely "a matter of chance", could we not argue that the programme enters into its proper rational stage when it subsequently embarks on the *systematic* molecular variation of such a find? Let us therefore look somewhat more closely, without losing ourselves in the jungle of chemical details, at Ehrlich's strategy of molecular design.

Drug action, in Ehrlich's thought, was almost exclusively a matter of chemical affinities. The problem with most therapeutically active substances is that they have affinities not only to the cells of the parasites but also to those of the host organism. They are, in Ehrlich's terminology, not just *parasitropic* but also *organotropic*. Antibodies represented the exemplar of ideal medicines to him, because as 'magic bullets' they act only on the harmful parasites. For artificial medicines this ideal was unattainable, but it would be feasible - whenever a promising 'lead' was found - to attempt to enhance the degree of parasitotropism and to diminish the degree of organotropism by suitable chemical modification. In his lecture to the German Chemical Society Ehrlich formulated the task of chemotherapy as follows: "Wir müssen zielen lernen, chemisch zielen lernen!" (We must learn to shoot better by chemical means!).[104] For practical purposes it was important that the relationship between 'organotropism' and 'parasitotropism', theoretical notions which derived their meaning from Ehrlich's receptor theory, could be defined in operational terms as the so-called *therapeutic index*, or as the ratio between 'dosis curativa' and 'dosis tolerata'. The value of the latter

[104] Ehrlich, op. cit. (note 52), p. 22.

index could be determined empirically for every possible chemical compound by straightforward though cumbersome laboratory routines.

Ehrlich had some rules of thumb or heuristic principles to guide him in preparing structural variants of the given 'lead' molecule. Cumulative experience in his laboratories told him that the introduction of certain substituent groups would lower the toxicity of the compound for the host organism. Ehrlich also postulated that the trivalent arsenic radical in arsenical compounds, corresponding to an assumed *arsenoreceptor* in the parasite, was responsible for the trypanocidal effect. Atoxyl (i.e. the sodium salt of *p*-aminophenylarsonic acid) contained pentavalent arsenic and had no lethal effect on trypanosomes in the test tube. Ehrlich therefore concluded that it was converted into a trivalent arsenical compound after its injection into the host organism. In order to find more effective drugs, Ehrlich held, this reduction process should not be left to the host organism but had to be undertaken by the synthetic chemist. Ehrlich's strategy of molecular modification thus combined two lines of attack: enhancing the toxicity for the parasites through successive reduction of the arsenic residue of the atoxyl molecule and diminishing the toxicity for the host organism by introducing certain substituents into the amino group. This strategy of directed molecular modification generated promising trypanocidal and spirochaetocidal compounds like arsacetin, arsenophenylglycine ('number 418') and also salvarsan ('number 606').

To what extent, then, can Ehrlich's chemotherapeutic programme be considered a 'rational' and 'purposive' approach to the development of new medicines? We must resist the temptation to assign it these qualities merely on the grounds of its ultimate success. True, Ehrlich did have some rules and guidelines to go by, but they were not generalizations of wide applicability. They had also been gained in a long and arduous process of (much more blind) trial-and-error. Ehrlich did not possess powerful and selective heuristics indicating a direct path to ultimate success through the labyrinth of almost infinite possibilities. As a matter of fact, he had had several hundred derivatives of atoxyl synthesized and tested, before hitting on one substance with apparently miraculous properties. It was perhaps not so much Ehrlich's ingenious theoretical viewpoints, but more his use of 'brute force' - chemical and biological *Massenarbeit* on a large scale - which eventually led to success. At the start of his chemotherapeutic programme, Ehrlich himself made an illuminating comparison:

> "Surely there exist numerous problems in which one can, with particular ingenuity and modest material resources, reach important results, just as it is possible for a skilled craftsman to open a complicated lock by means of a piece of wire; but one cannot proceed in this way when [..] practical-therapeutic problems are at issue. Here the door is closed and blocked so firmly, that it can be opened only through violence, just as a fortress can no longer be conquered nowadays through betrayal and a small key but only by modern methods".[105]

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[105] Cited in W. Greiling, *Paul Ehrlich: Zijn leven en werk*, Leiden (Stafleu), 1955, p. 105.

I think these words are a fitting description of Ehrlich's own approach. From Ehrlich's time to the present, pharmacologists have repeatedly proclaimed that the era of 'rational drug design' was just about to begin, dismissing simultaneously all previous attempts as merely 'empirical' gropings in the dark.[106] This should certainly give us pause to question the analytical value of the labels 'rational' and 'empirical'. A useful antidote is also provided by Ehrlich's former co-worker, Carl Browning, who in 1955 observed:

> "A more rational, planned attack on the problem would require meanwhile [..] the cooperation of agents with powers like those of Clerk Maxwell's 'demons'." [107]

It is finally possible to criticize 'the myth of rational drug design' from still another angle. This ideal, as Rein Vos has pointed out, is predicated on a *hierarchical view* of science, according to which knowledge is developed at one privileged site (the laboratory) and subsequently 'applied' at other sites (the practices of clinicians, pharmacists, general practitioners, etcetera). An alternative, non-hierarchical (heterarchical) view holds that (medical) knowledge is created at different sites in mutual interaction with each other.[108]

The unexpected problems that afflicted the clinical introduction of salvarsan provide a graphic illustration of the limitations of the hierarchical view. We will turn to these in the next section.

5. The clinical introduction of Salvarsan

Back in 1908 Ehrlich had declared that after extensive animal experiments the final test of a newly found drug on man would be no more than 'die Probe aufs Exempel' (proving the sum). In March 1910, however, in a letter to an American friend, he complained bitterly that the difficulties of "the transferring of chemotherapy into practice" had thoroughly spoilt his frame of mind:

> "Beautiful though laboratory experiments are in themselves, it is difficult to transfer them into practice, since one finds only a few practitioners who do this properly and, as I readily admit, are really *able* to do it properly. Then disappointments rain down, and all incidents which occur during the trial of a new

[106] J. Parascandola, 'Carl Voegtlin and the "Arsenic Receptor" in Chemotherapy', *Journal for the History of Medicine and Allied Sciences* 32 (1977): 151-71, p. 154; A. Wacker, 'Entwicklungslinien chemotherapeutischer Forschung', *Die Naturwissenschaften* 53 (1966): 396-403, p. 402.

[107] C.H. Browning, op. cit. (note 45), p. 618.

[108] R. Vos, 'De mythe van rationeel geneesmiddelenontwerp', *Pharmaceutisch Weekblad* 127 (1992): 749-54.

chemotherapeutic drug and which of course cannot be avoided at all with *any* new drug, are blamed on the originator".[109]

Gone is Ehrlich's previous optimism on the simplicity of clinical trials; he now recognizes that during the introduction of new medicines various 'incidents' will *unavoidably* occur. For a clinical introduction to be successful, it is essential that those inevitable 'incidents' will be blamed on 'improper' use and not on the drug itself (or its originator). This requires, as Ehrlich was to find out the hard way, a careful 'stage-management' and a sustained and almost superhuman effort of vigilance and monitoring.

After Hata's discovery of the effectiveness of preparation 606 against rabbit syphilis in June 1909, Ehrlich had not immediately arranged for clinical trials to be held. Further animal experiments were performed to determine the most effective dosage and mode of application (subcutaneous, intramuscular, or intravenous). How best to solve the substance was also the object of investigations. Additional tests were taken to ensure that preparation 606 would not produce neurological disorders in experimental animals (as some of its precursors had done to Ehrlich's unlucky 'dancing mice'). By the fall of 1909 Ehrlich was ready to release samples of his precious substance to a few selected specialists for clinical trials. In his instructions to them he let it be known that several aspects of dosage and mode of administration were still open to experimentation. Ehrlich kept in close contact with them. Meanwhile, in the winter of 1909-1910, a small pilot plant for the production of '606' was installed at the Georg Speyer Haus in anticipation of commercial production by the Hoechst company. Special vacuum apparatus had to be designed because production of the highly oxidizable '606' required exclusion of air. The packing of the product in glass ampules, which had to be sealed tight, also demanded special care.

The results of the initial trials with '606' were reported by Ehrlich, Hata, and several clinicians in April 1910 at an international medical congress held in Wiesbaden. Although the drug did not work on advanced paralytic syphilis, in other cases it showed excellent results. On the authority of Professor Iversen of St. Petersburg, intravenous injection was recommended as the most apropriate mode of administration. Ehrlich's aim to obtain a cure with one shot only - his ideal of a '*therapia sterilisans magna*' - proved, however, elusive. In most cases, repeated administration of the substance was necessary to effect a cure. Luckily, the spirochaetes did not develop resistance against '606'.

The findings reported at the Wiesbaden congress found much resonance in the medical and lay press and led to a world-wide sensation. The result was that during the following months countless physicians and other visitors from all over the world would flock to and besiege the Georg Speyer Haus asking for Ehrlich's magic substance. At that time, however,

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[109] Letter of P. Ehrlich to Dr A. Meltzer, 9 March 1910, printed in C.E. Dolman, 'A fiftieth anniversary commemorative tribute to Paul Ehrlich, with two letters to American friends', *Clio Medica* 1 (1966): 223-34, p. 231.

Ehrlich was not intent on making it indiscriminately available to any physician who wanted to use it;in any case supplies were still too limited (though frantic efforts were made to step up production). At a meeting of the Association of German Naturalists and Physicians held in Königsberg on 20 September 1910, Ehrlich declared: "I considered it necessary, before I would be ready to hand the drug over to practice, that observations were available on 10.000-20.000 cases, in order that one would know exactly how large the risks are and under what circumstances they occur".[110] In keeping with his so-called "concentric" approach[111], Ehrlich decided to give the drug exclusively to qualified specialists with adequate laboratory and clinical facilities. Between June and December 1910 he provided, free of charge, 65.000 doses of the medicine.[112] Ehrlich himself kept track of and tabulated the mass of information obtained in the clinical trials. When untoward effects occurred, he reviewed all the details of the treatment procedure to pin down the probable cause. Ehrlich's approach to clinical trials was praised by Professor Heubner of Göttingen in 1911 as "an organizational - one might be tempted to say 'moral' - achievement as great as the discovery of the effective chemical structure was a scientific one".[113]

Speaking of Ehrlich's moral achievements, it will by now be clear that he was unable to keep the promise, stated in 1908, that his approach based on animal experiments would largely do away with the need to experiment on human patients. In a sense, the above mentioned "10.000-20.000 cases" on which observations had to be obtained before the drug would be released for general use can be considered as just so many human guinea pigs. Ehrlich found himself in conflict with the traditional principle in medical ethics of doing no harm. To relieve his conscience, he sought a different ethical principle by comparing chemotherapy to surgery:

> "[In surgery] as in chemotherapy we are dealing with instruments which may at times become dangerous! But surgery owes its triumphs to the principle that it does not shrink from a certain risk and that its chosen devise is not '*primum, ne noceas*' but '*primum, ut profiteatur*'. If we therefore have the conviction - based on animal experiments - that we are in the possession of a truly curative drug which can bring health to severely diseased patients, then it seems imperative to take a certain risk rather than to avoid it altogether and thwart success by leaving the patient ultimately to his own fate".[114]

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110 A. Neisser et al., 'Die Behandlung der Syphilis mit dem Ehrlichschen Präparat 606. Verhandlungen auf der 82. Versammlung Deutscher Naturforscher und Aerzte in Königsberg am 20. September 1910', *Deutsche medizinische Wochenschrift* 36 (1910): 1889-96, p. 1894.

111 Greiling, op. cit. (note 105), p. 163.

112 Marquardt, op. cit. (note 38), p. 90.

113 Cited in Marquardt, op. cit. (note 38), p. 91.

114 Cited in W. Kolle and H. Hetsch, *Die Experimentelle Bakteriologie und die Infektionskrankheiten*, Berlin and Vienna (Urban & Schwarzenberg), 1911, Vol. II, p. 668.

In December 1910 salvarsan - as was the trade-name chosen for the substance previously designated as 'Ehrlich-Hata 606' or simply '606' - was released for sale by the Hoechst company. The preparatory work to establish the precise action of the drug and its dosage, the hazards, the indications and the contra-indications had been completed, or so Ehrlich thought. The further vicisssitudes of the drug would be entrusted to Hoechst. Ehrlich already considered himself relieved from his scientific responsibility.[115] But things would run differently. The reason was that Ehrlich had required the Hoechst company to keep him personally informed about all the complications and incidents arising from the use of salvarsan. There would be no lack of such occurrences during the following years.

It was about this time that, according to one observer, "the high waves of initial enthusiasm" began to subside and "hypercritical objections and attacks lacking objectivity" set in.[116] In a letter to Simon Flexner of the Rockefeller Institute for Medical Research, dated 3 January 1911, Ehrlich once again complained about the widespread incompetence of physicians in handling his curative substance and also referred to the sometimes hostile atmosphere:

> "You won't believe with what clumsiness and with what deliberate and wilful enmity 606 frequently has been handled: the majority of dermatologists in particular have not shown up at all well. They wanted to do miracles with the drug, and demanded complete cure after a single small injection used in the most unsuitable way, and now people are yelling as if they had been swindled. Then they should turn to a magician rather than a scientist".[117] One might think that Ehrlich's indignation was hardly justified. Had not he himself aroused expectations by proclaiming the ideal of a 'therapia sterilisans magna'? Was it unreasonable for people to attribute magical properties to a drug which after all had been designated as a 'magic bullet'?

The preparation and application of the drug indeed offered great difficulties to most physicians. Ehrlich had in fact added to the confusion by encouraging the initially selected specialists to treat every aspect of the administration of the drug as open to experiment. As a result, at first almost every specialist had his own preferred solvent to get the substance into solution (or suspension, or emulsion) and applied his own favourite form of administration (subcutaneous, intramuscular, intravenous, oral, rectal).[118] Consensus on the best preparation and application emerged only slowly. Subcutaneous and intramuscular injection went into

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[115] Greiling, op. cit. (note 105), pp. 166-67.

[116] J. Jadasohn, 'Unsere Erfahrungen mit Salvarsan', *Deutsche medizinische Wochenschrift* 36 (1910), p. 2377.

[117] Letter of P. Ehrlich to Prof. Dr. S. Flexner, 3 January 1911, printed in C.E. Dolman, op. cit. (note 109), p. 233.

[118] See the diversity reported in 'Berliner Dermatologische Gesellschaft: Diskussion über Ehrlich-Hata-Behandlung', *Deutsche medizinische Wochenschrift* 36 (1910): 2175-76, 2271-72.

disuse because they were too painful to most patients or led to unpleasant effects like infiltrations and necroses. Intravenous injection of salvarsan in alkaline solution became the recommended mode of administration. Preparation of the solution, which had to be used directly, was however rather laborious, and many physicians shrinked from injection into the veins, which at that time was not yet a standard medical procedure performed with a simple standard piece of apparatus and familiar to all practitioners. In 1911, the Breslau dermatologist Albert Neisser declared that intravenous injection was after all "very convenient, easy and administered with the simplest apparatus imaginable; a rubber tube and a needle [..] if one masters the technique of needle insertion there are no disturbing after-effects such as infiltration, necroses and so on." "Indeed", he added, "when looking over the innumerable inventions of new apparatus for intravenous injections I cannot restrain a smile".[119]

He then went on to devote a long paragraph to a description of the technique used in Breslau, thus testifying that the procedure of intravenous injection was indeed all but familiar at the time.[120] The situation in the USA appears to have been even worse. Patricia Ward notes that American physicians generally tended to equate intravenous injection with surgery and thus avoided using it. Those who dared to perform it, usually first made an incision to find the vein rather than injecting directly. During the course of treatment they so frequently caused "multiple punctures, incisions and infiltrated arms", that patients were sometimes discouraged to finish the cure.[121]

Widespread adoption of salvarsan among ordinary physicians was further hampered by the need to check the results of the treatment through the Wassermann reaction. Only briefly before, this serodiagnostic test had become accepted as a necessary instrument to regulate the intensity and duration of mercurial treatment[122], and now it was to fulfil a similar role in

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119 A. Neisser, 'On modern syphilotherapy with particular reference to salvarsan', *Bulletin of the History of Medicine* 16 (1944): 469-570, pp. 490-91 (translation of A. Neisser, *Ueber Moderne Syphilistherapie mit Besonderer Berücksichtigung des Salvarsans*, without place, 1911). In a letter to the Hoechst company, dated 30 November 1911, Ehrlich drew attention to a simple procedure for intravenous injection elaborated by one Dr. Fehde from Berlin, which was "much more convenient for the practical physician, because it avoids the many complicated apparatuses of injection"; see Farbwerke Hoechst Aktiengesellschaft, *Um die Zubereitung des Salvarsans* (Dokumente aus Hoechster Archiven Nr. 19), 1966, p. 64.

120 Patricia Spain Ward quotes a long description of the laborious technique used in the American Naval Hospital in her article 'The American reception of Salvarsan', *Journal of the History of Medicine and Allied Sciences* 36 (1981): 44-62, pp. 52-53. Following a suggestion of Harry Collins, one could analyze how skills such as intravenous injection become part of the *taken-for-granted* repertoire of a certain community (e.g. physicians) or society by looking at the historical evolution of (the length of) explicit instructions for the use of particular instruments or machines. Collins demonstrates this for slot machines in his *Artificial Experts*, Cambridge, MA (MIT Press), 1990, pp. 106-107.

121 Ward, op. cit. (note 120), p. 53.

122 See Chapter IV.

salvarsan treatment. The execution of this difficult and complicated test was however the prerogative of specially trained serologists. In October 1910, Ehrlich had expressed the hopeful expectation that "a method of the Wassermann reaction would be found, that can also be executed by the practical physician"[123], but this would remain an idle hope for the following decades.[124]

Even when properly administered, salvarsan could have all kinds of unpleasant side-effects: headaches, chills, fever, itching, nausea, vomiting, etcetera. Particularly worrisome for Ehrlich were some indirect side-effects on the auditory, optic, oculomotor and facial nerves. For most specialists these were peculiar relapses of syphilis (socalled '*Neurorezidive*'), somehow evoked or stimulated by the use of salvarsan; others, however, interpreted these manifestations as toxic phenomena caused by the drug itself.[125] Albert Neisser staked his full authority to acquit the medicine of this accusation; in his opinion these conditions were "unmasked but not caused by '606'".[126] In recent years, he stated, the number of syphilitics undergoing treatment had increased, and doctors had also become more attentive to the occurrence of such complications. At any rate, Neisser held, it should be possible to eliminate these same complications by continuing the vigorous treatment with salvarsan. Equally troublesome for Ehrlich was the occurrence of local inflammatory processes (so-called Jarisch-Herxheimer reactions) after application of salvarsan. Ehrlich explained these as resulting from an augmented secretion of toxins from spirochaetes stimulated by the therapeutic agent, but even if this explanation were correct it would not be completely reassuring.

In the summer of 1911 a number of fatalities resulting from *encephalitis haemorrhagica* or 'oedema of the brain' (*Hirnschwellung*) occurred after administration of salvarsan. The magic drug seemed to be incontestably implicated, but in the end Ehrlich and other investigators were able to shift the blame to something else. To prepare a salvarsan solution it was necessary to use distilled, sterile water. Now the unexpected and rather shocking discovery was made that the distilled water supplied by the pharmacies was far from sterile; on the

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[123] Neisser et al., op. cit. (note 110), p. 1894.

[124] Ilana Löwy describes the failed attempt by the Rockefeller Institute bacteriologist Hideyo Noguchi to introduce a simplified version of the Wassermann reaction usable by ordinary clinicians. The 'Noguchi method' floundered on the opposition of his serological colleagues who argued that the Wassermann reaction was too delicate to permit its use by non-specialists. See I. Löwy, 'Testing for a sexually transmissible disease, 1907-1970: the history of the Wassermann reaction', in V. Berridge and P. Strong (eds,), *AIDS and Contemporary History*, Cambridge (Cambridge University Press), 1993, 74-92, pp. 78-79.

[125] L. Michaelis, 'Die Ehrlich-Hata-Behandlung in der inneren Medizin', *Deutsche medizinische Wochenschrift* 36 (1910): 2278-84, p. 2281.

[126] Neisser, op. cit. (note 119), p. 486.

contrary, it could often be shown to contain millions of germs per cubic centimetre.[127] The unusually hot summer of 1911 appeared to have been conducive to their multiplication. So it was not the drug but this so-called *Wasserfehler* (water error), due to negligent pharmacists' practices, that had to be blamed for the several lethal cases of oedema of the brain. Thereupon, the Hoechst company constructed and sold special equipment for the preparation of distilled water. (From 1922 onward, the firm sold ampules containing re-distilled, sterile water together with ampules containing (neo)salvarsan.)

In view of all the incidents and complications attending the application of salvarsan, Ehrlich thought it still premature in December 1911 to actively promote its use among general practitioners, as was the commercial aim of the Hoechst company at that time. The first priority of the firm was to simplify the preparation and administration of the drug, in order to facilitate its adoption by ordinary physicians. In Ehrlich's judgement, however, it was of the greatest interest to concentrate first of all on clinical specialists and to ensure that in the clinics the best and most optimal treatment would be worked out. In a letter to Hoechst he explained why he considered the primary focus on general practitioners not a prudent strategy:

> "These ordinary physicians, the *Wald- und Wiesenarzt* [hardly translatable, something like: the physicans of provincial backwaters] will spoil our statistics by their incompetent treatment. It will rain abscesses, necroses, and neurorecidives [*Neurorezidive*], thus causing a vicious circle which will surely be exploited by the terrorists".[128]

Although Ehrlich did not agree with the commercial priorities of Hoechst, he worked hard on the development of a new arsenical derivative, preparation number 914, which was introduced in June 1912 under the trade-name of *neo-salvarsan*. Because it dissolved in neutral solution, it was less diffcult to handle.

Ehrlich was not only confronted with incidents stemming from lack of competence or unexpected complications arising from the use of salvarsan, he also had to stand up to other difficulties. At times the controversy over (neo-)salvarsan took on the character of a truly political struggle, merging with other themes like nationalism, antisemitism, class struggle, complaints about profiteering, etcetera. It must be admitted that the proponents of salvarsan did not always choose the most elegant methods in dealing with their opponents. Persistent critics were sometimes denied access to publication outlets. When Ehrlich's friends took the unusual step of having the Berlin police doctor and opponent of salvarsan, Dr. Dreuw,

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[127] Letter of P. Ehrlich to the Pharmaceutical Division of Hoechst Dyeworks, dated 15 December 1911, in Farbwerke Hoechst Aktiengesellschaft, op. cit. (note 119), p. 68-69; Th. Müller, *Ueber den Bakteriengehalt des in den Apotheken erhältlichen destillierten Wassers*, Munich (J. F. Lehmanns Verlag), 1912.

[128] Letter of P. Ehrlich to the Pharamaceutical Division of the Hoechst Dyeworks, dated 15 December 1911, in Farbwerke Hoechst Aktiengesellschaft, op. cit (note 119), pp. 66-67.

dismissed from his position, they created an intransigent enemy who in the following years would not flinch from slander. In the beginning of 1914 he presented a memorandum to the *Reichsgesundheitsamt* (Imperial Health Office) in which he demanded a prohibition of salvarsan on the grounds that it had been brought into commerce without sufficient prior testing and that it represented a severe danger to the lives and health of numerous patients.[129] Forced to debate the question, the German *Reichstag* endorsed the drug as a valuable enrichment of the therapeutic arsenal. That same year Ehrlich had to appear as a defence witness before a Frankfurt court to counter accusations that local prostitutes had been forced by the city hospital to undergo salvarsan treatment. The outbreak of the First World War brought an end to these political and legal quarrels.

All the difficulties surrounding the clinical introduction of salvarsan had taken a heavy toll in terms of Ehrlich's health. He died on 20 August 1915 at the age of 61.

6. Conclusions

After the preceding historical description of Ehrlich's laboratory practice of experimental therapeutics and of the clinical introduction of salvarsan, it is time to reconsider the analytical concerns that were raised in the introductory section.

The first issue was whether the work of a scientific genius like Ehrlich would be amenable to a sociological analysis along constructivist lines. The answer is in the affirmative. In the preceding sections I have shown that most of Ehrlich's insights and discoveries were not arrived at in complete independence. He borrowed extensively from others: material, conceptual and organizational resources from the German chemical industry, and suitable 'animal models' such as trypanosome-infected rodents or syphilitic rabbits from other researchers. Sometimes Ehrlich was also susceptible to fashionable trends in the 'epidemiology of intellectual contact', as when he decided to switch from the conquest of sleeping sickness to the conquest of syphilis. Yet all this hardly affects his stature as a scientific genius, it only shifts the touch of genius to another dimension. Ehrlich's unique contribution was not so much to be found in his superior insights as in his determination to combine synthetic chemistry with extensive animal experimentation, or chemical with biological 'Massenarbeit', which eventually enabled him to open with 'brute force' the door to the chemotherapy of syphilis which remained closed to others. Already in his early student years he had laid the foundation for his later achievements by acquiring knowledge and expertise in the field of synthetic dyes along with his training as a physician. In due course the German chemical industry would 're-borrow' and adopt Ehrlich's *construction machinery* for doing chemotherapeutic research.

[129] Dr. Gennerich, 'Zur Salvarsanfrage', *Die Naturwissenschaften* 2 (1914): 263-67.

A second issue was the significance to be attributed to the traffic of substances, materials, and test animals - or to 'resource-relationships' in the widest sense - as against the traffic of ideas or 'intellectual interaction' in a more narrow sense. The attentive reader will undoubtedly have noticed that my description of Ehrlich's practice has a definite 'materialist' (even 'raw-materialist') flavour to it in that it grants a prominent role to the raw materials with which and on which he had to work. Whereas Knorr-Cetina has stated that "'conceptual interaction' is not merely 'conceptual'", in this particular case it could be asserted with equal justification that the exchange of materials was not a purely material transaction. Ehrlich initially used the products of the dye industry for selective staining, but he thereby became infected with concepts and ways of thinking prevalent in synthetic dyestuffs chemistry. Later on he also borrowed organizational models from the industry. So with the transmission of materials went the transmission of conceptual and organizational resources.[130] In the end, the practice of experimental therapeutics could be conducted only in intimate symbiosis with the chemical industry.

A 'materialist' approach focusing on science-as-practice rather than science-as-knowledge would also entail the recognition that scientific practices are partly *constrained*, though not determined, by the nature of the materials employed and the objects or animals (including humans) worked upon.[131] We have seen that Ehrlich was unable in 1891 to pursue his malaria studies due to the absence of an experimental animal on which malaria parasites could be transmitted. Only after 1900, when an excellent experimental system in the form of trypanosome-infected rodents became available, could his chemotherapeutic research programme be launched. Even with such an ideal experimental system in place, the special characteristics of the particular experimental animals employed would still constitute a major liability for researchers, as Ehrlich was to find out when he and Shiga missed the trypanocidal effect of atoxyl in 1903 because - as he explained afterwards - his trypanosome strains happened to be naturally resistant to that substance. The reorientation of Ehrlich's chemotherapeutic programme to syphilis created a new need for suitable experimental animals. At first, the hundreds of monkeys and apes on which Albert Neisser experimented in the Dutch East Indies could be used for that purpose. Primates, however, were not the most ideal experimental animals for Ehrlich's programme. Only later, when a smaller and less refractory test animal, the rabbit, became available, could he fully deploy his characteristic strategy of biological 'Massenarbeit' in combination with synthetic chemistry. The example of Ehr-

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130 Nelly Oudshoorn characterizes the role of research materials by the metaphor of 'carriers': "With this metaphor, we can understand how research materials mediate, both in establishing relationships between actors, and in the selection of knowledge claims". See N. Oudshoorn, 'On the Making of Sex Hormones: Research Materials and the Production of Knowledge', *Social Studies of Science* 20 (1990): 5-33, p. 25.

131 Actually, Pickering objects strongly to the notion of 'constraint'. I will address the issue later on.

lich's programme thus illustrates the dependence of scientific practices on the special properties of the materials, objects and animals with which they have to deal.

Another illustration of the relevance of 'materials dependency' for scientific practices is provided by the development of the Wassermann reaction, as described in chapter V. Let us briefly summarize the main points here. The disappointing initial results obtained with antibody determination could be partly explained, as the serologist Julius Citron pointed out, by the peculiar nature of the 'material' from the Breslau dermatological clinic, i.e. by the fact that Neisser's syphilitic patients had been heavily treated with mercury. If only the choice of 'patient material' had been a little bit more fortunate, Citron implied, the results would have looked more promising from the start. The story of the Wassermann reaction also illustrates that the 'field of resources' is not static and that advantageous positions in such a field may be only temporary. Initially, the spirochaete-swarming livers of stillborn luetic babies - "a much sought-after article", according to one investigator[132] - constituted the source of 'antigen' for Wassermann, Neisser, and Bruck. They had ample access to this resource thanks to a large network of contacts (in their first research paper on the serodiagnosis of syphilis they expressed their acknowledgements to no less than *thirty-six* persons who had provided them with material). When later on extracts from normal livers turned out to be equally usable (it is significant that this was found out by investigators who had a much less privileged access to syphilitic livers!), the value of the syphilitic resource rapidly diminished. Likewise, Neisser's artificially infected primates in Breslau lost their value as suppliers of strong luetic sera when the serodiagnosis switched from antigen to antibody determination. In science as in economic life the value of given resources may be highly unstable.

It may be that the above deliberations on the 'materials dependency' of scientific practices grant too much independent force to the material world, thus crossing the thin line separating a 'materialistic' constructivism from outright realism. Here we deal with a third issue which was briefly alluded to in the introductory section. From a constructivist point of view, the notion of research materials *constraining* scientific practices appears to be unacceptable; at the most, constructivists are ready to admit the occurrence of 'resistances' in Pickering's sense.[133] It must be conceded that all the above-mentioned material 'constraints' (or 'resistances') were encountered in socially situated scientific practices and could not possibly have been established outside such practices; the notion of constraint should there-

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132 H. Beitzke, 'Ueber Spirochaete pallida bei angeborener Syphilis', *Berliner klinische Wochenschrift* 43 (1906): 781-84, p. 781.

133 See Knorr-Cetina: "You can get *resistances* in the laboratory; but in order for these resistances to make sense, they have to be interpreted. The very moment you interpret them, you enter the realm of the social world [..]"; in W. Callebaut, *Taking the Naturalistic Turn or How Real Philosophy of Science is Done*, Chicago and London (The University of Chicago Press), 1993, p. 185. In contrast to Knorr-Cetina and Pickering, Jan Golinski opts for an explicit recognition of 'constraints' in the sociological study of scientific practice, see J. Golinski, 'The Theory of Practice and the Practice of Theory: Sociological Approaches in the History of Science', *Isis* 81 (1990): 492-505.

fore not be treated "as somehow structuring and thus explaining the flow of practice from without".[134] This issue also has a bearing on the constructivist view of the laboratory as a reconfiguration of the natural and social order. The power of the laboratory, according to Knorr-Cetina, resides precisely in the "enculturation of natural objects". She thus emphasizes the *malleability* of natural objects. A laboratory science does not need to put up with the object *as it is*; nor does it need to accomodate the natural object *where it is*, anchored in a natural environment. Natural objects are "brought home" and subjected to the local conditions of the laboratory. It is from this reconfiguration that epistemic effects are derived.[135] This view finds support in contemporary laboratory studies, which demonstrate for instance how drastically laboratory rats and mice have been transformed by breeding programmes and the like into 'epistemic objects' bearing no longer any comparison to their wild and domestic counterparts despite the common name.[136] In Ehrlich's time, this transformation process had of course advanced much less far. Ehrlich still had to put up with 'natural' variations in virulence, resistance and sensitivity among his experimental organisms; nowadays, such variations would have been largely excluded from the outset by using specially prepared and selected standard organisms. This does not mean that the phrase 'reconfiguration' is only applicable to present-day laboratories and not to Ehrlich's institutes. For one thing, exotic diseases like *Mal de Caderas* and sleeping sickness, or rather their agents, had been literally 'brought home' from the Tropics to Ehrlich's Institute in Frankfurt, and incorporated into new host organisms: white mice. Nor did Ehrlich and his co-workers leave their experimental organisms in the state they found (or better: received) them; they raised, for example, special strains of trypanosomes with various profiles of resistances on which new chemical substances could be tried. Ehrlich even thought about introducing some kind of 'passport' to standardize the quality of the strains employed in different laboratories. Still, despite all these reconfiguration efforts, one would feel the need to counterbalance Knorr-Cetina's stress on the *malleability* of natural objects with a similar emphasis on their *refractoriness*. [137] Only in that way can scientific practice be understood as a dialectic between resistance and accomodation.

The fourth and final issue to be discussed here is how the difficulties surrounding the clinical introduction of salvarsan should be interpreted from a constructivist perspective. If the power of the laboratory resides in the enculturation of natural objects, this is also the

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[134] Pickering, op. cit. (note 9), p. 584, note 35. In Pickering's view, "constraints are as emergent as anything else".

[135] Knorr-Cetina, op. cit. (note 8), p. 117-18.

[136] Amann, op. cit. (note 13).

[137] In her 1983 review of laboratory studies Knorr-Cetina herself noted: "Variability caused by raw materials is a dreaded source of nuisance for the scientists". See Knorr-Cetina, op. cit. (note 5), p. 125. There would be no reason to fear this variability if raw materials were perfectly malleable.

source of its limitations. Ehrlich had to find out, to his own surprise and chagrin, that the transfer of laboratory findings to the outside world was a far from simple task. Knorr-Cetina's view that such a transfer requires hard *work* and also social *intervention* has been amply confirmed. But let us look more closely at the nature of the problem and the types of work and intervention used to deal with it. Laboratories do not generally leave the objects as they are found in their natural environment; they are 'brought home' and subjected to the local conditions of the laboratory setting. In this particular case the 'object', human syphilis, was 'brought home' by having it transmitted to rabbits. It is evident, however, that by this 'translation' the very object changes its character. Rabbit syphilis is not the same as human syphilis. Only a few of the manifestations characteristic of the human affection are also found in the artificially induced disease of the rabbits (the main reason for considering the latter condition a form of syphilis at all is that it can in turn be transmitted to apes and monkeys, which then show many more symptoms similar to those of human syphilitics). Translation, as Latour would say, involves treating situations that are not equivalent as if they were equivalent. A substance that is effective for rabbit syphilis would not automatically also be effective for human syphilis. Traditionally, the problem has been formulated as turning on the representativeness and reliability of the 'animal model' for extrapolating the results obtained in animal studies to the human counterpart. A recent publication concludes that such studies are little more than heuristics, based on weak analogies.[138] The lamentation uttered in 1929 by the Dutch gynaecologist J.A. van Dongen with regard to the results of female sex hormone therapy must sound familiar to many laboratory and clinical researchers: "the route from the white mouse in the laboratory to homo sapiens in the consulting-room is a long route that is not yet bridged".[139] Ehrlich was also to find out that the 'final test' of salvarsan on man was more than just 'proving the sum'.

In the constructivist literature, at least two strategies have been distinguished for dealing with the transfer of a laboratory object to the 'normal world': adaptation of the 'normal world' to laboratory conditions (Latour's option) and 'normalization' of the object (Knorr-Cetina et al.). The Dutch philosopher Gerard de Vries has offered a Latourian analysis of the clinical introduction of salvarsan, the upshot of which is summarized in the following passage:

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[138] H. LaFollette and N. Shanks, 'Animal models in biomedical research: some epistemological worries', *Public Affairs Quarterly* 7 (1993): 113-30; David Badcott, in his paper 'Predictive value of animal studies in drug development: some initial philosophical observations', presented at the World Congress on Medicine and Philosophy in Paris on 1 June 1994, offers a less negative appraisal of the predictive value of animal studies.

[139] Cited in N. Oudshoorn, 'United We Stand: The Pharmaceutical Industry, Laboratory, and Clinic in the Development of Sex Hormones into Scientific Drugs, 1920-1940', *Science, Technology & Human Values* 18 (1993): 5-24, p. 13.

> "The successes from the laboratory become usable in clinical practice only after the latter has been reorganized and become similar in certain respects to the laboratory: physicians have to learn to comply with the instructions just like Ehrlich's assistants; the selection of patients has to proceed in the same cautious manner in which test animals in the laboratory are chosen; etcetera. Without Ehrlich's extensive meddling with medical practice *outside* the laboratory, Salvarsan would never have become an effective medicine. Only guinea pigs and rabbits would then have benefited from his discovery".[140]

If this Latourian-style analysis makes one thing clear, it is, in my view, that Latour's own solution, i.e. that laboratory conditions are simply transplanted to the outside world, cannot be the full answer to the fruitful question that he himself formulated with so much insistence: how does the science get out of the laboratory? As a matter of fact, the circumstances of the laboratory could not simply be extended to the hospital and to the wider society. There is a crucial difference between giving instructions to subordinate assistants and to independent physicians (even apart from the fact that the 'instructions' to both groups widely differed in substance); nor can the selection of patients be equated with the choice of experimental animals. So 'extension' in the strict sense did not travel very far in this particular case. It is nonetheless true that the success of salvarsan required Ehrlich's active intervention with medical practice outside his laboratory, but my point is that his meddlings cannot be fully understood as attempts to extend the conditions of his laboratory.

With a lot of charity, we might perhaps recognize the 'education' of physicians into the proper handling of intravenous injection which proved necessary to foster the spread of salvarsan as an example of adapting the 'normal world' to laboratory conditions. The development of salvarsan into neo-salvarsan, which simplified the preparation and application of the substance, by contrast, can be considered as a 'normalization' of the object. The successful introduction of salvarsan also required social intervention. Ehrlich's "concentric" strategy of first approaching and convincing qualified specialists with laboratory facilities and then ordinary physicians was a clever form of 'stage-management'. Legal and political battles had to be fought to neutralize tenacious opposition. Another salient aspect of the clinical introduction of salvarsan also needs to be stressed. It is that 'experimentation' was not yet concluded when the product left the laboratory and entered the stage of clinical trials (we have seen that Ehrlich explicitly instructed the first specialists that most aspects of preparation and administration of the drug were still open to experimentation) or even when it entered commercial production.[141] Theoretically, this means that the distance between laboratory and 'normal world' can also be reduced from a different angle: rather than normalizing laboratory objects or extending laboratory conditions to the 'normal world', the latter is itself turned into a large-scale laboratory or field of experimentation. Such "implicit experiments", although

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[140] G. de Vries, *De Ontwikkeling van Wetenschap*, Groningen (Wolters-Noordhoff), 1995 (third, revised and extended edition), p. 167.

[141] Compare Oudshoorn: "[..] the development of a drug does not stop the moment it appears on the market - that is just the beginning". See Oudshoorn, op. cit. (note 139), p. 20.

mostly unacknowledged, are far from exceptional during the introduction and implementation of new technologies.[142] Under 'normal' conditions, as Bettina Heintz has observed, Pickering's 'resistances' take the form of incidents and accidents.[143] (We have seen that in 1910 Ehrlich expressed the view that various 'incidents' will indeed unavoidably occur during the introduction of new medicines.) The risks of experimental failure are thus shifted toward society and become an integral part of the development of new drugs and new technologies in general. Perhaps this is the inevitable price we have to pay for the benefits they also confer.

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[142] W. Krohn and J. Weyer, 'Gesellschaft als Labor: Die Erzeugung sozialer Risiken durch experimentelle Forschung', *Soziale Welt* 40 (1989): 349-73 (English version: 'Society as a Laboratory: the Social Risks of Experimental Research', *Science and Public Policy*, June 1994, 173-182). Consider also the suggestion by Thomas Pynchon: "Suppose we considered the war itself as a *laboratory*?", which is taken up in A. Pickering, 'Cyborg History and the World War II Regime', *Perspectives on Science* 3 (1995); 1-48, esp. on pp. 6 ff.

[143] B. Heintz, 'Wissenschaft im Kontext: Neuere Entwicklungstendenzen der Wissenschaftssoziologie', *Kölner Zeitschrift für Soziologie und Sozialpsychologie* 45 (1993): 528-52, pp. 545-46, note 34.

CHAPTER VIII

RECONSTRUCTING THE 'SEROLOGICAL' THOUGHT STYLE

In Chapter V I observed that Fleck was unable to construe the successful development of the Wassermann reaction into a clinically usable test for detecting syphilis as a clear instance of a so-called 'stylized thought constraint'. At any rate, the entire idea of a thought style exercising constraining power over individuals is rejected by modern constructivists on finitist grounds. This does not exclude the possibility that thought styles may constitute a distinct class of phenomena in their own right, worthy of systematic exploration. It is my intention to show in this chapter that Fleck's so-called 'serological' thought style, despite its apparently superfluous role in his explanatory scheme, can indeed be suitably re-defined and extended in such a way as to meet Jonathan Harwood's requirements for the comparative use of style concepts.[1] Several of the materials necessary for this reconstructive exercise have already been supplied in earlier chapters. After having completed this reconstruction, I will finally re-examine the question whether, and if so in what sense, a thought style can have a constraining power over scientific investigators.

Fleck's description of the serological thought style

Fleck introduced his readers to the serological thought style by presenting the first chapter of Dr Julius Citron's textbook, *Die Methoden der Immunodiagnostik und Immunotherapie* (1910 edition), which had the status of a 'catechism' (55-59/74-79). From this source he gleaned the following list of style elements (I have changed the arrangement and formulation):

(1) (serological) specificity: the reaction between antigen and antibody is specific;
(2) systematic clinical and laboratory observation constitutes the foundation of 'diagnosis' (i.e. the fitting in of a result into a system of distinct disease entities);
(3) the battle metaphor (*Kampfgedanke*): an infectious disease is seen as a conflict between a micro-organism and a host; the old proto-idea of a 'demon' has transformed itself into the modern notion of an aetiological 'agent';
(4) a rigid distinction and opposition between cellular and humoral immunity;
(5) a strong emphasis on the methodical necessity of 'controls';

1 See Chapter II. The following account draws heavily on an earlier article on this subject. See H. van den Belt and B. Gremmen, 'Specificity in the Era of Koch and Ehrlich: a Generalized Interpretation of Ludwik Fleck's "Serological" Thought Style', *Studies in History and Philosophy of Science* 21 (1990): 463-79. In this article we reconstructed Fleck's 'serological' thought style as a specimen of the style of 'specificity'. What is new in this chapter is the contrasting of the 'specificity' (or 'pluralist') style with an alternative, the so-called 'unitarian' style, thus allowing a more complete stylistic analysis in comparative terms.

(6) the 'chemical delusion' (*chemischer Wahn*): toxins, antibodies and complement are seen as well-defined chemical substances and not simply as particular serum effects.

Before turning to a systematic analysis of the 'serological' thought style, I must briefly comment on some of Fleck's formulations which may strike the reader as unduly partisan and biased. How can an analyst who advocates a comparative epistemology taking "a less egocentric, more general point of view" (22/34) use such a dismissive description as "chemical delusion" to refer to certain views of early 20th-century immunology? Fleck's assertion that he has been able to distil several style components from the first chapter of Citron's textbook by asking what elements it contained "that cannot be justified" (59/79) only serves to reinforce our suspicions. As a standard of comparison he used what he called "new views", which in his opinion belonged to the future but which he held to be already discernable in the mid-1930s. This strategy makes indeed a mockery of Fleck's advocacy of a non-egocentric comparative epistemology and violates the modern constructivist principles of symmetry and impartiality.

It appears that Fleck, from the vantage point of the alleged wisdom of the future, looked down contemptuously on a number of style elements as if they represented completely obsolete views. The irony is that the modern reader will not always share Fleck's appraisal. In present-day views toxins and antibodies are really well-defined chemical substances; we will therefore see no reason to speak of a 'chemical delusion'.[2] Fleck himself preferred to attribute different serum effects to physico-chemical and colloidal changes in complex solutions rather than to reduce them to reactions between chemically defined substances. Just as Fleck looked down at Ehrlich's era as "die Epoche des chemischen Wahnes" (the era of chemical delusion), later writers on the history of science have passed an equally unfavourable judgement on the kind of 'colloidal' views Fleck adhered to and have even used the expression "dark age of biocolloidology".[3] In both cases we have 'Whig history' before us.

Fleck also took an 'egocentric' stand when dealing with the style element of (serological) specificity. He commented very briefly: "Ein ausgesprochen mystischer Begriff!" (An extremely mystical notion!) (63/84).[4] Previously, Fleck had observed that certain words are not logically examined but immediately make either enemies or friends, one of his examples being the word 'specificity' (43/59). So there is no doubt that Fleck was an enemy of 'speci-

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2 Complement, however, has evolved from a fairly simple serum factor in the beginning of this century into a, by now, very complex 'complement system', comprising several substances functioning in different 'pathways'.

3 M. Florkin, *A History of Biochemistry, Parts I and II (Comprehensive Biochemistry, Vol. 30)*, Amsterdam and New York (Elsevier Publishing Company), 1972, pp. 279-283.

4 As before with "die Epoche des chemischen Wahnes" I cite from the German original, because the characteristic shade of meaning and tenor of Fleck's formulations have not been preserved in the English translation.

ficity'. His hostility to this notion can be related to his unfavourable opinion on the famous *key-and-lock* image that Paul Ehrlich had borrowed from Emil Fischer to visualize the specific reaction between antigen and antibody. In Fleck's judgement this particular image had had an excessive influence on the whole of immunological thinking (117/155).

Although it is somewhat disheartening to see that Fleck did not follow his own precepts for a "less egocentric" epistemology, his deficient procedure also contains some hints towards a truly comparative analysis of thought styles. In order to achieve a description of the 'serological' thought style he identified elements from Citron's text "that cannot be justified" by taking the "new views" to which he himself subscribed as a criterion. However wrongheaded and whiggish, this strategy has at least the merit of demonstrating, if only implicitly, that characterization of thought styles can be done in comparative terms only. The trouble is that Fleck did not recognize his own views as exemplifying an alternative thought style which could be contrasted with the 'serological' style he was describing. Our aim must therefore be to find some "less egocentric, more general point of view" from which both Fleck's and the older serological way of thinking can be interpreted as instances of different styles.

Looking for the unity in the diversity

If we look again at the set of style elements that Fleck gathered together, it seems hardly feasible to explicate the underlying unity of this set. Do these various elements really add up to a distinct style? Fleck himself does not make clear in what respect his so-called 'serological' thought style is more than the sum of its parts. His list appears to be a congeries of disparate elements. Nevertheless, upon examination it turns out to possess much more unity and coherence than one would have expected at first sight.

The hidden unity in the collection of style elements can be expressed in one single keyword: *specificity*. This term need not be understood in the limited sense of serological specificity (the first style element); its meaning can be taken much more broadly. August Wassermann was alluding to such a broad meaning in his lecture 'Ueber den Einfluss des Spezifizitätsbegriffes auf die moderne Medizin' (On the impact of the concept of specificity in modern medicine), which he presented in 1910 at the 82nd meeting of the *Verein deutscher Naturforscher und Aerzte* held at Königsberg.[5] Each fertile period in the history of medicine has, according to Wassermann, its own '*Grundidee*' (basic idea), which, like a stone falling in a pond, produces ever wider circles. In Virchow's time, for instance, the basic idea was the notion of the cell. "But if we now ask ourselves what the basic idea for the entire modern trend in medicine would be, then the acute observer will have no doubt that this question should be answered with one word: specificity".[6] Specificity is the '*Grundidee*' of medicine

[5] A. Wassermann, 'Ueber den Einfluss des Spezifizitätsbegriffes auf die moderne Medizin', *Deutsche medizinische Wochenschrift* 36 (1910): 1860-63.

[6] Ibid., p. 1860.

in the era of Robert Koch and Paul Ehrlich. Serological specificity, but also Koch's postulates and Ehrlich's chemotherapy, are all placed under Wassermann's broad umbrella. Specificity in this broad sense might be an exemplary expression of what Fleck called a "stylistic bond" existing "between many, if not all, concepts of a period" (9/15). We must therefore have a closer look at the ever wider circles our basic idea of specificity produced in different parts of medicine.

In *nosology* we can recognize our basic idea in the *ontological conception* of disease. It is by no means a coincidence that in the second characteristic of the serological thought style 'diagnosis' is described as "the fitting in of a result into a system of distinct disease entities"; and, adds Fleck, "this assumes that such entities actually exist" (64/84). Opposed to the ontological conception stands what is usually called the *physiological conception* of disease. Traditional Hippocratic medicine was based on this conception; it understood illness as a disturbance of the proper balance of the bodily humours and had no interest in an abstract classification system of diseases. In the 19th century the physiological conception was defended by the French physician Broussais (1772-1838), who "fanatically opposed everything that savoured of the idea of *spécificité*" and held the view that symptoms follow no rules.[7] His great opponent Bretonneau (1778-1862) was a representative of the ontological conception. Two centuries before, this view had already found an early advocate in Thomas Sydenham (1624-1689). Although the ontological conception was not invented by the era of Koch and Ehrlich, it rose to prominence with the rise of bacteriology. Infectious diseases provided paradigm cases for this view. The medical philosopher Henk ten Have characterized the ontological disease concept in the following way: "Like Pandora's box, nature harbours hostile entities that can invade the human body. These entities, moreover, are characterized by specificity: they are clearly distinguishable, because each of them has a typical combination of properties".[8] Note that the term 'specificity' is used here. Moreover diseases are also characterized as hostile entities. Fleck's *Kampfgedanke* (the third style element) is not too far off.

To a distinct disease entity corresponds a specific *aetiology*. One formulation of this (style-dependent) 'necessary' correspondence is as follows: "The principle of etiological specificity of disease implies that every disease entity is produced by a quite particular cause, that different diseases cannot arise from the same cause, nor can different causes produce the same disease".[9] This has become an established principle largely as a result of the work of Robert Koch. Without an ontological conception of disease - if diseases were not seen as objectively existing, clearly distinguishable entities (or *entia morborum*) - the search for a specific aetiology would of course be meaningless. But fulfillment of the principle also

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7 W. Bulloch, *The History of Bacteriology*, New York (Dover), 1979 [1938], p. 156.

8 H. ten Have, 'Ziekte als wijsgerig probleem', *Wijsgerig Perspectief* 25 (1984-85)-1: 5-12.

9 Jendrassik, cited in K. Faber, *Nosography*, New York, 1930, p. 183.

demands that the 'causes' of diseases - in this connection living causes or 'agents' - can be described in a specific and unambiguous way. This was denied by the German botanist Carl Nägeli. He recognized no boundaries between different species of bacteria, only differences of degree, and held that one form of bacterium could easily transform itself into another. If Nägeli's *pleomorphism* had been sustained, the whole idea of a specific aetiology would have remained wishful thinking. Against Nägeli and other adherents of this doctrine Koch persuasively argued, in the footsteps of Ferdinand Cohn and with the support of new techniques of microscopy and staining, that the pathogenic bacteria could be divided into clearly distinct species. Thus the principle of aetiological specificity could be fulfilled: "A distinct bacteric form corresponds, as we have seen, to each disease, and this form always remains the same, however often the disease is transmitted from one animal to another".[10] Koch defined the methodological rules (known as *Koch's postulates*) for the identification of specific aetiological 'agents' of the various infectious diseases.[11]

The distinction and opposition between humoral and cellular immunity (the fourth style element on the above list) can also be brought under Wassermann's *Grundidee*, once we realize that the primary interest of German (as against French) immunology was in humoral immunity. This is recognized by Fleck: "the French stress the second, the Germans the first" (63/84). In the field of humoral immunity the basic idea of specificity could be translated into serological specificity. As Ilana Löwy declares about the debate between the 'cellular' school and the 'humoral' school: "In fact this polemic not only was about the mechanism by which immunity is obtained but dealt also with the problem of specificity. While the 'cellular' school was interested in *non-specific* mechanisms of defence against bacterial infection, the 'humoral' school was interested in *specific* anti-infectious protection obtained by the mediation of humoral antibodies [my emphasis - HvdB]".[12]

The fifth element of the 'serological' thought style - the emphasis on the methodical necessity of 'controls' - can also be easily assimilated to the basic idea of specificity. In his didactic introduction Citron urged the indispensability of carrying out control tests from the necessity of ascertaining the true specificity of a serological reaction: "When the specificity of a reaction becomes doubtful, its diagnostic utilization must accordingly suffer. For this reason, we must repeatedly discuss the question whether and to what extent any given reac-

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10 R. Koch, *Untersuchungen über die Aetiologie der Wundinfectionskrankheiten*, Leipzig, 1878; cited in H. Freund and A. Berg (eds), *Geschichte der Mikroskopie*, Frankfurt am Main (Umschau Verlag), 1964, p. 184.

11 In Chapters III and IV we have seen that Koch's postulates were not strictly followed in the case of syphilis. Without having been brought into pure culture, *Spirochaeta pallida* was ultimately accepted as the causative agent of this disease.

12 I. Löwy, 'The Epistemology of the Sciences of an Epistemologist of the Sciences: Ludwik Fleck's Professional Outlook and its Relationship to his Philosophical Work', in R.S. Cohen and T. Schnelle (eds), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, pp. 421-42, on p. 423.

tion is specific and ascertain true specificity in any way possible, especially by means of control tests" (Citron cited in Fleck 58-59/78).

The sixth style element on the list, Fleck's much abused 'chemical delusion', also stands under the sign of specificity. The unfortunate victim of this depreciatory expression was Paul Ehrlich's *side-chain theory*, which had been formulated to elucidate the formation of antibodies and the nature of the antigen-antibody reaction. The expression was not aimed at two rivals of Ehrlich's theory, namely Arrhenius' physico-chemical (electrolytic) theory and Bordet's colloid-chemical view.[13] Ehrlich's theory was the most popular among serologists in Germany.[14] Although Ehrlich was criticized by his opponents for not properly respecting the principle of simplicity or 'parsimony' (because for every particular serum effect he postulated the existence of a separate substance), the great majority of German serologists preferred Ehrlich's theory. In their eyes it could account in a most satisfactory way for the specificity of the antigen-antibody reaction. As Rubin concludes: "The specificity of the immune response certainly counted heavily for its popularity".[15]

As the above illustrates, it is indeed possible to discern a stylized unity and coherence in the set of style elements collected by Fleck. This unity can be expressed by the basic idea of specificity. The social sphere of influence of this basic idea, however, extends far beyond the boundaries of the collective of serologists. It may be appropriate, therefore, to consider Fleck's 'serological' thought style as only a part of a more general style of wider social scope, which we could provisionally dub the thought style of specificity. Following Wassermann, we could say that the basic idea of specificity - like a stone falling in a pond - has 'produced' ever wider circles in the fields of nosology, bacteriology and serology. Moving outside the narrow compass of the serological discipline (actually, Fleck's description of the so-called 'serological' thought style already went beyond these confines!), we can add one further field of medical science in which the impact of 'specificity' was clearly manifest: therapeutics.

Taking this further step appears logical. The description of disease entities, the search for specific aetiological agents and the diagnostic (sometimes therapeutic) use of specific reactions between antigens and antibodies have as their counterpart in the sphere of therapeutics the search for specific remedies. In this connection Ehrlich's programme of *chemotherapy*, already mentioned by Wassermann as falling under his broad umbrella of specificity, deserves special attention. The programme's mission was to find specifics, or as Ehrlich declared concisely in 1908: "wir müssen zielen lernen, chemisch zielen lernen" (we must

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13 P.M.H. Mazumdar, 'The Antigen-Antibody Reaction and the Physics and Chemistry of Life', *Bulletin of the History of Medicine* 48 (1974): 1-21.

14 L.P. Rubin, 'Styles in Scientific Explanation: Paul Ehrlich and Svante Arrhenius on Immunochemistry', *Journal of the History of Medicine* 35 (1980): 397-425.

15 Ibid., p. 422.

learn to aim in a chemical way).[16] Ehrlich would hit the bull's-eye in 1909 with his discovery of arsphenamine or Salvarsan as an effective drug against syphilis. Fleck traces chemotherapy back to mercury treatment, which had been introduced at an early stage in the history of syphilis: "From the mercury idea a general therapeutic idea arose which contributed such wonderful remedies as Salvarsan among others" (17/26). Although mercury treatment indeed also exemplifies the basic idea of specificity, there is no need to go that far back in history to uncover the roots of Ehrlich's chemotherapy. As we saw in Chapter VII, Ehrlich himself considered his chemotherapy as a logical extension of Emil Behring's serotherapy. Natural antibodies were the ideal 'magic bullets'; whenever serotherapy was not practicable, however, chemical remedies should be called into service as a second best. Throughout his scientific career - from vital staining through immunology to chemotherapy - Ehrlich always remained loyal to the basic idea of specificity. His structural-chemical imagination enabled him to make this basic idea a particularly fruitful one for different parts of medical science.

In search of a contrasting style

In the foregoing analysis I have demonstrated that specificity was a recurring element in several fields of medical science: nosology, aetiology (bacteriology), serology/immunology, and therapeutics. According to Harwood's criteria, this would not yet be sufficient to warrant us to speak of a thought style of specificity. It is also necessary to show that this particular element or trait can be matched with at least one *contrastive* element or trait covering a comparable range of sectors. In other words, we have to find another style which serves as an alternative to the style of specificity. Happily, with regard to this requirement the preparatory work has already been done. In her book *Species and Specificity* the historian Pauline Mazumdar recounts and analyzes the recurrent opposition between the protagonists of 'pluralist' and 'unitarian' views or styles in the history of botany, bacteriology, immunology and immunochemistry, and blood group genetics.[17] If we can equate our 'specificity style' with her 'pluralist' style, then clearly the 'unitarian' style represents the alternative we are looking for.

Mazumdar follows Immanuel Kant in noting that 'students of nature' generally fall into two groups: the first are always on the watch for the unity underlying the diversity of nature; the second try to differentiate nature, to accentuate its diversity and to divide into species rather than to unify into genera.[18] A paradigmatic example of the first group, dubbed the

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[16] P. Ehrlich, 'Ueber den jetzigen Stand der Chemotherapie', *Berichte der deutschen chemischen Gesellschaft* 42 (1909): 17-32.

[17] P.M.H. Mazumdar, *Species and Specificity: An Interpretation of the History of Immunology*, Cambridge (Cambridge University Press), 1995.

[18] Ibid., p. 4 ff. Mazumdar refers to Kant's *Critique of Pure Reason*, translated by Norman Kemp Smith, Toronto (MacMillan), 1965, p. 540 ff. Compare *Kritik der reinen Vernunft*, A 655 ff (according to the pagination of the original first edition of 1781).

'unitarians', is Galilei with his large following among the physicists; an example of the second group, dubbed the 'pluralists', is Linnaeus. Nineteenth-century German botany and bacteriology offered the scene for the first dramatic confrontations between 'unitarians' and 'pluralists', personified by Carl von Nägeli and Ferdinand Cohn, respectively. Nägeli was a 'unitarian' thinker *par excellence*: he stressed continuity in nature and differences in degree ('*quantitative Abstufungen*') and did not allow for sharp boundaries between species; among the bacteria he did not even recognize the existence of separate species. His opponent Cohn, by contrast, was an arch-typical 'pluralist' who attempted to develop a morphological classification of bacteria along Linnaean lines by division into orders and genera. It was of course Cohn's pupil, Robert Koch, who won a decisive victory for the 'pluralist' side on the terrain of bacteriology, thanks to an impressive "morphological technology" (stains, solid media, improved microscopy). In the new German *Reich* Koch and his group of students and co-workers attained institutionally secured positions of power; for them, as Mazumdar notes, "a belief in absolute specificity was an essential mark of group loyalty".[19] From bacteriology the confrontation between 'unitarianism' and 'pluralism' now moved on to the domain of immunology. Max Gruber, trained by Nägeli, attacked views of the Koch school on the specificity of antisera. For Gruber, such specificity was not absolute but only a matter of degree. Later the same subject was at stake in his clash with Paul Ehrlich, for whom, as Mazumdar notes, absolute specificity set the style of his theory of immunity. The defence of the 'unitarian' cause in immunology and especially immunochemistry was passed on to Gruber's student Karl Landsteiner, who later also opposed Ehrlich's views on immunity. The baton was finally transmitted to Landsteiner's American student at the Rockefeller Institute, Alexander Wiener, who in the late 1940s and 1950s stubbornly defended 'unitarian' views in the field of blood group genetics against attacks of the geneticists Race and Fisher. This constitutes the last act in the conflict that has been going on now for five generations and that has been extensively described in Mazumdar's book.

Mazumdar's analysis usefully extends our account in several pertinent ways. It will have become sufficiently clear, I hope, that our 'specificity style' may indeed be equated with her 'pluralist' style. She also supplies us, moreover, with the alternative style we looked for. We now have available a well-defined characterization with which to assess whether various deviant cases fit the pattern of this style.

In Chapters IV and V we met a colourful critic of bacteriology and defender of clinical medicine, Ottomar Rosenbach. His trenchant criticisms of the nosological delineation of syphilis as a well-defined entity, his attacks on a supposedly specific microbic agent and his sarcastic dismissal of the 'myth' of the specific action of mercury and iodine, all fit a coherent pattern. Rosenbach's advocacy of a so-called 'energetic' approach, both for diagnosis and therapy, also seems to represent a unitarian style. Thus his dissident views in the field of bacteriology and the diagnosis and treatment of syphilis clearly exhibit stylistic coherence.

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19 Ibid., p. 8.

Another notable dissident with respect to the dominant style of specificity, of course, was Fleck himself. I remarked above that he did not recognize his own medical, bacteriological and immunological views as exemplifying a certain style, although he used them as an implicit yardstick for identifying and describing the so-called 'serological' thought style. We can attribute a stylistic significance to Fleck's preference for a colloid-chemical view (as against the alleged 'chemical delusion' of Ehrlich's theory) when we take cognizance of the fact that Landsteiner too, during a certain period of his career (from 1903 to about 1912), considered colloid chemistry an ideal alternative for Ehrlich's structural organic chemistry: "The new and exciting field of colloid chemistry, the youngest branch of physical chemistry, seemed to suggest that chemical specificity might play no part at all in the reactions that took place in the living organism".[20] We have already seen that Fleck, according to his own testimony, was no 'friend' of specificity. There are more indications that his professional thought was marked by a 'unitarian' style. His early rejection of the view of diseases as objectively given entities (see Chapter II) was not just a first step towards a constructivist approach to medical science, it also accords well with a 'unitarian' style in the domain of nosology. In the field of bacteriology Fleck's eager adoption of 'bacterial variability' and his rejection of the assumption of the fixity of bacterial species - which is constitutive of Koch's methodology (cf. Fleck: "Species were fixed because a fixed and restricted method was applied to the investigation [..]"; 92/122) - are equally characteristic. Fleck's professional thought thus exhibited stylistic coherence over a variety of different fields (nosology, bacteriology, immunology). Given the dominance of the Koch-Ehrlich group in the German-speaking world of medical science, Fleck's position was clearly that of a dissident outsider.[21]

The question of stylistic constraint revisited

In the foregoing analysis I followed Wassermann's simile of a stone falling in a pond when I stated that the basic idea of specificity produced ever wider circles in various fields of medical science. This formulation has an idealistic savour in that it attributes power and efficacy to a 'mere' idea. Such 'empowering' of ideational factors conflicts with the finitist tenets of many forms of modern constructivism. One might hold, however, that the construc-

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20 Ibid., p. 9. See also p. 222: "Immune specificity, [Landsteiner] suggested, could be regarded as the sum of a number of component reactions, in themselves nonspecific". Fleck's adoption of a colloid-chemical view of the nature of immune reactions during the mid-1930s was however rather outdated by the standards of the discipline of immunology.

21 Mazumdar shows that Gruber and Landsteiner were given little opportunity to propagate and elaborate their deviant views in Austria and Germany. After his clash with Ehrlich, Gruber abandoned immunology altogether to turn to public hygiene. His student Landsteiner finally found a niche for completing his immunochemical lifework at the Rockefeller Institute in New York. "The power structure built up within the Koch-Ehrlich group may be one of the most effective ever formed in science" (Mazumdar, pp. 381-82).

tivist rejection of the power of ideas goes too far. What else does the case of 'specificity' show us but the example of an idea that has been immensely infuential in a number of fields? If the power of this basic idea is denied on *a priori* finitist grounds, then perhaps so much the worse for finitism! How to solve this apparent paradox? Do we have to drop finitism? Or is there something fundamentally wrong with our historical perception that the notion of specificity has been a very 'powerful' idea indeed?

We may attempt to find a way out of this dilemma (or at least to soften it somewhat) by recognizing that speaking about the 'power' of ideas might be an elliptical mode of expression. To attribute power to ideas, then, would not necessarily imply the suggestion that ideas are able to exercise a decisive influence exclusively of their own accord, as it were, but might involve the much weaker claim that such ideas can make a crucial difference in the appropriate circumstances when they are combined and supported by suitable social forces.

In this connection it is interesting to note that Mazumdar stresses the extraordinary importance of 'pedagogical moulding' in the transmission of a certain style of thought from one generation to the next. In the pluralist camp the characteristic way of thinking was passed on from teacher to student, e.g. from Cohn to Koch and from Koch to his followers. In the unitarian camp teacher-student relations for propagating the faith were even more salient. An extensive quotation from the concluding chapter of Mazumdar's book may be appropriate here:

> "It is my feeling that in the culture that I have discussed here, which is mainly that of nineteenth- and early twentieth-century German science, it is almost impossible to exaggerate the determining effect of [the] mixture of technology and intellectual patterning that is passed from teacher to student. It is a patterning that outweighs in many cases the desire to have a successful career: in the social climate of the time, it was the pluralists, the Koch-Ehrlich team, who were guaranteed a good position. Unitarians, from Nägeli to Gruber and Buchner, to Landsteiner, to Wiener, all suffered the fate of outsiders, yet they persisted in their opposition. The young people were given a sketch, as it were, of their life's work. They were taught to look at the empirical data of their science in a particular way, to expect a certain structure in nature, and to feel that they had made a successful achievement when they had found such a structure".[22]

The latter description neatly fits Fleck's definition of a thought style as a particular 'preparedness' or 'readiness' for selective perception and thinking. Mazumdar herself refers to the parallel with Fleck's discussion of Citron's introductory lecture as a pedagogical induction, effected with "gentle constraint", into the serological style of thought (63/73). In the quoted passage she mentions an additional component of the process of 'pedagogical moulding', namely technology. As an example we can think of Koch's methods of cultivating bacteria, preparing solid media, staining and microscopy, which Mazumdar characterizes as a "mor-

22 Ibid., p. 380.

phological technology". New recruits were introduced into Koch's way of thinking partly by being trained in the use of these techniques. Occasionally, Fleck also draws attention to the effect of scientific devices and instruments in directing our thinking on to the tracks of a certain style: "Living as we do among devices and instruments from the current scientific thought-style, we always obtain 'objective' stimuli urging us to that way of thinking but no other".[23] Mazumdar puts the intellectual patterning effected through material techniques within the broader pedagogical context of training by example.

Pedagogical moulding, however, is not the only social force sustaining a particular thought style. As Mazumdar notes, "[t]he ideas had to be fitted into a social power structure in order to acquire authority".[24] An unprecedentedly robust power structure, manifesting itself in the control of university chairs, directorships of institutes and journals, had been built up by the Koch-Ehrlich group in late 19th- and early 20th-century German bacteriology and immunology. Speaking of the power of the idea of specificity may be just an oblique way of referring to the social power of this particular group. If a belief in absolute specificity was a mark of loyalty to the Koch-Ehrlich group, as Mazumdar maintains, then the power of the group to some extent explains the influence of this idea. As a 'social' explanation, however, this account goes some way but doesn't travel very far. It does not explain why, or by which social mechanisms, the Koch-Ehrlich group adopted the idea of specificity as its hallmark *in the first place*. I must admit that I cannot think of any social explanation in the format of the Strong Programme (apart from the formative influence exercised on Koch by his teacher Cohn) which would give a convincing answer to this question. Nor does Mazumdar provide us anything of this sort.

A less ambitious but perhaps more feasible approach can be taken if we interpret the basic idea of specificity in terms of Mary Hesse's network theory as a *coherence condition* for the organization of a widely extended knowledge network.[25] Among (the sources of) such coherence conditions she also counts "relatively *a priori* and perhaps culturally conditioned metaphysical principles"[26] - and it could be reasonably argued that specificity constitutes just such a metaphysical principle -, but this line of thought has been blocked by Hesse's constructivist followers Barnes and Bloor for being too idealistic. What I want to stress here is that, as a coherence condition, the basic idea of specificity operated as an overarching 'regulative principle' through which divergent research efforts extending over a broad range of fields - nosology, bacteriology, serology, therapeutics - could be mutually coordinated.

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23 L. Fleck, 'The Problem of Epistomology [1936]', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, pp. 79-112, on p. 109.

24 Mazumdar, op. cit. (note 17), p. 381.

25 M. Hesse, *The Structure of Scientific Inference*, Berkeley and Los Angeles (University of California Press), 1974.

26 Ibid., p. 52.

Given the necessity of mutual coordination in the social division of scientific labour, one can easily imagine - by way of a thought experiment - a cumulative and self-reinforcing process being set in motion once the idea of specificity, for one reason or another, has acquired a foothold in any of these fields. From this initial field of strength the idea of specificity will then spread to the other fields. If the coordination succeeds, that is, if the idea will be put to successful use in the new fields, this will of course enhance its overall strength. Ultimately, a coherent and robust network of knowledge may be the result. (Something like this may have happened in actual history inasmuch as the idea of specificity spread from bacteriology to serology and from serology to therapeutics. In my judgement, its initial strength in the field of nosology was not sufficient to have triggered the entire movement; rather, its position there was strengthened through retroactive feedback from bacteriology.) Once such an extensive and tightly coupled network of knowledge is in place, it might be defensible to attribute some constraining force to the dominant thought style of specificity in virtue of both social and intellectual factors; socially, it derives from the necessity of mutual coordination in the social division of scientific labour; intellectually, from the solid coherence of the existing cognitive network.

It is along lines such as those sketched above, I think, that the riddle of the apparent power of the idea of specificity (and thus of the apparent compelling force of the 'serological' thought style which so much impressed Fleck) must be solved.

CHAPTER IX

CONSTRUCTIVISM, REALISM AND THE SOCIAL

In this chapter I shall deal more thoroughly with two 'big problems' that are raised both by Fleck's work and by modern constructivism and that have been broached repeatedly in previous chapters, to wit, the issue of realism and the complex question of how to conceive of 'the social' and of the relationship between the individual and the collective. The latter problem includes the quest for an adequate conception of social practices in general and of scientific practices in particular. In the course of developing a defensible and coherent position on these controversial issues I inevitably have to confront the relevant views of the philosophical and sociological literature, but I will also draw occasionally on the empirical materials presented in the preceding chapters.

1. The question of realism

"One of the curious things about being part of the new sociology of science", the American sociologist Susan Leigh Star once remarked, "is that one is immediately plunged into philosophical debates about realism and relativism".[1] With barely disguised annoyance she then described how she had been drawn into such debates time and again:

> "I have been involved in the sociology of science and technology for about ten years [..]. I have given perhaps 75 talks on various aspects of science studies. Only once have I not been asked some version of what I now call the 'there there' question: but are you saying it's *all* socially constructed? Doesn't that mean anything could be true? Isn't there anything out there? Are scientists making it all up?".[2]

Instead of directly answering these questions, Star formulates a sociological counter-question by way of response: "Rather, as sociologists, let's ask: under what conditions do such questions about reality routinely get raised?". In my view, this evasive strategy will not work. The questions about reality simply won't go away, even if the sociologist tries hard to shift the debate to a different level of discourse. Besides, one would think that Star owes her perplexed audiences a serious reply. Whoever defends the thesis that scientific knowledge or scientific facts or even material objects are (socially) constructed must be prepared to answer the objections such a *prima facie* counterintuitive view is bound to provoke. The trouble is that an authoritative and unanimous answer to such objections is unlikely to be

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1 S.L. Star, 'Introduction: The Sociology of Science and Technology', *Social Problems* 35 (1988): 197-205, on p. 201.

2 Ibid.

forthcoming from constructivist studies of science. As Star herself rightly notes: "Scholars in science studies have disagreed about this issue and will continue to do so for some time to come".[3] I shall attempt to develop my own position on this matter by critically engaging myself with the several views that are represented in this field.

Representations and their objects

As a way to simplify the whole gamut of opinions relating to this issue, let me once more return to the distinction between 'moderate' and 'radical' forms of constructivism that was introduced in Chapters I and II. Moderate constructivists either take a (methodologically motivated) agnostic stance toward objective reality (Collins) or profess to be ontological realists (the adherents of the Strong Programme), without however giving any role to a (purported) 'correspondence with reality' in explaining the acceptance or rejection of scientific theories. In short, moderate constructivists are epistemological relativists. Radical constructivists such as Karin Knorr-Cetina and Bruno Latour, by contrast, claim to have circumvented the entire epistemological debate between realism and anti-realism. They also claim to have opened up the closed universe of social interaction between humans in favour of a triumphal return of material objects and other non-human 'actants'. Characteristic of the turn from science-as-knowledge to science-as-practice proclaimed by other radical constructivists is the recognition that as a constructive practice scientific work is deeply engaged with the material world. Yet, despite all these apparent approaches to a more 'realist' position, radical varieties of constructivism have not been able to avoid a head-on collision with current versions of scientific realism on epistemological and especially on ontological issues.

The root of the trouble is to be sought in the overextended use of the construction metaphor by radical constructivists. Sergio Sismondo has rendered us the useful service of sorting out several different meanings of 'construction'. He distinguishes the following senses of the term:

> "(a) the construction, through the interplay of actors, of institutions, including knowledge, methodologies, fields, habits, and regulative ideals; (b) the construction by scientists of theories and accounts, in the sense that these are structures that rest upon bases of data and observations; (c)the construction, through material intervention, of artefacts in the laboratory; and (d) the construction, in the neo-Kantian sense, of the objects of thought and representation".[4]

It is clear that Sismondo's distinctions are based on the different types of objects (including 'social objects' like institutions) that are held to be (socially) constructed. Meanings (a) and (b) are prominent in the work of moderate constructivists; their concern is primarily with the

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[3] Ibid., p. 202.

[4] S. Sismondo, 'Some Social Constructions', *Social Studies of Science* 23 (1993): 515-53, on p. 516.

construction of scientific *knowledge* (or with the construction of facts, but then these are understood as items of knowledge). The precise distinction between meanings (a) and (b) is not of much importance to their purposes, because 'knowledge' as a system of collectively held beliefs about nature is ipso facto treated as a social institution. As such this usage of the construction metaphor is ontologically innocuous and therefore not too offensive to scientific realists. Construction in sense (c) figures prominently in some of the work of Knorr-Cetina. She has argued that 'nature' is conspicuously absent in the scientific laboratories nominally devoted to its study, because the objects, raw materials and test animals that are worked upon or with have all been specially prepared, purified, bred or 'pre-constructed'.[5] She has thus drawn attention to the utter artificiality of the laboratory setting which constitutes a critical challenge - termed the 'Bachelardian challenge' by Hans Radder[6] - for established forms of realism. The fourth sense of construction, meaning (d), which is not always clearly distinguished from the other meanings, is the most problematic. It is to be found in the writings of several radical constructivists and implies a view of ontology that is highly offensive to various forms of scientific realism.

What Sismondo calls the 'neo-Kantian' sense of construction involves the idea that objects are created by their representations. This idea has been most strongly advocated by Steve Woolgar.[7] He criticizes other British representatives of the sociology of scientific knowledge for being insufficiently radical. Sociologists like Barnes and Bloor insist on maintaining a clear distinction between the natural world and the theoretical accounts we give of it, although they emphasize that our experience of the world may in principle always give rise to more than one account (because of the 'underdetermination' thesis). In order to fully realize the 'radical potential' of the sociological study of science, Woolgar argues that we should be prepared to reverse the arrow, that is, to consider the natural world as a product of scientific knowledge rather than vice versa. This amounts to saying that accounts or representations constitute their objects. A clear illustration of this way of thinking can be found in the so-called 'splitting-and-inversion model' that was used earlier in Latour and Woolgar's *Laboratory Life* to understand the construction of scientific facts. The solid existence of a fact 'out there', they maintained, results from the settlement of a scientific controversy. As long as controversies are still raging, there is no stable reality to which scientific statements refer. Once agreement sets in, something strange happens: "*The statement becomes a split entity*. On the one hand, it is a set of words which represents a

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5 K. D. Knorr-Cetina, 'The Ethnographic Study of Scientific Work: Towards a Constructivist Interpretation of Science', in K. Knorr-Cetina and M. Mulkay (eds.), *Science Observed: Perspectives on the Social Study of Science*, London (Sage), 1983, pp. 115-40.

6 H. Radder, *In and About the World: Philosophical Studies of Science and Technology*, Albany (State University of New York Press), 1996, p. 74.

7 S. Woolgar, *Science: The Very Idea*, Chichester (Ellis Horwood), 1988.

statement about an object. On the other hand, it corresponds to an object in itself which takes on a life of its own".[8] Subsequently, history is rewritten, as it were. Now it is held that the object has been there all along, waiting to be discovered. Thus an inversion takes place: "the object becomes the reason why the statement was formulated in the first place".[9] Part of the construction of a scientific fact is the removal of all traces indicating that construction has actually taken place.[10] A remarkable feature of Latour and Woolgar's view is that an object enters into existence precisely at the time of its discovery. Thus they held that the hormone TRF (thyrotropin releasing factor), the object figuring in their case study, began to exist at the time of its official discovery, in 1969, but did not exist before that year.

Although the 'splitting-and-inversion model' of *Laboratory Life* is undoubtedly very subtle and ingenious, it also contains a thesis that is highly problematic, to wit, the idea that objects are created out of negotiation and eventual consensus, or in other words that representations, once generally shared, give rise to their objects. It is as if entities which initially existed only 'on paper', would suddenly jump from the pages of scientific publications to the real world outside the texts once scientists had made up their mind. The problem with this view is that it can hardly be reconciled with established conceptions of causality, or as Sismondo states: "The idea that representations routinely create their objects has little or no connection with our ordinary notions of cause and effect".[11] It also severely strains our imagination to believe that, say, microbes made their entry into the world at the time of Koch and Pasteur and not already much earlier.

How widely held is this particular view in contemporary forms of constructivism? Sismondo suggests that in his later work Latour has effectively abandoned it, citing the "distinctly realist flavour" which many commentators have attributed to his more recent contributions.[12] It thus seems that the thesis that accounts are constitutive of their objects has become the special hallmark of Woolgar's brand of sociology. This impression is deceptive. In one version or another the idea is to be found in many varieties of radical constructivism. It is significant that in a critical reply to Sismondo's article Knorr-Cetina

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8 B. Latour and S. Woolgar, *Laboratory Life: The [Social] Construction of Scientific Facts*, Princeton, NJ (Princeton University Press), 1986 (2nd edition), pp. 176-77.

9 Ibid.

10 In their discussion Latour and Woolgar rather carelessly switch from speaking about a 'fact' to speaking about an 'object'. This may have to do with the particular case under study in their book. It deals with the formation of the *fact* that 'TRF (thyrotropin releasing factor) is pyro-glu-his-pro-amide', which can also be viewed as the isolation of the *object* TRF, the active substance responsible for thyrotropin release, and the elucidation of its chemical structure. In this case, then, the discovery of a 'fact' is simultaneously the discovery and identification of an 'object'.

11 Sismondo, op. cit. (note 4), p. 536.

12 Ibid., p. 537.

openly admitted to support a version of the thesis so much criticized by the latter.[13] And I'm also convinced, against Sismondo's suggestion, that the more recent work of Latour still assumes the validity of the idea. The realist philosopher Ronald Giere has raised the pertinent question how much different Latour's position actually is from that of his one-time co-author Woolgar: "On the surface, there appears to be considerable difference in that Latour has no interest in the wholesale deconstruction of modern technoscience. He is perfectly willing to engage in 'realist' discourse about unproblematical entities like microbes and DNA. *But if one asks how such entities came to be unproblematical, similarities with more radical versions of social constructivism* [i.e. with Woolgar's position - HvdB] *reappear* (my emphasis)".[14] I think Giere is here on the trail to solving the oft-noted paradox about Latour's work in so far as it represents a strange mixture of constructivism and realism. This paradox derives from a duplicity that was already built into the splitting-and-inversion model of *Laboratory Life* and that has since been reinforced by Latour's adoption of the methodological injunction to 'follow the actors'. It is this rule which enjoins the analyst of technoscience to take a skeptical or agnostic attitude toward the entities under discussion as long as controversies are raging (because during that period scientists themselves are skeptical) and to become a committed realist with regard to the existence of those same entities after the controversies have been settled (because then the scientists have themselves turned into realists): "We are realists as much as the people we travel with [..]. But as soon as controversy starts we become as relativist as our informants".[15] Yet in one crucial respect Latour is not fully consequential in following his 'informants'; whereas they will find that the object on whose existence they have come to agree has been there all along, Latour continues to hold that this object was only brought into being by their reaching agreement. Due to this inconsequentiality Latour's alleged realism is a very weird kind of realism indeed.

From this background we will understand more easily how 'technoscience', on Latour's account, is engaged in the ceaseless proliferation of ever new entities, 'hybrids' or 'quasi-objects', or whatever these products may be called.[16] Society and nature, Latour asserts, are being 'co-produced' by technoscience - please take note that he speaks about the production of nature, not just representations or accounts of nature! Similar world-making capacities are granted to scientific practice in Knorr-Cetina's version of radical

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13 K. Knorr Cetina, 'Strong Constructivism - from a Sociologist's Point of View: A Personal Addendum to Sismondo's Paper', *Social Studies of Science* 23 (1993): 555-63, on p. 557.

14 R.N. Giere, 'Science and Technology Studies: Prospects for an Enlightened Postmodern Synthesis', *Science, Technology, & Human Values* 18 (1993): 102-12, on p. 107.

15 B. Latour, *Science in Action*, Milton Keynes (Open University Press), 1987, p. 100.

16 B. Latour, *We Have Never Been Modern*, Cambridge MA (Harvard University Press), 1993.

constructivism, which considers laboratories as "instruments of world construction".[17] According to her, "science secretes an unending stream of entities and relations that make up 'the world'".[18] Among the objects and entities science supposedly gives rise to are not just counted plastics, synthetic drugs or genetically modified organisms, but also microbes, genes, atoms, quarks, pulsars, black holes, supernova, etcetera. Thus Latour and Knorr-Cetina appear to deliberately blur the distinction between meanings (c) and (d) of the term 'construction' in Sismondo's list. For the first category of objects, 'construction' refers to a material procedure like chemical synthesis or the technique of transgenesis - meaning (c) of Sismondo. For the second category of objects, 'construction' refers to something completely different, namely, the social process of building consensus about the existence and properties of the said entities - corresponding to Sismondo's meaning (d). It is rather misleading to use the term 'secretion' to describe this social process as if it were a purely material process!

In her reply to Sismondo and in later work Knorr-Cetina has come to the defence of the criticized thesis that representations create their objects. A more acceptable and plausible version of this thesis, in her view, states that what comes into existence when science 'discovers' a microbe or a subatomic particle, "is a specific entity distinguished from other entities (other microbes, other particles) and furnished with a name, a set of descriptors, and a set of techniques in terms of which it can be produced and handled".[19] Formulated in yet another way, "some part of a preexisting material world becomes specified and thereby real as something to be reckoned with, accounted for, and inserted in manifold ways into scientific and everyday life".[20] It is also not a matter of bringing a new object into being by the simple act of snapping fingers; scientists have rather to struggle hard to accomplish such a thing. Knorr-Cetina thus looks primarily at the various ways in which the new entity is assigned a place in a familiar cultural world, how it is drawn into that world, so to speak, and only thereby acquires a distinct identity. She is willing to grant the pre-existence of an (unknown) material world, but not the pre-existence of specific objects. In my opinion these qualifications do not suffice to warrant radical constructivist talk of refurnishing the world

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17 K. Knorr-Cetina et al., 'Laboratorien: Instrumente der Weltkonstruktion', in P. Hoyningen-Huene and G. Hirsch (eds.), *Wozu Wissenschaftsphilosophie? Positionen und Fragen zur gegenwärtigen Wissenschaftsphilosophie*, Berlin and New York (Walter de Gruyter), 1988, pp. 315-44.

18 Knorr-Cetina, op. cit. (note 13), p. 557; this is a rephrasing of a previous statement dating from 1983 ; see Knorr-Cetina, op. cit. (note 5), p. 135. It is significant that the term 'secretion' was already used in a similar connection by Latour and Woolgar, op. cit. (note 8): "Reality is secreted" (p. 243).

19 K. Knorr Cetina, 'Laboratory Studies: The Cultural Approach to the Study of Science', in S. Jasanoff et al. (eds.), *Handbook of Science and Technology Studies*, Thousand Oaks, London, New Delhi (Sage), pp. 140-66, on p. 161.

20 Ibid.

with new objects and of thereby (re)creating the world. Knorr-Cetina still makes the existence of an object depend on human knowledge, which in Sismondo's eyes amounts to the fallacy of conflating ontology with epistemology (Knorr-Cetina rejects the charge, but I fail to see on which grounds she can do so).[21] If an object X counts as a *specific* object only if it is furnished with a name and a description, then of course it is trivially true that X comes into being *as a specific object* only as a consequence of our epistemic activities (we did not create X, but we contributed to the creation of X-furnished-with-a-name-and-a-description simply because we supplied the name and the description!). If anything deservedly invites the charge of conflating ontology with epistemology, it is such a conception of 'existence'.

Scientific realism or moderate constructivism?

What is the alternative if we reject the radical constructivist thesis that science, by developing new accounts of nature, refurnishes the world with new objects? Are we then, as Knorr-Cetina suggests, condemned to accept some strong form of scientific realism which "assumes the pre-existence of specific objects before they have been delimited by science in precisely the way they are delimited by science"[22]? Or, in other words, do we have to accept that the world comes 'carved at its joints'? I do not think so. There is a third option. It is perfectly possible for a constructivist analyst of science to adopt, during a scientific controversy, a deliberately agnostic attitude vis-à-vis the entities under debate, *and to persist in this uncommitted stance even after the scientists involved have resolved their differences!* (What is not possible is to be an uncommitted agnostic 'across the board', that is, with regard to all domains of human knowledge, but that is not advocated here.) Although Latour maintains that "[w]e cannot be more relativist than scientists about [the stabilized parts of science] [..] [b]ecause the cost of dispute is too high for an average citizen, even if he or she is a historian or sociologist of science"[23], this is just a practical difficulty, not an impossibility in principle. The historian or sociologist may still argue that the accepted theoretical accounts of the phenomena in question are not the only possible ones, even if he or she is unable to come up with concrete alternatives. If we avoid the duplicity of being an agnostic constructivist during a controversy and a dyed-in-the-wool realist thereafter, then we can also avoid the implausible thesis that objects are created by their representations. This

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[21] The tendency to define 'existence' as dependent on human practices is also found in Joseph Rouse's otherwise balanced and sophisticated discussion of realism and anti-realism: "What it is for an x to exist (as an x) is constituted [..] by the ways it can be encountered in the course of intelligibly dealing with the world". Similarly: "what there is does depend upon what can be made manifest in our practices". See J. Rouse, *Knowledge and Power: Toward a Political Philosophy of Science*, Ithaca and London (Cornell University Press), 1987, p. 161 and p. 145.

[22] Knorr-Cetina, op. cit. (note 13), p. 557.

[23] Latour, op. cit. (note 15), p. 100.

line of approach is generally taken by moderate constructivists. I have followed this line in Chapters III and IV on the history of syphilis concepts and the discovery of the causative agent. The point of these chapters was not that at each new turn of events medical science sent out new diseases or new microbes into the world, but that modern notions of syphilis and of its causative agent were the negotiated (provisional) outcomes of debates on carving up the world in different ways.

If we have to reject radical constructivism, why opt for moderate constructivism? Wouldn't a strong form of scientific realism be preferable to the latter? In my judgement, the flavour of current versions of scientific realism is just a little bit too strong to swallow them whole. Take, for instance, Michael Devitt's version of scientific realism, which he presents as a robust realism "worth fighting for". According to this version, "tokens of most current common-sense and scientific physical types objectively exist independently of the mental".[24] A realist of this variety is committed to the belief in the independent existence not just of stones, cats, dogs, trees (common-sense realism), but also of electrons, muons, black holes, curved space-time and many other unobservable entities posited by modern physical science (scientific realism). To me it would seem that such a form of realism is committing itself heavily to *what is currently accepted in modern science*, in apparent contradiction to its own maxim that the ontological question should be settled before epistemic issues. However that may be, the realist's world is certainly a 'ready-made' world. Devitt grants us the freedom which kinds to pick out from the world with words, but after this initial act of discretion the world itself immediately takes over: "Whatever it is to be a member of the kind *rose* simply *is* whatever it is to be a rose. That is something that *the world* has control over, not any of us".[25] I think the finitist theory of meaning offers a more plausible picture of concept application: meaning is open-ended, and created in typical step-by-step fashion. There is no privileged moment in time at which the meaning of a scientific or common-sense term (say, 'rose') is *fixed* once and for all. Finally, I have to address a certain type of argument that is often advanced in support of scientific realism. I would like to call it, somewhat maliciously, the Argument from Practical Success, in analogy to the so-called Argument from Design that was once popular for proving the existence of an intelligent Creator until Darwin destroyed its credibility. The undeniable success of modern science in practical applications, so the argument runs, would be nothing less than a miracle if we did not assume the scientific theories on which these applications are based to be true or approximately true and the entities posited therein to be really existent. To give a specific example: Would the fact that our CD player works, and works so well, be understandable at all if we did not assume the existence of photons and lasers and their

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24 M. Devitt, *Realism and Truth*, Oxford (Blackwell), second edition, 1991, p. 303.

25 Ibid., p. 243.

properties as revealed in modern laser theory?[26] Now there is much to be said in reply to this type of argument. Formally, it has the format of a so-called 'inference to the best explanation', which is in fact highly controversial in modern philosophy of science.[27] I do not want to dwell on the formal aspects of this type of inference here, however, but to draw attention to the rather 'one-dimensional' notion of practical success that is used unproblematically by its protagonists. Marvelling at some selected wonders of modern technology, those latter-day followers of physico-theological reasoning cannot help yielding to the irresistable conclusion that the fountain-head of these good things, modern science, must have been virtually omniscient indeed, just like their pre-darwinian predecessors revelled in the apparent purposeful ingenuity of living nature as tangible evidence for the wisdom of God. But if practical successes count in favour of modern science, then what about the failures? Or would one want to assert that the record of scientific applications is made up exclusively of successes? What counts as 'success' anyway? Is the application of nuclear energy a clear instance of success, as its protagonists would have it, or rather of failure, as its critics maintain? In Chapter V we have seen that during the inter-war years the Wassermann reaction was generally taken as highly successful (even by Fleck!), whereas after the Second World War it was judged much more unfavourably. This example is not unique. Several constructivist studies of science and technology have shown that what is counted as success or failure is at least partly a matter of social negotiation. In proceeding from an unproblematically held one-dimensional notion of success, the Argument from Practical Success blithely passes over such complications. In my opinion a more skeptical attitude, instead of uncritically gazing at the marvels of science and technology, is called for. Such a stance may be found in the following passage from Fleck (this time more skeptical than vis-à-vis the Wassermann test!): "Practical applicability is not a touchstone [for the truth of a scientific theory], for due to the harmony of illusions even a false theory is applicable. The alchemists' gold allegedly did enrich many people, and even the cost of wars was paid for by alchemists' gold".[28] Thus Fleck struck out at the nub of the Argument from Practical Success.

Although moderate constructivism is incompatible with strong versions of scientific realism, it is not opposed to realism *tout court*; indeed it declares itself in complete accord with the common-sense realism of everyday life which contrasts "theory and indeed speech generally with reality 'itself', unverbalized reality, whatever is 'out there' independent of

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[26] The example is taken from R. Nola, 'There are More Things in Heaven and Earth, Horatio, Than are Dreamt of in Your Philosophy: A Dialogue on Realism and Constructivism', *Studies in History and Philosophy of Science* 25 (1994): 689-727, p. 719.

[27] P. Lipton, *Inference to the Best Explanation*, London (Routledge), 1991.

[28] L. Fleck, 'Problems of the Science of Science [1946]', in R.S. Cohen and T. Schnelle (eds.), *Cognition and Fact: Materials on Ludwik Fleck*, Dordrecht (Reidel), 1986, pp. 113-27, on p. 127.

perception, thought and word".[29] Barnes, Bloor and Henry hold that the realist mode of speech is in all likelihood a cultural universal that is even employed by its would-be deconstructivist detractors. Their position agrees with that of the philosopher John Searle, who argues that belief in an independent external reality is a necessary presupposition for communication in everyday life.[30] Devitt would not hesitate to dismiss such external realism as a version of what he calls *fig-leaf realism*, a doctrine "so weak as to be uninteresting".[31] For his part, the anti-realist Nelson Goodman would probably agree, for he also holds that this minimal form of realism posits "a world not worth fighting for or against".[32] Barnes and his associates, however, think that such criticism is beside the point: "It is in the way that they make reference to an external world beyond speech that people everywhere reveal their mastery of the realist mode of speaking, a mastery which keeps them safe from domination by the objects and essences which they themselves create".[33] Another advantage, I would add, is that this admittedly minimal form of common-sense realism would also caution against blurring the distinction between accounts and reality, or between representations and objects.

The ontology of scientific practice

What would be the proper attitude for the constructivist sociologist of science to adopt if he or she wanted to study the activities of scientists (and technicians) in the 'natural' setting of their laboratories? One would surmise that the simple belief in the existence of an external reality without any further commitments to the existence of specific entities would not suffice. Even if the study were oriented to accounting for the actions performed and decisions reached by the scientists and technicians, it would hardly make sense to consider those actions and decisions as isolated from the material context consisting of pieces of apparatus, chemical substances and preparations, biological specimens, experimental animals and the like in which they occur. So the 'material side' has somehow to be incorporated into the analysis, but in what way? *Under what description should the material elements of the situation enter the account of the constructivist analyst?* In my view the task calls for a thoroughly pragmatic approach. Take as an example the early work on the Wassermann reaction as described in Chapter V. It was said there that Wassermann and his collaborators

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29 B. Barnes, D. Bloor and J. Henry, *Scientific Knowledge: A Sociological Analysis*, London (Athlone), 1996, pp. 87-88.

30 J. Searle, *The Construction of Social Reality*, New York (Free Press), 1995, p. 183 ff.

31 Devitt, op. cit. (note 24), p. 23.

32 N. Goodman, *Ways of Worldmaking*, Indianapolis (Hackett Publishing Company), 1992 [1978], p. 20.

33 Barnes et al., op. cit. (note 29), p. 88.

initially employed extracts from the livers of stillborn syphilitic foetuses teeming with spirochaetes as a source of antigen for performing the complement fixation test for syphilis. Was it correct for me to describe the raw materials in these terms, that is, as spirochaete-infected livers? Or would it have been better to distantiate myself from the beliefs of the participants and to designate those raw materials as livers that were *thought to be* infected with spirochaetes? Perhaps. I can certainly imagine a situation where this more cautious, uncommitted stance would be the correct line to follow, e.g. in a context where the debate is not on the existence of a serological reaction for syphilis but on whether or not Schaudinn's pale spirochaete is the causative agent of syphilis (cf. Chapter IV). In such a case it would not be wise for the analyst to prejudge the debate among the antagonists by taking a stand on an issue on which opinions are divided. But among Wassermann's team and his critics this point was not in dispute, so I saw no harm in using the particular description that I used. Even more radically, one could call into question the wisdom of my use of the designation 'livers', but again to such a charge I would defend my choice by pointing out that the debate was surely not one about anatomy. I hope it is becoming clear what I am driving at. If the aim is to explain, or account for, the interpretation which a scientist bestows on a certain experimental result, it is usually very recommendable to keep your distance from the terms in which the latter himself or herself describes the result. It does not make sense, however, to extend such local and circumscribed 'agnosticism' into an attitude of *generalized* 'agnosticism' - a policy that appears to be suggested by some of Harry Collins' methodological prescriptions. Nor would modern versions of scientific realism, on the other hand, be of much help. A constructivist student of scientific knowledge and of scientific practice cannot proceed, it is true, in a presuppositionless way. To account for the formation of knowledge (the 'topic' of the inquiry), he or she unavoidably starts from substantive assumptions about what the world (albeit even the confined 'world' of the laboratory) is like. Those assumptions are his or her 'resources'. There is no way to do it without resources, or to remain entirely uncommitted as to the character of the world. But the precise inventory of commitments to be made does not have to be dictated by a general philosophical doctrine such as scientific realism. It calls instead for pragmatic judgement that is commensurate with the task at hand.

The claim of radical constructivists like Latour and Knorr-Cetina to have brought material objects (the 'nonhumans') back in is not fully credible. In fact, those objects had never been driven out to begin with, at least not by the adherents of the Strong Programme who have always professed to be full-blooded materialists (the case of Collins may be a slightly different matter). Nor have the latter ever denied agency to things, if 'agency' is simply understood as 'causal agency' (e.g. things may stimulate our sense organs). They refuse, however, to accept the idea that 'nonhumans' can be party to the disputes and negotiations between scientists and others; *this* type of 'agency' is indeed reserved to humans. The other side of the alleged 'rehabilitation' of material objects and the like, is that there is also a tendency among radical constructivists to treat the natural world as being part of the social and cultural world. I do not deny that especially in our material practices, including the material practices of science, the world of culture *makes contact with* the world of nature. One could even speak of an *intermingling* of both worlds. Everyday practices can thus be seen as mixtures of nature and culture. But that does not mean that nature is entirely absorbed into those practices. The 'nature' that is involved in human practices is just the 'cultivated', that is, culturally domesticated and appropriated, part of nature; it does not exhaust the whole of nature. One can say that this cultivated part of nature which has been drawn into human practices consists of 'physico-cultural objects', to have a collective designation indicating the ontological status of tools, raw materials, domesticated plants and animals and the like.[34] Indeed, the practice of agriculture (literally, the cultivating of the land!) provides the paradigmatic example to which the material practice of laboratory science can be assimilated. The question is, however, whether all objects of scientific research can be assigned the status of physico-cultural objects, or in other words, whether they all must be considered as belonging to the cultivated part of nature. I don't think so. My ontological position disagees with that of Joseph Rouse, who, using Heideggerian terminology (but against Heidegger's own intentions!), tends to put items of equipment and objects of research on a par: "My argument suggest[s] [..] that supposedly present-at-hand things like electrons are ontologically no different from hammers. To be an electron is to belong likewise to a configuration of things we can intelligibly encounter in our purposive practices".[35] He explains this parallel more fully in another passage: "There must be nails for there to be hammers; for there to be electrons, there must be such things as atoms, on the one hand, and cathode-ray tubes on the other. That is, there must be things that they interact with and the equipment that enables us to interact with them".[36] I would like to insist on the difference in ontological status between electrons on the one hand, and hammers, nails and cathode-ray tubes on the other, if only to avoid the conflation of ontology and epistemology which Rouse commits in the second passage.[37] Let us briefly review some examples from previous chapters to see if this distinction can be sustained. The chemical substances synthesized by Ehrlich (including the drug salvarsan) can be considered physico-cultural objects - because

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34 The notion of 'physico-cultural objects' has been derived from Herman Koningsveld's notion of 'bio-cultural concepts' which refer to such things as dairy-cows, breeding-stallions, fatting-calves, crops, weeds etcetera as used in agriculture and the agricultural sciences. See H. Koningsveld, 'Cognitive and Social Factors in Agricultural Science', *Methodology and Science* 23 (1990): 142-55.

35 J. Rouse, op. cit. (note 21), p. 158.

36 Ibid., p. 160.

37 Rouse himself appears to be not fully consistent, because on p. 157 he states: "Electrical currents came into existence at the turn of the century, and they did so because human beings constructed them. But this could be done because there were already electrons [..]". So here the existence of electrons is not made dependent on configurations of things organized in human practices!

of the human involvement in the synthesis of these substances and the imposition of a role or function they have to fulfil in human endeavours. On the other hand it is clear, I think, that the causative agent of syphilis, *Spirochaeta pallida*, does not belong to the cultivated part of nature (and not merely because the attempted 'cultivation' on nutrient media did not actually succeed in this particular case!). The case of the Wassermann reaction is somewhat difficult, but this is because the designation itself is ambiguous. If the term is read as synonymous with 'Wassermann *test*', then the Wassermann reaction clearly is a physico-cultural object. If the designation is however taken as referring to a particular immune reaction occurring in syphilitic patients, apart from any experimental setup, then the reaction belongs to the non-cultivated part of nature. The strains of trypanosomes figuring in Ehrlich's chemotherapeutic programme offer another interesting example; they were used as *research tools* (particularly after they had been bred with various profiles of resistances) and in this capacity they qualified as physico-cultural objects.[38] The mere fact of being an object of human cognition does not suffice, in my view, for being considered part of cultivated nature. To count as culturally appropriated, a natural object has at least to be assigned a function in some human project. Often, such assignment is accompanied by attempts to reconstruct or reshape the object ('construction' in Sismondo's sense [c]) in order to ensure that the latter will more adequately fulfil its assigned function.

Further questions and corollaries

One corollary from the foregoing analysis is that insights gained from the sociology of science may not be completely transferable to the sociology of technology. The suggestive phrase 'the social construction of facts and artefacts' introduced by Trevor Pinch and Wiebe Bijker conceals essential differences in the way scientific facts and technical artefacts are being 'constructed'.[39] The same is true of Latour's adoption of the term 'technoscience' which is explicitly intended to blur the distinction between science and technology. I admit that it is usually not very clear in concrete cases where exactly the boundary line between the two has to be drawn. For some purposes it might indeed be acceptable to treat science and technology as a kind of Siamese twin, Science&Technology, leaving undecided where the one ends and the other starts. There is however a real danger that different meanings of 'construction' - especially meanings (c) and (d) of Sismondo's list - will be lumped together. Then the implausible sense (d) of 'construction' can evade critical scrutiny by riding

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38 One might suggest that spirochaetes also played the part of research tools when syphilitic livers containing masses of spirochaetes were employed as a source of antigen in the initial attempts of Wassermann and his co-workers to apply the complement fixation test to syphilis. I have no principled objection to this suggestion.

39 T.J. Pinch and W.E. Bijker, 'The Social Construction of Facts and Artefacts: Or how the Sociology of Science and the Sociology of Technology might Benefit each other', *Social Studies of Science* 14 (1984): 399-441.

piggyback, as it were, on the quite acceptable meaning (c). We have already seen that this is indeed what happens in Knorr-Cetina's and Latour's writings.

Another matter to be re-examined is whether the so-called 'Bachelardian challenge' still stands as a powerful challenge to realist interpretations of science. The question raised was how scientific knowledge can be about a human-independent reality, if most concrete realizations of experimental phenomena do not exist in nature until they are artificially produced by human beings.[40] I have already made it clear that, in my view, the latter claim should not be construed as asserting that e.g. electrons would not exist were it not for the manufactured existence of cathode-ray tubes (Rouse's example) or that the hormone TRF would not exist were it not for the existence of bio-assays (an example of Latour and Woolgar). Realists may point out that by creating 'abnormal' circumstances in the artificial setting of the laboratory we just produce special effects which reveal the underlying capacities and powers of natural substances and the like.[41] This answer is acceptable but does not remove the full force of the challenge. We are still confronted with the weighty problem of how the very special insights gained in artificial laboratory settings can be transported to 'natural' non-laboratory settings. The philosopher of science, Nancy Cartwright, formulated what amounts to a new version of the challenge for the case of physics, which can probably be extended to other sciences:

> "Do the laws of physics that are true of systems (literally true, we may imagine for the sake of argument) in the highly contrived environments of a laboratory [..], do these laws carry across to systems, even systems of very much the same kind, in different and less regulated settings?".[42]

Cartwright gives several reasons for holding that such carrying-over is far from obvious. Latour's question - how does the science get out of the laboratory? - still stands, although we have seen in Chapter VII that his own answer cannot be the full story.[43] Much more was involved in the clinical introduction of salvarsan than a simple transference of the conditions of Ehrlich's laboratory to the 'wider' society. To understand this process I used additional insights derived from other constructivists. Constructivist analyses do have

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40 Radder, op. cit. (note 6), p. 74 and p. 77.

41 See Nola, op. cit. (note 26), p. 698.

42 N. Cartwright, 'Fundamentalism vs the Patchwork of Laws', in D. Papineau (ed.), *The Philosophy of Science* Oxford (Oxford University Press), 1996, pp. 314-26, on p. 316.

43 Radder, op. cit. (note 6), pp. 88-89, argues that the non-trivial achievement of reproducability of material realizations of experimental phenomena points to the existence of human-independent potentialities in nature. He also claims that this circumstance refutes Latour's solution (laboratory conditions are simply transplanted to the outside world) to the problem of the applicability of laboratory findings.

something to offer to highlight the difficulties that must be confronted when laboratory products are to find their way into the outside world.

The final matter to be considered here is the accusation that constructivist analyses do not allow to draw a distinction between facts and artefacts (in the sense of spurious phenomena or results) or even worse, that they effectively degrade all facts to artefacts. If facts are mere (social) constructions, then in what respect do they differ from artefacts? I think a moderate constructivist can respond to this charge by conceding that there is indeed a meaningful distinction to be drawn between facts and artefacts. He or she would however reject the claim that it is the duty of the analyst of science to tell fact from artefact in concrete cases (that is the job of the scientists themselves!) or even to provide general standards and criteria by which this distinction can be made in actual scientific practice. It is true that some philosophers of science (e.g. Allan Franklin) have proposed such standards and criteria, but constructivists will be quick to point out that the application of these standards usually leaves considerable scope for indeterminacy. In Chapter IV we have seen, for instance, that appeal to the methodological principle of agreement between different observation procedures was unable to resolve the controversy about the factual or artefactual nature of the pale spirochaete. Nicolas Rasmussen's historical study of the so-called 'mesosomes', identified during the 1950s with the aid of the new electron microscope, is even more telling.[44] It took about 15 years before these purported cell structures were called into question as being mere artefacts (although even now there are still researchers who are convinced of their factual status!). This long period militates against the idea that science disposes of readily applicable criteria for telling fact from artefact. Rasmussen also shows in detail that researchers often disagreed about the relative importance of the various criteria that could be brought to bear on the issue; and even where they agreed on the principles to be applied, they disagreed about their concrete application! It would thus seem that there is indeed wide scope for a constructivist analysis to handle the social construction of facts and the (de)construction of artefacts simultaneously using a single analytical framework. This is precisely what was attempted in Chapter IV. The possibility of such an analysis is a direct corollary of the symmetry principle. For purposes of analysis, facts and artefacts are put on a par and treated symmetrically, but this does not mean that the former are transformed into the latter.

2. The constitution of the social

When Fleck proclaimed that cognition is "the most socially-conditioned activity of man" (42/58), he was not in doubt about the meaning and import of the process of 'social

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[44] N. Rasmussen, 'Facts, Artifacts, and Mesosomes: Practicing Epistemology with the Electron Microscope', *Studies in History and Philosophy of Science* 24 (1993): 227-65.

conditioning'. Things are different for the adherents of modern forms of constructivism. The precise meaning of 'the social' and the acceptability of the very idea of social determination of scientific knowledge are hotly debated issues. In a recent dissertation Rob Hagendijk even noted that "the problems associated with constructivist approaches have more to do with general problems of sociological analysis and social theory than with the nature and organization of science".[45] As with the problem of realism, the range of available positions can be reduced to manageable proportions by distinguishing between moderate and radical varieties of constructivism. Whereas the former generally attempt to uphold established conceptions of the nature of social reality and their relevance for explaining (variations in) knowledge, the latter assert that there are no pre-given and fixed social structures which could be taken as analytical resources for explaining what goes on in scientific practice. Just as in the previous section, I will clarify and develop my own view on these issues by critically confronting the various positions that are represented in the field of constructivist science and technology studies. In the process I will also draw on the empirical analyses reported in previous chapters, which have been inspired by insights derived from both moderate and radical constructivists.

Before entering into the issues which divide the ranks of constructivists, it will be helpful to indicate the area of agreement. Moderate and radical constructivists alike reject the view that scientific knowledge can be established by the solitary investigator working on his or her own. As the moderate constructivist Steven Shapin declares: "What counts for any community as true knowledge is a collective good and a collective accomplishment. That good is always in others' hands, and the fate of any particular claim that something 'is the case' is never determined by the individual making the claim".[46] It would seem that the radical constructivist Bruno Latour largely agrees, for he pronounces the following first principle of his actor-network approach: "The fate of facts and machines is in later users' hands; their qualities are thus a consequence, not a cause, of a collective action".[47] Of course, the happy agreement between moderate and radical constructivism immediately ends when it comes to specifying the nature of this collective action.

My discussion will centre on the following series of critical challenges and objections which have been raised by radical constructivists with regard to the treatment of 'the social' by moderate constructivists:

(1) Science and technology cannot be explained from given social structures, because the latter are themselves changed through the activities of scientists and technologists (Latour);

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45 R. Hagendijk, *Wetenschap, Constructivisme en Cultuur*, Amsterdam (thesis), 1996, p. 267.

46 S. Shapin, *A Social History of Truth: Civility and Science in Seventeenth-Century England*, Chicago and London (University of Cicago Press), 1994, p. 5.

47 Latour, op. cit. (note 15), p. 259.

(2) Reflexivity demands that the social be taken not as an explanatory resource but as a topic of inquiry (Woolgar, Knorr-Cetina);
(3) Moderate constructivists illegitimately assume that the social validation of knowledge-claims occurs in relatively self-enclosed 'scientific communities' demarcated along disciplinary lines (Rouse);
(4) There is no social locus for the validation of knowledge through consensus formation above the level of the laboratory; the selective incorporation of previous results into ongoing laboratory research is all there is to the process of 'solidifying' results (Knorr-Cetina);
(5) In attempting to establish the social determination of scientific knowledge moderate constructivists treat individuals as passive automatons, 'cultural dopes' or 'interest dopes' rather than as knowledgeable and capable actors (Woolgar).

Given my sympathies with moderate constructivist views as shown in the preceding section, it is perhaps not surprising that I will reject most of the arguments advanced by radical constructivists. On certain issues, however, I will make limited concessions to their point of view. I think radical constructivists have developed some special insights which can be usefully adopted by moderate constructivists without the latter being forced to swallow the radical skepticism vis-à-vis the social. (Indeed I have used such insights myself in previous chapters.) I also believe that answering the radical constructivist challenges and objections to the use of social categories in science and technology studies does not exhaust the issues. In the concluding and more constructive part of this section I will therefore take Anthony Giddens's so-called 'structuration theory' as a starting point for developing an integrative conception of social structure, agency and (scientific) practice.

Latour's argument against social explanations

In several of his works Latour wages a relentless battle against 'social' explanations in science and technology studies. In the same book from which the above-quoted principle derives, he also formulated the following rule of method (rule 4): "Since the settlement of a controversy is the *cause* of Society's stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enrol human and non-human resources".[48] In Latour's view, facts (and technical artefacts) are 'co-produced' along with their corresponding social structures in the constructive practices of technoscience. Thus George Eastman invented, simultaneously with his simple black-box camera, the social group of amateur photographers ('You press the button, we do the rest', or in French: 'Clic, clac, merci Kodak'). Another famous 'network builder' or 'society builder' was Louis Pasteur, the hero of Latour's book *Les Microbes*, of which the English translation expresses Pasteur's 'society building' impact: *The Pasteurization of France*. It was

[48] Ibid., p. 258.

especially in the French overseas colonies, according to Latour, that the 'Pasteurians' (microbiologists and immunologists) could exhibit their veritable society-building powers as they met with less resistance than in France itself. He thus rejects the 'social' explanations that have been given of the emergence of tropical medicine around 1900 on the basis of the colonial interests that were dominant at the time (in Chapter VII I explained the initial focus of Ehrlich's chemotherapeutic programme on trypanosomes in the same way).[49] Without the 'Pasteurians', Latour argues, there would have been no colonial society at all:

> "With only whites and blacks [..] that Colonial Leviathan which spread across the globe could never have been built. Nor can the colonial medicine of the Pasteurians be explained in terms of 'society' and its 'interests', since the Pasteurians were capable, once more, of moving their programs of research sufficiently to obtain a richer definition of society than all the exploiters and exploited of the period".[50]

In my view, there is nothing wrong with explaining the rise of tropical medicine from the social interests associated with colonialism. The explanation may not be too spectacular, but it is at least quite plausible. Although I admit that Latour raises an interesting point, I also think that his argument is not free from rhetorical exaggeration. It may be conceded that the 'Pasteurians' contributed to building colonial society, but did they build such a society single-handedly from scratch? Moreover, at the end of the 19th century the colonial movement was a powerful social current *in the home countries*. It was not invented by the 'Pasteurians'. Why should the existence of such a powerful movement be deemed irrelevant for explaining the great interest in tropical medicine that was exhibited at that time?[51]

I think we can do justice to Latour's insight that technoscientists often act as 'society builders' without ipso facto subscribing to his conclusion that 'social' explanations are therefore to be banned. The interventions of technoscientists may be of lesser or wider scope but in principle they are always aimed at particular social relationships or aspects of social life and never affect the structure of society as a whole.[52] Perhaps it would be better to replace the term 'society builders' by the more modest designation 'agents of social change' to make clear that no totality claim is involved. Precisely because technoscientists are no

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49 M. Worboys, 'The Emergence of Tropical Medicine: a Study in the Establishment of a Scientific Specialty', in G. Lemaine et al. (eds.), *Perspectives on the Emergence of Scientific Disciplines*, The Hague (Mouton), 1976, pp. 75-98.

50 B. Latour, *The Pasteurization of France*, Cambridge, MA, and London (Harvard University Press), 1988, p. 144.

51 For an in-depth examination of the extent to which Western 'imperial' concerns have shaped tropical medicine in the case of schistosomiasis or bilharzia, see J. Farley, *Bilharzia: A History of Imperial Tropical Medicine*, Cambridge (Cambridge University Press), 1991.

52 Why 'holistic social engineering' in the literal sense is impossible in principle is explained in K.R. Popper, *The Poverty of Historicism*, London and Henley (Routledge & Kegan Paul), 1961 [1957], esp. pp. 67-70.

'holistic social engineers' à la Popper, it still makes sense to interpret their social interventions against a background of stable social structures which are not affected by their overhauling efforts. As an example we can turn to the creation of high-yielding and 'economically sterile' varieties of maize, so-callled 'hybrid corn', by American plant geneticists during the 1920s and 1930s.[53] By creating these varieties the plant geneticists acted as agents of social change, for they thus helped create a socio-economic order which offered ample room for a thriving private seed sector and in which maize farmers were also willing to abandon their cultural 'prejudice' against yearly seed purchases. On the other hand, their activities did not take place in a social void. Their choice of a 'hybridization' breeding strategy (rather than of an equally effective alternative which would not have resulted in 'economically sterile' varieties) can be seen as informed by the dominant socio-economic interests and political power-relationships of the 'wider' society. In general, we can admit the idea that scientists and technologists often act as agents of social change without thereby being forced to give up the possibility of social explanations.

From 'resource' to 'topic'?

In criticizing social explanations Latour uses a characteristic mode of argumentation that is also employed by other radical constructivists. When moderate constructivists appeal to, say, interests or social structures, to explain the content of knowledge, their radical colleagues will typically react by claiming that these factors cannot be seen as explanatory but must themselves be considered as in need of explanation. In other words, their strategy is to turn every explanatory *resource* that is on offer into a *topic*. Sometimes this required problem shift is argued by invoking the demand of reflexivity. Thus in an interview Knorr-Cetina declared:

> "In social constructivism [that is, moderate constructivism - HvdB], social reality is used as a resource. Sociologists use the resource for studying natural-scientific facts and natural science. But it's never used as a topic. The construction of sociology - that is, of sociological categories, sociological texts, etc. - and of social reality in general is not studied and documented in the same way as natural-scientific facts are studied and documented. *Now what we have to do, of course, is to be reflexive enough to apply the constructivist program and method also to our own resources* [emphasis added - HvdB]".[54]

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[53] The example has been described in J.R. Kloppenburg Jr., *First the Seed: The Political Economy of Plant Biotechnology*, Cambridge (Cambridge University Press), 1985. Kloppenburg's work has been used to make some points against Latour's actor-network theory in H. van den Belt, 'How to Critically Follow the Agricultural Technoscientists: Kloppenburg versus Latour', in The Agrarian Questions Committee (eds.), *Agrarian Questions: The Politics of Farming anno 1995. Proceedings*, Volume 1, Wageningen, The Netherlands, 1995, pp. 43-53.

[54] W. Callebaut, *Taking the Naturalistic Turn: or How Real Philosophy of Science is Done*, Chicago and London (University of Chicago Press), 1993, p. 117.

Here, I will not engage in a debate on reflexivity. It is clear, however, that a consistent implementation of the strategy of turning resources into topics will ultimately leave the analyst empty-handed. In the end, we will have a lot of phenomena to be considered as *consequences*, but no longer any *causes* to explain them. It seems that this is precisely the fate that has befallen Latour's actor-network theory. Kyung-Man Kim spells out this criticism in the following passage:

> "Latour's network is composed not only of scientists but also of nearly everyone and everything that comes into contact with them - financiers, politicians, the public, and machines, and so on. This means that, in contrast to traditional sociological practice, we can no longer treat science and society separately. One important consequence of such a simultaneous determination of the natural (scientific) and social order is that we cannot use society (or social factors) to account for the closure of a scientific controversy. For Latour, just as Nature is not the cause but the 'consequence' of the settlement of the controversy, Society is not the cause but the consequence of the settlement of the controversy. *However, in rejecting both Nature and Society as 'causal factors', Latour fails to provide us with an alternative theory which can account for the realignment of allies - or [..] the 'restructuring' of the network of allies* [my emphasis]".[55]

In the preceding section I argued against the radical constructivist view of Nature as a consequence of science and technology. Now I object to the view which considers Society as (entirely) created by science and technology. 'Technoscience' is not the extramundane and extrasocietal demiurge of natural and social reality.

Are scientific communities treated as self-enclosed?

The American philosopher Joseph Rouse opposes what he calls the theme of "the openness of scientific practices" (presumed to be characteristic of the so-called cultural studies approach of science and technology) to "a widespread sense of scientific communities as relatively self-enclosed, homogeneous, and unengaged with other social groups or cultural practices" which he sees exemplified in the work of Thomas Kuhn and of his moderate constructivist successors like Harry Collins and David Bloor: "The social constructivist tradition [that is, moderate constructivism - HvdB] has [..] focus[ed] on either the social interests or the social interactions that constitute the shared beliefs, values, and concerns of scientific communities".[56] He contrasts this attributed view with Knorr-Cetina's early notion of 'transscientific fields' (or 'transepistemic fields'), with Latour's statement to the

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55 K.-M. Kim, 'Natural versus Normative Rationality: Reassessing the Strong Programme in the Sociology of Knowledge', *Social Studies of Science* 24 (1994): 391-403, on p. 403.

56 J. Rouse, *Engaging Science: How to Understand its Practices Philosophically*, Ithaca and London (Cornell University Press), 1996, p. 249; in note 34 on the same page he specifies his charge: "prominent examples of social constructivists who emphasize the role of relatively self-enclosed scientific communities or forms of life include Collins 1992 (especially his notion of a 'core set') and Bloor 1983".

effect that scientific work itself destabilizes any distinction between inside and outside, and with the many studies which have shown "a constant traffic across boundaries that allegedly divide scientific communities (and their language and norms) from the rest of culture".[57]

Rouse's criticism of moderate constructivism, which runs together several different charges, is rather astonishing. When constructivist studies, inspired by the Edinburgh Strong Programme, first appeared in the domain of the history of science during the 1970s, they were identified by so-called 'internalist' historians as simply a new edition of the old 'externalist' historiography, that is, they were condemned for paying too much attention to social, economic and cultural factors external to the proper sphere of science instead of giving due emphasis to the purely intellectual factors presumed to guide its development. And now, oddly enough, Rouse comes to mount precisely the opposite charge! What, then, are we to make of this criticism?

Under the circumstances we could do worse than recall an early but quite balanced view of the matter expressed by one of the two founding fathers of the Strong Programme. Already in 1974 Barry Barnes wrote the following statement of principle:

> "From [our] perspective [..], the extent to which scientific change is determined or influenced by 'external' factors is a contingent matter, requiring separate investigation for every particular instance. Science is a part of culture like any other. To the extent that actors define it as a bounded set of meanings, beliefs and activities with an 'inside' and an 'outside', we may inquire about the strength of the boundary, and the degree to which external determinants operate across it. There is no reason why such determinants should be regarded as absent, or as all important [..]".[58]

From a (moderate) constructivist perspective the drawing of boundaries, both between science and the rest of culture and within science between various (sub-)disciplines, is a contingent social activity.[59] In his 1974 book Barnes was advocating more historical and sociological research into the constitution and effects of such boundaries.[60] It may be difficult or virtually impossible to understand intellectual debates of previous centuries through the prism of our 20th-century disciplinary boundaries. Rather than projecting our ideas of 'proper' boundaries back into historical time, we should try to understand how actors in the past

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57 Ibid., pp. 249-50.

58 B. Barnes, *Scientific Knowledge and Sociological Theory*, London (Routledge & Kegan Paul), 1980 [1974], p. 99.

59 See also Barnes et al., op cit. (note 29), Chapter 6: Drawing Boundaries.

60 A famous study fulfilling Barnes's desiderata is S. Shapin and S. Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, Princeton (Princeton University Press), 1985. It deals with Robert Boyle's demarcation of proper questions of 'natural philosophy' (experimental science) from questions of church and state, which demarcation was contested by Thomas Hobbes. See also S. Shapin, 'Discipline and Bounding: The History and Sociology of Science as Seen through the Externalism-Internalism Debate', *History of Science* 30 (1992): 333-69.

themselves defined and demarcated their activities. Thus when the historian Robert Young attempted to reconstruct the Victorian debate on "man's place in nature", he could not limit his inquiries to any of the currently established fields of geology, biology or psychology but had to locate the debate in a "common context" of Victorian intellectual life borne by the leading periodical journals of the time which also gave pride of place to such a by now extinct entity like natural theology.[61] All this did not preclude that during the last two decades of the 19th century the "common context" would become fragmented; by then the boundaries around such disciplines like geology, biology and psychology as we know them today had been more rigidly drawn. Indeed, Barnes and other moderate constructivists hold that the standard pattern for science in the 20th century is to be organized and conducted in clearly demarcated sub-cultures of disciplines and specialties.[62]

Although it may be possible that moderate constructivists overestimate the strength and rigidity of the boundaries around contemporary disciplines and specialties, this is just a matter of empirical contingency, not of theoretical principle. In his criticism Rouse appears to assume that the existence of boundaries precludes any traffic across them. This assumption is clearly unwarranted. Documenting the amount of two-way traffic that constantly occurs between scientific communities and the rest of culture provides no compelling argument against a division between 'inside' and 'outside', if such a distinction is actively maintained by the actors involved. Indeed, moderate constructivists have themselves gathered extensive evidence on this score. Insofar as Rouse's argument tends to deny any distinction between scientific disciplines and the surrounding culture, he should ask himself if he is still entitled to refer to the former by using their conventional designations. But perhaps the main thrust of his argument is that the coherence of scientific fields should not be thought of in terms of shared values and beliefs. Such a charge would probably apply to Kuhn's idea of a scientific community as a paradigm-sharing collective, but less so, I think, to moderate constructivist views.[63] Anyway, the argument remains too underdeveloped to have much critical force. Elsewhere in his book Rouse opposes the notion of 'background knowledge' as a network of widely accepted or presupposed beliefs or of previously justified sentences and presents an alternative view:

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[61] R.M. Young, 'Natural Theology, Victorian Periodicals and the Fragmentation of a Common Context', in C. Chant and J. Fauvel (eds.), *Darwin to Einstein: Historical Studies in Science & Belief*, Harlow (Longman), 1980, pp. 69-107.

[62] Barnes, op. cit. (note 58), p. 121.

[63] Harry Collins explicitly denies the view Rouse attributes to him: "The set of allies and enemies in the core of a controversy are not necessarily bound to each other by social ties or membership of common institutions [..] This set of persons does not necessarily act like a 'group'. They are bound only by their close, if differing, interests in the controversy's outcome. I refer to such a set of allies and enemies as a 'core set'." See H.M. Collins, *Changing Order: Replication and Induction in Scientific Practice*, Chicago and London (The University of Chicago Press), 1992 [1985], pp. 142-43.

"Instead, my claim will be that the 'field' within which scientific claims and practices acquire their significance and justification involves many things that cannot be reduced to sentences or beliefs 'internal' to a domain of investigation: skills and techniques, instruments and material systems (including networks for their manufacture and supply), availability of resources (money, of course, but also staff, information, an audience, and so on), institutional structures, relevance to other social practices or political concerns, and much more".[64]

It is interesting to observe that all of the various factors listed by Rouse also loom large in my own historical reconstructions of the development of the Wassermann reaction and of the design of a chemotherapeutic drug against syphilis (Chapters V and VII). Still, as a proposed 'theory' about how scientific claims and practices "acquire their significance and justification", I think Rouse's suggestions are much too diffuse to be useful. Merely enumerating a motley of elements that allegedly compose a particular 'field' does not define the coherence of that field.

In the historical chapters on the Wassermann reaction and on salvarsan the analysis was deliberately not confined to what happened within the bounds of a scientific community or collective demarcated along disciplinary lines. In the case of salvarsan that would have been hardly possible because there was no disciplinary community devoted to the pursuit of 'chemotherapy', Ehrlich and his team being virtually unique in this regard. Indeed I found it useful to invoke Knorr-Cetina's notion of 'transscientific field' or 'transepistemic field' to highlight the *resource relationships* (chemical substances, money, experimental animals) between Ehrlich's team and the chemical industry and other actors. Although in the case of the Wassermann reaction there was the nascent (sub)discipline of serology, it would have been inadequate to give an account of the development of this serodiagnostic test as if it occurred exclusively within the confines of the so-called "serologists' collective" - this was precisely my criticism of Fleck's analysis. If, to paraphrase Latour, the fate of a claim is in the hands of later users, then whoever claims to have developed a new diagnostic test or a new medicine for a particular disease will have to convince (the relevant segments of) the medical community of their usefulness and reliability. In other words, he or she has to 'colonize the minds' of the prospective users or - in Latourian terminology - to enroll, capture and translate their interests. It is true that in this process all the elements mentioned by Rouse may have a part to play. It is equally true, however, that in our case studies there was no serious doubt about the social identity of the prospective users: they were, first and foremost, the clinical specialists charged with the diagnosis and especially the treatment of syphilitic patients. There was in place, then, a clear 'social forum' for assessing the claims put forward by Wassermann and Ehrlich and their co-workers - at the demand side rather than at the supply side, so to speak.

The social locus of validation

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64 Rouse, op. cit. (note 56), p. 188.

A prominent representative of ethnographic lab studies, Karin Knorr-Cetina was confronted from the outset with the criticism that focusing on what happens within the scientific laboratory is not the most suitable approach for investigating how scientific results become validated and accepted. Validation and acceptance requires, so the criticism goes, a process of consensus formation involving a larger scientific community. To defend the value of the lab studies approach, Knorr-Cetina countered this criticism by asserting that it wrongly assumed validation and acceptance to be a process of opinion formation separate from and therefore not located within the actual research process. "But where do we find the process of validation, to any significant degree", she asked rhetorically, "if not *in* the laboratory itself? [..] What *is* the process of acceptance if not one of selective incorporation of previous results into the ongoing process of research production?".[65] And in a more recent interview she explained: "One has to get away from a conception of consensus formation as essentially a process of *argumentation*, as a sort of mentalistic process that involves the discussion of many scientists among each other. One has to see it much more as a *material* process [..]".[66]

I think Knorr-Cetina's argument for the self-sufficiency of the laboratory setting is singularly unsuccessful. It is true that laboratory outcomes will often become incorporated in further research by the same laboratory, but usually not before having been authenticated, as it were, by the relevant scientific field or community. A laboratory is not a self-enclosed world but operates in what Latour and Woolgar referred to as an 'agonistic field' also consisting of other laboratories.[67] It publishes its 'findings' with a view to their being accepted by 'the field'. If it tries to skip this process of social acceptance and 'prematurely' reincorporates those findings in new research processes, it will surely run a greater risk that the next series of results to be produced will be rejected by other laboratories. At times Knorr-Cetina herself reluctantly recognizes the relevance of the scientific field: "Many processes of consensus formation would seem to involve more than one laboratory, and potentially a whole scientific field".[68] Her reluctance appears to derive from an aversion to the notion of consensus formation as a process of argumentation. It is not necessary, however, to construe this process in a rationalist manner as the straightforward application of universal criteria of appraisal leading of themselves to definite outcomes. Sociologists of scientific knowledge have brought the received philosophical notion of a 'context of justification' down to earth by admitting persuasion and negotiation among its constituent processes. To me it appears that one can hardly do without some such sociologized version

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65 K.D. Knorr-Cetina, *The Manufacture of Knowledge*, Oxford (Pergamon Press), 1981, p. 8.

66 Callebaut, op. cit (note 54), p. 305.

67 Latour and Woolgar, op. cit. (note 8), p. 237.

68 Knorr-Cetina, op. cit. (note 19), p. 161.

of the idea of consensus formation among scientists, unless one wants to give up accounting for the way knowledge-claims become accepted and facts get stabilized. Indeed, even the radical constructivist 'splitting-and-inversion model' holds that statements begin to refer to an independent reality as soon as controversies are resolved and thus assumes that processes of consensus formation take place.

Are scientists and other actors made out as 'cultural dopes'?
The criticism is sometimes raised, especially by authors who have been influenced by ethnomethodology, that the very attempt to demonstrate the 'social conditioning' or social determination of scientific knowledge involves the portrayal of scientists and other participants as passive automatons or mere puppets of underlying social and cultural forces - or, in Harold Garfinkel's terminology, as 'judgmental dopes' or 'cultural dopes'. This is held to be inadmissable, because human agents are considered to be active rather than passive. They should accordingly be treated as 'knowledgeable and capable actors'.

This particular criticism deserves close examination. It seems to fit some of Fleck's formulations in which scientists are made out as the 'judgmental dopes' of a compelling thought style. In Chapter VI we have also seen that the anti-individualistic tenets of his sociological approach were at least partly to blame for the inadequacies of his account of the struggle over the intellectual ownership of the Wassermann reaction. Thus the question arises whether similar criticisms can be levelled at Fleck's modern successors, especially the moderate constructivists. How much scope do they grant to the 'agency' of individual actors?

Woolgar has claimed that much of the sociology of scientific knowledge is characterized by what he calls an "over-interested conception of the scientist". Because scientists are depicted as acting (that is, producing knowledge) in response to their interests, he holds that they are actually treated as "interest-dopes".[69] Moderate constructivists reject this criticism. To account for a scientist's actions in the light of his or her interests is to highlight the goal-oriented character of scientific work. It is not to portray the scientist as an interest-dope passively responding to his or her interests.[70] Shapin too defends the (moderate) constructivist case against similar criticisms from rationalist philosophers:

> "[..] We are invited to conceive of 'social determination' as if it were a sort of mugging. But which model is it really that makes out actors as 'judgmental dopes'? In an instrumentalist perspective actors are seen to produce and evaluate knowledge against the background of socially transmitted knowledge and

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69 S. Woolgar, 'Interests and Explanation in the Social Study of Science', *Social Studies of Science* 11 (1981): 365-94, on pp. 374-75.

70 D. MacKenzie, 'Interests, Positivism and History', *Social Studies of Science* 11 (1981): 498-504, on p. 502. See also B. Barnes, 'On the "Hows" and "Whys" of Cultural Change (Response to Woolgar)', *Social Studies of Science* 11 (1981): 481-98.

according to their goals. *The role of the social, in this view, is to prestructure choice, not to preclude choice* [emphasis mine - HvdB]".[71]

Although moderate constructivists recognize the active role of human agents, they do not want to inflate this agency to the point where the very idea of an influencing social order becomes vacuous. The latter tendency can be found with some radical constructivists. Thus several commentators have discerned a strong voluntaristic and machiavellian tenor in Latour's analyses of the pursuits of Louis Pasteur and other scientists.[72] Yves Gingras has criticized Latour's tendency to describe actors "as if they were absolutely free to move in any direction".[73] Why then, he asks, do some scientists or engineers go into the minister's office while others do not? (In France, engineers graduating from a *grande école* or from a *faculté* do not have the same possibilities of access to ministries.) In other words, Gingras argues that we should also keep an eye to the *structural constraints* under which actors are operating.

In my view, it would indeed be very desirable to have a balanced theoretical framework in which the agency of actors and the social constraints under which they act are both given their appropriate places. In my earlier comments on Latour's idea of technoscientists as 'society-builders' I intended to do justice to both aspects: as well as conceding their capacity for acting as 'agents of social change', I also stressed that such agency does not occur in a social void. Granting that an integrated theoretical conception reconciling agency and social constraints would indeed be desirable, the critical question, of course, is whether such a conception is feasible at all. In the final part of this section I will inquire if Anthony Giddens's 'structuration theory' offers a promising starting point to elaborate the desired framework.

Agency, social structure and (scientific) practices

The 'structuration theory' developed by Anthony Giddens represents a well-known, widely acclaimed (though also criticized) and sustained effort to overcome and reconcile established dualisms in social theory, such as the dualism between individual and society, voluntarism and determinism, or agency and structure.[74] At this point of my argument it may be useful

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71 S. Shapin, 'History of Science and its Sociological Reconstructions', in Cohen and Schnelle, op. cit. (note 28), pp. 325-86, on p. 370.

72 Hagendijk, op. cit. (note 45), p. 105 ff.

73 Y. Gingras, 'Following Scientists through Society? Yes, but at Arm's Length!', in J.Z. Buchwald (ed.), *Scientific Practice: Theories and Stories of Doing Physics*, Cichago and London (University of Chicago Press), 1995, pp. 123-48, on p. 138.

74 The main works in which the 'structuration theory' is developed are the following: A. Giddens, *New Rules of Sociological Method: A Positive Critique of Interpretative Sociologies*, London (Hutchinson), 1976; A. Giddens, *Central Problems in Social Theory: Action, Structure and*

to consider his theory to see whether it offers a viable approach to the theoretical dilemma sketched above. Other authors in the field of science and technology studies have already turned to Giddens's theory in the hope of finding an acceptable solution to similar dilemmas.[75] An additional reason for considering his theory is that it also promises to give us a conceptual handle on the notion of 'practice' or 'practices', a notion that has attained prominence in recent contributions to the constructivist literature (as transpires, inter alia, from the loudly proclaimed 'turn to practice'). Not surprisingly, perhaps, the term 'practice' is used in a wide variety of meanings and invested with rather different theoretical stakes and commitments. One author even speaks of "the Babel of practice".[76] It thus falls upon me to clarify in what sense and with what theoretical intention I have used the term in this book.

Giddens cuts through current dualisms in social theory and replaces them with a central insight of his own which he refers to as the notion of the *duality of structure*: "By the duality of structure I mean the essential recursiveness of social life, as constituted by social practices: structure is both medium and outcome of the reproduction of practices".[77] 'Structure', Giddens holds, should not be conceived of as a kind of framework like the skeleton of a body or the girders of a building; instead, it should be conceived of as the 'rules and resources' which are 'drawn upon' by actors in actual interaction. These structural elements have a 'virtual existence'; they exist only to the extent that they are instantiated in concrete practices. The paradigmatic example and source of inspiration for Giddens's conception of structure is the grammar of a language: the rules of grammar are drawn upon when the members of a linguistic community utter well-formed sentences (or perform speech acts). Grammar is thus constitutive of speech but is also reproduced by the series of speech acts which comprise daily life. The same is true for 'structure' in general, a circumstance that the phrase 'duality of structure' is precisely meant to capture. 'Structure' is thus both 'enabling' and 'constraining': it enables us to act but also places limits on possible courses of action. To complete this abstract summary of the broad outlines of Giddens's structuration theory, let me add that he does not present an extended discussion justifying his use of the notion of 'practices'. He simply links the concept to a Wittgensteinian notion of 'rules' and 'rule-following': "To know a rule, as Wittgenstein says, is to 'know how to go on', to know

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Contradiction in Social Analysis, London (Macmillan), 1979; A. Giddens, *The Constitution of Society: Outline of the Theory of Structuration*, Cambridge (Polity Press), 1984.

75 Hagendijk, op. cit. (note 45), pp. 125-156; W.E. Bijker, 'Do Not Despair: There Is Life after Constructivism', *Science, Technology & Human Values* 18 (1993): 113-38, on p. 123.

76 B.S. Baigrie, 'Scientific Practice: The View from the Tabletop', in J.Z. Buchwald (ed.), *Scientific Practice: Theories and Stories of Doing Physics*, Chicago and London (University of Chicago Press), 1995, pp. 87-122, on p. 88.

77 A. Giddens, *Central Problems in Social Theory*, London (Macmillan), 1979, p. 5.

how to play according to the rule. This is vital, because it connects rules and practices. Rules generate - or are the medium of the production and reproduction of - practices".[78]

Giddens's stress on the recursiveness of social life bears some resemblance to the emphasis on the 'self-referential' and 'performative' character of social institutions that is the hallmark of Barry Barnes's theory about the constitution of social reality.[79] According to Barnes's view, the nature of a 'social object' such as a social status or position is constituted by the surrounding context of belief and action. Thus, for example, "John is the leader of the gang [..] because the members know him to be the leader, and act routinely on the basis of what they know".[80] Or a certain piece of metal is money just because and insofar as the members of a particular society recognize it as such and accept it as a means of exchange for accomplishing their commercial transactions. Something similar holds for all social statuses and institutions. As a consequence, in Barnes's view, society appears as "a sublime, monumental, self-fulfilling prophecy".[81] This is in effect a particular way of formulating the recursive character of social life.

It would seem that Giddens has succeeded in elegantly combining the aspects of 'structure' and 'agency' in one unified theoretical conception. However, we should be wary about the precise terms under which this unification has been negotiated. Perhaps the happy reconciliation of both aspects is a little bit too smooth and too glib. At least that is what Giddens's critic John Thompson suggests. He argues that the theory of structuration one-sidedly emphasizes the 'enabling' rather than the 'constraining' side of structure. Thompson holds that this theory allows virtually no place to a notion of 'structural constraints'. Giddens claims that even a prisoner who is gagged and bound and placed in solitary confinement remains an 'agent', for the latter can still choose the option of a hunger strike or, ultimately, suicide. But if the definition of agency is stretched that far, then, Thompson concludes, "any individual in any situation could not *not* be an agent".[82] In this way the reconciliation between structure and agency has been achieved too easily. According to Thompson, there is not only complementarity but also tension between the two. As yet the theory of structuration is ill-equipped to accommodate this tension.

The probable source of the defect is Giddens's conceptualization of structure in terms of 'rules and resources'. Thompson directs his criticism to the first component, that of the

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78 Ibid., p. 67.

79 See B. Barnes, *The Nature of Power*, London (Polity Press), 1988, especially Chapter 2. On p. 166 Barnes himself recognizes that Giddens has moved "in much the same direction".

80 Ibid., p. 49.

81 Ibid., p. 52.

82 J.B. Thompson, 'The Theory of Structuration', in D. Held and J.B. Thompson (eds.), *Social Theory of Modern Societies: Anthony Giddens and His Critics*, Cambridge (Cambridge University Press), 1989, pp. 56-76, on p. 74.

'rules'. He holds that the reference to Wittgenstein does not remedy the essential vagueness of this key notion.[83] There are many different kinds of rules, each with different, more or less important, functions in social life. Giddens does not want to claim that all kinds of rules are equally relevant and important for describing social structure, but neither does he have a criterion of importance to select a relevant set of rules. Moreover, the notion of rules appears an insufficient basis for describing and analyzing the (prima facie) structurally important phenomenon of *differential* opportunities and restrictions among various social groups (such as differential access to higher education among different classes and genders). Nor are structural effects exclusively obtained by actors consciously 'drawing upon' rules. In sum, according to Thompson's criticism, the concept of rules fails to capture essential aspects of social structure.

Remarkably, Thompson devotes hardly any critical attention at all to the other component making up Giddens's notion of structure: resources. Giddens defines 'resources' somewhat obscurely as "the media whereby transformative capacity is employed as power in the routine course of social interaction".[84] He distinguishes two types of resources, 'authoritative' and 'allocative' resources: "By 'authorisation' I refer to capabilities which generate command over *persons*, and by 'allocation' I refer to capabilities which generate command over *objects* or other material phenomena".[85] There is a perplexing ambiguity in Giddens as to whether 'resources' refer to the 'command(s)' over persons and objects or to the 'capabilities' which (in the passage just quoted) are said to 'generate' such command(s) or to 'capabilities' which (in yet another passage) are said to 'arise from' such command(s).[86] I will return to this ambiguity in a moment. On the basis of his distinctions between two types of resource (authoritative and allocative) and between two types of rules (interpretative schemes and normative rules), Giddens distinguishes four basic institutional dimensions (or 'modalities of structure') which in his view are possessed by *all* social practices: symbolic (semantic), political, economic, and moral-legal dimensions. This entails an important instruction for the study of social practices: "[N]o social practice expresses, or

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83 Thompson could have strengthened this point by referring to the controversies about 'rule-following' in the exegetical literature on Wittgenstein, where we can distinguish between a 'skeptical' view (Kripke) and a 'non-skeptical' view (Baker and Hacker). This opposition has also found its way to science and technology studies, with David Bloor defending the skeptical and Michael Lynch defending the non-skeptical view. For the debate between the latter two, see A. Pickering (ed.), *Science as Practice and Culture*, Chicago and London (University of Chicago Press), 1992, pp. 215-300.

84 Giddens, op. cit. (note 77), p. 92.

85 Ibid., p. 100.

86 For critical comments on this ambiguity see T.R. Schatzki, *Social Practices: A Wittgensteinian Approach to Human Activity and the Social*, Cambridge (Cambridge University Press), 1996, p. 145 ff.

can be explicated in terms of, a single rule or type of resource. Rather, practices are situated within intersecting sets of rules and resources that ultimately express features of the totality".[87] Hagendijk has elaborated Giddens's 'four-dimensional' analysis of social practices for science considered as a practice.[88] In principle, science can be analyzed from a cognitive perspective (the symbolic dimension), from political and economic perspectives (e.g. Latour), or from a moral-legal perspective (e.g. Merton's work on the 'scientific ethos'), but the distinctive character of science as a practice (or institutional domain) is revealed only in the particular way in which cognitive, politico-economic, and normative elements are combined. The same holds true for other practices or institutional domains such as law, journalism and administration. For each practice a particular dimension may be of central importance (such as the symbolic dimension for science or the normative dimension for law) but the other dimensions are also relevant. It will be clear from this brief discussion that in Hagendijk's elaboration 'practices' are taken as more or less equivalent to 'institutional domains'.

Other elaborations of the notion of 'practices' turn out to be possible, however, when we scrutinize the idea of 'resources' more critically. Here I shall discuss two responses to the ambiguity inherent in Giddens's conceptualization. The first reaction comes from the American philosopher Theodore Schatzki. He denies that resources are ontologically 'on a level' with rules and asserts that power ultimately rests on rules, not on resources:

> "Suppose, for instance, that a boss, drawing on the authoritative resource to orchestrate employee's actions that is based on her identity and rights as boss, instructs an employee to post a letter by courier service and the employee complies. Her capacity to determine the employee's action rests on the codes and norms structuring business practices. This means, however, that her drawing on this authoritative resource really comes down to her drawing on the rules that structure the field of business interaction".[89]

The conclusion, Schatzki holds, can be generalized from this particular example to all cases involving a command over persons or things, so that, "to draw on a resource is at bottom to draw on some set of rules in a specific situation".[90] In this way, resources simply drop out as an independent element that, in conjunction with rules, structure practices. Consequently, the four fundamental dimensions of social practices formulated by Giddens are reduced to two: the semantic and the normative. The economic and political institutional orders are to be seen as particular assemblages of the semantic and normative dimensions.

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87 Giddens, op. cit. (note 77), p. 82.

88 Hagendijk, op. cit. (note 45), p. 147 ff.

89 Schatzki, op. cit. (note 86), p. 155.

90 Ibid.

(For Schatzki, practices are the basic and elementary building blocks of the social order and are logically prior to all other social formations.)

I think that Schatzki's decision to drop 'resources' from among the structuring elements of practices is a legitimate response, given the ambiguity in Giddens's formulations. It may not be the most attractive response, however. One might object that the very idea of a 'practice' essentially includes the commerce of human beings with each other and with material objects. Schatzki rests his case on the argument that to 'draw on' a resource comes down to 'drawing on' a rule or a set of rules. On some of Giddens's conceptualizations of 'resources', this argument may indeed be valid, but it does not hold for all possible definitions of 'resources'. In many cases, even where *access* to resources is regulated by social rules, 'drawing on' resources does not only mean 'drawing on' the rules determining access, but also involves *mobilization* of those resources in a more physical sense. This reasoning invokes a much more simple and mundane, less convoluted, conceptualization of 'resources' than that of Giddens. For this we can turn to the second response to Giddens's ambiguity, coming from the social historian William Sewell. The latter provides a simple translation of Giddens's obscure definition: "resources are anything that can serve as a source of power in social interactions".[91] He also replaces Giddens's distinction between 'allocative' and 'authoritative' resources by a fairly simple distinction between non-human and human resources:

> "Nonhuman resources are objects, animate or inanimate, naturally occurring or manufactured, that can be used to enhance or maintain power: human resources are physical strength, dexterity, knowledge, and emotional commitments that can be used to enhance or maintain power, including knowledge of the means of gaining, retaining, controlling, and propagating either human or nonhuman resources. Both types of resources are media of power and are unevenly distributed".[92]

Sewell also notes that Giddens failed to give clear content to the notion of rules, beyond the reference to Wittgenstein. For a richer vocabulary, Sewell turns to the detailed descriptions of 'the rules of social life' provided by cultural anthropologists. The latter have uncovered various conventions, recipes, scenarios, principles of action, and habits of speech and gesture involved in the daily life of people. Sewell proposes the designation 'cultural schemas' as a substitute for the term 'rules', because the latter term is too easily identified with publicly fixed *codifications* of rules. He agrees, however, with Giddens's definition of rules (or 'schemas') as "generalizable procedures applied in the enactment/reproduction of social

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91 W.H. Sewell Jr, 'A Theory of Structure: Duality, Agency, and Transformation', *American Journal of Sociology* 98 (1992): 1-29, on p. 9.

92 Ibid., pp. 9-10.

life".[93] The adjective 'generalizable' rightly points to the circumstance that cultural schemas can be extended and transposed to new contexts and new situations whenever the opportunity arises. It is precisely because of this *generalizability* or *transposability* that cultural schemas are considered to have a 'virtual' existence.

If rules or cultural schemas must be understood as virtual, what cannot be so understood, however, is resources. For non-human resources that is clear enough. Factories owned by capitalists, stocks of weapons in the hands of generals, land rented by peasants and the like can hardly be called 'virtual' because as material things they exist in time and space. The case of human resources is only slightly more difficult. Skills, physical strength, emotional commitments and knowledge, though not 'material' in the same sense as non-human resources, are 'resources' only insofar as they are incorporated or actualized in people's bodies and minds: "It is not the disembodied concept of the majesty of the king that gives him power, but the fear and reverence felt for him by his actual subjects".[94]

The fact that resources have to be understood as actual rather than virtual entails serious difficulties for Giddens's notion of structure. As a combination of rules and resources, 'structure' was considered to have a virtual existence. Now this view is no longer tenable. Nor can the famous idea of the duality of structure - with its 'virtual' order being actualized in and reproduced through concrete practices - be maintained in the same form. It is obvious that the situation calls for some drastic theoretical repair work. One possible response is to clean up Giddens's notion of structure by removing the element of resources. Structures, consisting only of cultural schemas, could then again be thought of as virtual. It would be necessary, however, to grant such structures some measure of causal power in animating and shaping actual arrays of resources. Sewell notes that resources, especially human resources, are indeed in some respects the manifestations and consequences of the enactment of cultural schemas. Thus the amount and kind of military power that a given number of enlisted men represents will depend on the currently established conventions of warfare (including chivalric codes), the notions of strategy and tactics and the training regimes that are available. Although non-human resources, as far as their material existence is concerned, cannot be seen as produced by cultural schemas, it is nevertheless true that their value as resources to a large extent also depends on the schemas that inform their use. Sewell offers a nice example borrowed from cultural anthropology to establish this point:

> "[..] an immense stack of Hudson Bay blankets would be nothing more than a means of keeping a large number of people warm were it not for the cultural schemas that constituted the Kwakiutl potlatch; but given these schemas, the blankets, given away in a potlatch, became a means of demonstrating the power

93 A. Giddens, *The Constitution of Society: Outline of the Theory of Structuration*, Cambrdige (Polity Press), 1984, p. 21.

94 Sewell, op. cit. (note 91), p. 10.

of the chief and, consequently, of acquiring prestige, marriage alliances, military power, and labor services [..]".[95]

So the option to retain a cleaned-up notion of structure (with schemas or rules, but without resources) that would restore its 'virtual' character is indeed feasible. Sewell nevertheless urges that we should not choose this particular option because it would represent a lapse into undiluted idealism. It would install mental structures (i.e. schemas) as the only form-giving instances and reduce human beings to "agents of these mental structures, actors who can only recite preexisting scripts"[96] or, as we could say, it would reduce them to 'cultural dopes'. Sewell therefore opts for a different solution, which at once is more materialist and also offers more scope for human agency than the rejected option.

The alternative solution Sewell proposes is to redefine the duality of structure as a duality of schemas and resources. According to him, structure has to be understood as consisting both of schemas, which are virtual, and of resources, which are actual. The duality is expressed in the fact that in some sense schemas are the effects of resources, while resources are also the effects of schemas. Often, the cultural schemas can be *read off* from the material form of the resources. Take the example of a factory: "The factory gate, the punching-in station, the design of the assembly line: all of these features of the factory teach and validate the rules of the capitalist labor contract".[97] On the other hand, to be reproduced over time, schemas must be supported and regenerated by resources which are deployed in their enactment. Without such support, schemas would wither away. Thus, sets of schemas and resources mutually imply and sustain each other over time and in this manner constitute structures.

The redefinition of the duality of structure as a duality of schemas and resources also gives rise to a more clearly articulated conception of *agency*. Conceiving of human beings as *agents* means, first, conceiving of them as *empowered* by access to resources of one kind or another. It means, second, conceiving of them as having the competence and capacity to apply existing cultural schemas to new cases in new contexts. Human beings have a generalized capacity for agency, Sewell holds, just as they have the capacity for respiration,

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95 Ibid., p. 12. The circumstance that the value of resources is largely dependent on the cultural schemas which inform their use has important corollaries for the meaning of sustainability. It implies that the attempt to measure the so-called 'environmental utilization space' (which would be available for exploitation without violating intergenerational justice) in a culture-free way is doomed to failure. On this question, see H. van den Belt, 'Measuring the Environmental Utilization Space: Natural or Social Limits?', paper presented at the July 1995 meeting of the International Society for Hermeneutics and Science, 12-15 July, 1995, Leusden, Holland. See also P. Thompson, 'Markets, Moral Economy and the Ethics of Sustainable Agriculture', in W. Heijman et al. (eds.), *Rural Reconstruction in a Market Economy*, Manholt Studies 5, Wageningen, the Netherlands, 1996, pp. 39-54.

96 Ibid.

97 Ibid., p. 13.

but this general capacity is is given a more specific form by the cultural schemas and resources that are available in their particular social environments (cf. the general linguistic capacity and the learning of specific languages). He also stresses that he understands agency, not merely as individual, but as profoundly social or collective.

This new conception of agency already transpired in the previously cited definition of cultural schemas or rules as "generalizable [or as Sewell prefers to say: transposable] procedures applied in the enactment of social life". "To say that schemas are transposable [..] is to say that they can be applied to a wide and not fully predictable range of cases outside the context in which they are initially learned".[98] It would seem that with this particular insight Sewell comes very close to the finitist theory of meaning which is such an important philosophical underpinning for the sociology of scientific knowledge.[99] In Chapter II we have seen that finitism too emphasizes the agency of human beings against any attempt to 'empower' rules or other cultural items. However, Sewell takes one further step which in my judgement is highly relevant for a theoretical understanding of practices in general and for scientific practices in particular. He points out that the transposability of cultural schemas implies the possibility of reinterpreting and remobilizing existing arrays of resources, precisely because resources instantiate and embody cultural schemas: "Any array of resources is capable of being interpreted in varying ways and, therefore, of empowering different actors and teaching different schemas. [..] Agency [..] is the actor's capacity to reinterpret and mobilize an array of resources in terms of cultural schemas other than those that initially constituted the array".[100] To return to the example of a factory: it may indeed embody capitalist notions of private property, as we saw earlier, but it may also - as Marx would argue - bring out the collective character of the labour process and thereby undermine those notions. One point that is not explicitly stated by Sewell, and that should be made explicit for the sake of having a more complete conception of practices, is that the 'remobilization' of resources often involves a *material transformation* of them as well. By taking this final step, we in fact complete the transposition of finitism from the domain of cultural schemas to the field of resources.

I believe that this kind of 'generalized finitism', inspired by Sewell's theory of the duality of cultural schemas and resources, offers a suitable synthetic framework for systematizing and assimilating the valuable insights that are implicitly or explicitly available in contemporary constructivist approaches to 'science-as-practice' (and it also provides a basis for rejecting the less valuable ideas proposed by these approaches!). Let me consider

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98 Ibid., p. 17.

99 In a footnote Sewell provides an interpretation of the verb 'transpose' which brings out this similarity with finitism more fully: "The verb 'transpose' implies a concrete application of a rule to a new case, *but in such a way that the rule will have subtly different forms in each of its applications*" (Ibid., p. 17, footnote; emphasis added).

100 Ibid., p. 19.

here the contributions of Andrew Pickering and Joseph Rouse. Pickering's theory of 'the mangle of practice' generalizes the Kuhnian and finitist idea of open-ended *modelling* so that it covers not just conceptual and theoretical change but also the modification of material procedures, the re-fitting of apparatus, and the like.[101] Indeed, Pickering defines 'practice' (as distinct from 'practices') simply though rather idiosyncratically as "the work of cultural extension and transformation in time", where 'culture' is taken to refer to the stock of 'resources' (conceptual, material, as well as social) existing at any one point in time.[102] Thus, scientific practice is considered as nothing more nor less than 'modelling' or the open-ended modification of material, conceptual and social resources over time. I would not characterize such a conception of scientific practice as 'posthumanist', although this is precisely the label Pickering wants to stick on it. The posthumanist tendency is even stronger in Rouse's case.

The theory of scientific practice(s) developed by Joseph Rouse also lays great theoretical store on the openness of practices: "[..] practices are radically open: whether a subsequent action counts as a continuation, transformation, deviation, or opposition to a practice is never fixed by its past instances".[103] Although this might be taken as closely resembling the 'finitist' emphasis on the open-ended character of concept application so typical of moderate SSK-type constructivism, Rouse deploys this argument precisely with the aim of combatting the latter. He takes the openness of practices to such extremes, that any consideration of possible social influences or even the attribution of agency to human beings (and not to things or animals) is already held to be an abrogation of this openness. His argument is that the alleged social factors ("or even their characterization as social") "may be what is at issue in the continuation of the practice"; similarly, "who or what can count as an agent is itself at issue within various practices and is not established by a timeless nature of agency or of the various kinds of putative candidates for agency".[104] I wonder whether Rouse seriously contemplates the possibility that, for instance, in the practice of experimentally testing new drugs, the laboratory rats might one day emerge as the true 'agents' and administer the chemical preparations to the human experimenters rather than vice versa. By going the 'posthumanist' road all the way down, Rouse risks ending up with a conception of practice in which human activity is no longer the central element, e.g. when he states that he understands practices "not as the doings of human agents but as the meaningful situations in

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[101] A. Pickering, *The Mangle of Practice: Time, Agency & Science*, Chicago and London (University of Chicago Press), 1995, p. 19.

[102] Ibid., pp. 3-4. 'Practices' (in plural), on Pickering's understanding, relate to "specific, repeatable sequences of activities on which scientists rely in their daily work" (p. 4) and are taken to be a component of 'culture'.

[103] Rouse, op. cit. (note 56), p. 141.

[104] Ibid., p. 141 and p. 143 (note 34).

which those doings can be significant".[105] In contrast to Rouse's view, Sewell's theory of agency, structure and practice remains firmly located on the 'humanist' side of the humanism-posthumanism divide. (Let me balance this criticism of Rouse's theory by also noting that it offered us the very useful notion of 'experimental systems' or 'phenomenal microworlds' as characteristic of laboratory work and the idea of the necessary interconnectedness of practices.)

The conception of practice based on the duality of cultural schemas and resources also offers a useful framework in which to integrate the historical analyses presented in Chapters V and VII. The reader will probably remember that my descriptions of the work of Wassermann's and Ehrlich's teams paid extensive attention to the material resources used in both endeavours. Indeed, the experimental practices of serology or chemotherapeutics could only start when all the necessary resources - raw materials, test animals, patient material, scientific and technical manpower - had been assembled. Cultural schemas were involved in recruiting these resources as well as in employing them effectively once they were in place. The acquisition of resources was often achieved through the market, i.e. by drawing on the rules of monetary exchange and contract law, or by more informal and complex deals (as between Ehrlich and chemical firms). In the latter case too, however, such deals were made possible thanks to the existence of intellectual property regulations concerning patents and the like. As Sewell rightly emphasizes, the core schema of capitalism - the so-called 'commodity form' - is unique in allowing for an almost universal interconvertibility of resources.[106] Latour and Woolgar's 'cycle of credibility' whereby scientific reputations are converted into additional funds (and the latter again into new results and into more prestige) is just an example of a particular circuit inscribed within this general circulation.[107] It was only because Ehrlich had already passed through this cycle several times with ever greater success that he was able to gather the funds needed to embark on his very costly chemotherapeutic programme. Once he had brought a complete array of material and human resources together under one roof, so to speak, he had to 'animate' these resources by drawing on the cultural schemas of the organization and management of 'scientific mass-labour' (just as in Sewell's example enlisted men and weapons are turned into military power by subjecting them to training regimes and drawing on notions of strategy and tactics). As I described in Chapter VII, Ehrlich had borrowed this organizational model and management pattern from the German synthetic dye industry, adapting them to the particular

[105] Ibid., p. 38. On p. 30, practices are described as "meaningful situations or configurations of the world".

[106] Sewell, op. cit. (note 91), pp. 25-26.

[107] In his essay 'Portrait d'un biologiste en capitaliste sauvage', Latour points out the similarity between the cycle of credibility within science and the circuit of capital accumulation as expressed in the Marxian formula M-C-M (Money-Commodity-Money); see B. Latour, *La Clef de Berlin*, Paris (La Découverte), 1993, pp. 100-129, esp. p. 125 ff.

needs of his chemotherapeutic practice as he went along. He thus exhibited 'agency' in his competence to transpose these cultural schemas from this particular industry to the new context of chemotherapeutics. It is also true, however, that the specific form which 'agency' assumed in Ehrlich's person had been shaped under the influence of the dominating presence of an advanced synthetic dye industry, to such an extent that Ehrlich considered himself to be a dye chemist *manqué*. In carrying on the practice of chemotherapeutics, he found himself condemned to rely heavily on this same industry for material and financial support. His goals could be reached only in intimate symbiosis with chemical firms, with all the dependencies and constraints thereby entailed.

The example of Ehrlich's work thus demonstrates Sewell's point that agency is of a thoroughly social nature:

> "[..] I do see agency as profoundly social or collective. The transpositions of schemas and remobilizations of resources that constitute agency are always acts of communication with others. Agency entails an ability to coordinate one's actions with others and against others, to form collective projects, to persuade, to coerce, and to monitor the simultaneous effects of one's own and others' activities".[108]

I do not entertain the illusion that the view stated in this passage is the definitive answer to the fundamental dilemmas about individual and society, or agency and structure, that continue to haunt the social sciences, but I do believe that it represents a quite reasonable and balanced position which is eminently worth defending.

108 Ibid., p. 21.

NEDERLANDSE SAMENVATTING (SUMMARY IN DUTCH)

[For an English summary, see Chapter I under the section headed 'Synoptic Preview']

In 1935 publiceerde de Joods-Poolse arts en bacterioloog/seroloog Ludwik Fleck (1896-1961) een verhandeling over de historische ontwikkeling van het syfilisbegrip en de totstandkoming van de Wassermann-reactie onder de titel *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. Fleck gaf daarin een kennissociologische analyse van het ontstaan van een wetenschappelijk feit (namelijk de relatie tussen de Wassermann-reactie en syfilis) en kan om die reden beschouwd worden als een voorloper en pionier van de constructivistische stroming in de hedendaagse wetenschapsfilosofie, -sociologie en -geschiedenis. Sleutelbegrippen in zijn benadering zijn de noties 'denkcollectief' en 'denkstijl'. Wetenschappelijke feiten komen volgens Fleck tot stand in een proces van intensieve sociale interactie in het kader van een 'denkcollectief', waarvan de leden de werkelijkheid in overeenstemming met een bepaalde 'denkstijl' leren waarnemen. Hij demonstreerde zijn filosofische en sociologische theorie over wetenschapsontwikkeling met een gedetailleerde analyse van een casus die aan zijn eigen vakgebied was ontleend.

Deze studie treedt in de voetsporen van Fleck. De door hem gekozen casus wordt aan een hernieuwde analyse onderworpen. In die zin zou je van een historisch replicatie-onderzoek kunnen spreken. Deze werkwijze is tamelijk uniek. Vrijwel alle commentatoren hebben zich gestort op de conceptuele en theoretische kwesties die door Flecks baanbrekende monografie worden opgeworpen zonder overigens in te gaan op de empirische adequaatheid van zijn case-study's. Dat laatste doe ik wel. Toch pretendeert deze studie meer te zijn dan alleen een replicatie van het werk van Fleck. Enerzijds is er een uitbreiding van het empirische domein. Naast de reeds door Fleck geanalyseerde ontwikkeling van het syfilisbegrip en de totstandkoming van de Wassermann-reactie heb ik nog twee andere belangrijke episoden uit de geschiedenis van de syfilologie als extra onderwerpen aan mijn onderzoek toegevoegd, namelijk de ontdekking van de verwekker, *Spirochaeta pallida* of *Treponema pallidum*, door Schaudinn en Hoffmann in 1905 en de ontwikkeling van het effectieve geneesmiddel *Salvarsan* door Ehrlich en zijn team in 1909-1910. Historisch gezien vormen deze ontdekkingen (resp. uitvindingen) een eenheid met de ontwikkeling van de Wassermann-reactie als serologische test voor het aantonen van syfilis (1906). Het eerste decennium van de twintigste eeuw is wel het meest vruchtbare genoemd uit de hele vijfhonderdjarige geschiedenis van de ziekte. De ontdekkingen die in dit decennium plaatsvonden zijn ook niet te beschouwen als geïsoleerde 'punt-gebeurtenissen' maar eerder als de knooppunten van een zich uitbreidend *netwerk* van medische kennis over syfilis. Anderzijds wil ik met de analyse van deze verschillende episoden uit de geschiedenis van de syfilologie niet alleen concepten en theorieën die ontleend zijn aan het werk van Fleck zelf, maar ook die welke in hedendaagse versies van het constructivisme een belangrijke rol spelen, op hun bruikbaarheid beproeven. Deze uitbreiding ligt alleen al voor de hand omdat moderne constructivisten uiteenlopende lezingen van het werk van Fleck hebben gegeven en verschillende aspecten daaruit centraal hebben gesteld. Bovendien komt het de actualiteit van deze studie uiteraard ten goede wanneer zij zich met empirische en theoretische argumenten in de vaak heftige debatten in en rond het hedendaagse constructivisme weet te positioneren.

Het hedendaagse constructivistische wetenschaps- en technologie-onderzoek biedt door zijn grote verscheidenheid aan stromingen en benaderingen een nogal verwarrende aanblik. Om het beeld wat overzichtelijker te krijgen heb ik dankbaar gebruik gemaakt van de door Rob Hagendijk ingevoerde onderscheiding in twee hoofdvormen van constructivisme, namelijk *gematigd* en *radicaal* constructivisme. Dit onderscheid wordt gemaakt alnaargelang de mate waarin de diverse benaderingen diepgewortelde opvattingen over natuur, maatschappij en wetenschappelijke kennis ter discussie stellen. Tot het gematigde constructivsme rekent Hagendijk de aanhangers van het sterke programma (Barnes, Bloor) en van het empirisch-relativistische programma (Collins, Pinch). Zij nemen een relativistisch standpunt tegenover wetenschappelijke kennis in: variaties in kennis moeten worden verklaard door deze te herleiden tot sociale structuren en processen. Het bestaan van deze laatste wordt niet geproblematiseerd. Radicale constructivisten proberen het epistemologische debat tussen realisme en anti-realisme te omzeilen. Zij achten het ook niet legitiem om uit te gaan van voorgegeven sociale structuren, aan de hand waarvan de inhoud van wetenschappelijke kennis zou kunnen worden verklaard. Zowel de natuur als de maatschappij worden gezien als een gelijktijdig produkt van de wetenschap, welke opgevat wordt als een verzameling praktijken die orde uit chaos scheppen. Elk *a priori* onderscheid, zoals tussen 'cognitief' en 'sociaal', 'subject' en 'object', of 'natuur' en 'cultuur', moet volgens de radicale constructivisten worden afgewezen. Onder het etiket radicaal constructivisme rangschikt Hagendijk het etnografische laboratoriumonderzoek van Knorr-Cetina, de actor-netwerkbenadering van Callon en Latour en het reflexieve programma van Woolgar en Ashmore. Aan dit lijstje zou ik nog de 'wetenschap-als-praktijk' benadering van Andrew Pickering en de Heideggeriaans geïnspireerde 'praktische hermeneutiek' van Joseph Rouse willen toevoegen. Het is opmerkelijk dat Fleck zelf niet eenduidig aan één van de twee kampen kan worden toegedeeld; sommige aspecten van zijn werk komen overeen met het gematigde constructivisme, terwijl andere trekken eerder op een affiniteit met het radicale constructivisme wijzen.

Na een algemeen overzicht in hoofdstuk I ga ik in hoofdstuk II uitgebreid in op de overeenkomsten en verschillen tussen de theorie van Fleck en varianten van het moderne constructivisme. Flecks pleidooi voor een 'niet-egocentrische' kennistheorie kan worden beschouwd als een anticipatie op het door David Bloor in de jaren zeventig geformuleerde symmetriebeginsel dat constitutief is voor alle moderne vormen van constructivisme. Flecks poging de Durkheimiaanse traditie in de kennissociologie voort te zetten en door te trekken naar de studie der moderne wetenschap is een andere opvallende overeenkomst, vooral met het sterke programma. De vergelijking met hedendaagse constructivistische posities geschiedt verder vooral aan de hand van twee grote, fundamentele problemen die zich permanent aan het sociologisch georiënteerde wetenschapsonderzoek lijken op te dringen, namelijk de vraag naar de aard van 'het sociale' en de relatie tussen individu en collectief enerzijds en het zogenoemde realismeprobleem anderzijds. Met betrekking tot het eerste probleem kan gesteld worden dat Fleck neigt naar een extreem collectivistisch standpunt. In overeenstemming daarmee kent hij aan de heersende 'denkstijl' een dwingende kracht toe over het denken en waarnemen van de leden van het corresponderende 'denkcollectief'. Moderne constructivisten verwerpen op grond van Mary Hesse's zogenoemde finitistische betekenistheorie principieel elke 'empowering' van normen, regels, waarden, ideeën en dus ook van zoiets als een 'denkstijl'. Met betrekking tot het realismeprobleem is Flecks standpunt vergelijkbaar met de positie van het radicale constructivisme, voorzover hij claimt dat tijdens het proces van kennisverwerving niet alleen het subject maar ook het object verandert. Met de kennis zou dus tegelijk de wereld waarop die kennis betrekking heeft veranderen. Deze radicaal-constructivistische visie botst met de verschillende realistische opvattingen over de verhouding tussen kennis en werkelijkheid, maar ook met de positie van het gematigd constructivisme (in het bijzonder het sterke programma). De analyse van het empirisch-historische materiaal in de hoofdstukken over verschillende episoden uit de geschiedenis van de syfilologie dient mede om uiteindelijk een gefundeerde en beredeneerde stellingname ten aanzien van de geschetste fundamentele kwesties mogelijk te maken. Het accent verschuift daarbij geleidelijk. Aanvankelijk staan inzichten ontleend aan het gematigd constructivisme centraal, later worden ook steeds meer radicaal-constructivistische inzichten in de case-study's verwerkt.

In hoofdstuk III bekijk ik opnieuw, in het voetspoor van Fleck en in het licht van nieuwe historische gegevens, de wording van het moderne syfilisbegrip vanaf de Renaissance. De nadruk ligt hier niet op de kritiek, maar op de consolidatie en uitbreiding van Flecks inzichten (ondanks correcties die op het vlak van de historische details nodig blijken). In overeenstemming met Flecks intenties probeer ik te laten zien dat een (gematigd) constructivistische benadering van de historische genese van ziektebegrippen rechtmatig en vruchtbaar is. Daartoe kies ik als analysekader de finitistische betekenistheorie (of netwerktheorie) van Mary Hesse, die ook als filosofische grondslag van het gematigd constructivisme (met name van het sterke programma) fungeert. Mijn 'replicatie' bevestigt het (sociaal) geconstrueerde en cultuur-geladen karakter van het moderne syfilisbegrip. Flecks meer specifieke suggestie dat de constructie van het ziektebegrip vooral sterk door morele overwegingen is gekleurd heb ik grotendeels kunnen staven door de discussies in medische kring over de indertijd aangenomen erfelijkheid van syfilis en over de relatie met verwante niet-venerische ziekten te traceren.

Hoofdstuk IV behandelt de ontdekking van de verwekker van syfilis. In de jaren 1905-1907 dongen twee microbiële kandidaten, *Spirochaeta pallida* en *Cytorrhyctes luis*, naar deze tot dan toe onvervulde aetiologische vacature. Zo'n situatie roept als het ware om toepassing van het moderne symmetriebeginsel. Het is opmerkelijk dat Fleck in zijn monografie zelf reeds een poging heeft gedaan deze episode symmetrisch te analyseren, maar helaas had hij in de uitvoering daarvan een minder gelukkige hand. In dit hoofdstuk doe ik dus een nieuwe poging. Ik heb me daarbij laten inspireren door het empirisch-relativistische programma van Harry Collins dat zich vooral op de studie van wetenschappelijke controverses heeft toegelegd. Volgens dit programma moet de analyst een strikt agnostische houding innemen ten aanzien van het al dan niet bestaan van het in het geding zijnde fenomeen en de argumenten en handelingen van de strijdende partijen op een symmetrische en onpartijdige wijze behandelen. De historische controverse over de aetiologie van syfilis is vanuit constructivistisch oogpunt zeer interessant omdat er principiële vragen werden opgeworpen over de betrouwbaarheid van (microscopische) waarneming en de mogelijke creatie van 'artefacten' door het kleuren van weefselpreparaten. Dit laatste biedt de gelegenheid om de sociale constructie van feiten en de sociale deconstructie van artefacten tegelijk binnen één enkel kader af te handelen, hetgeen van belang is met het oog op de gangbare kritiek dat constructivisten elk onderscheid tussen feiten en artefacten uitwissen. Verder laat het hoofdstuk zien dat een wetenschappelijke controverse niet enkel met een beroep op formele methodologische regels en criteria kan worden beslecht. Tenslotte ga ik in op wat volgens de constructivistische visie een 'ontdekking' eigenlijk inhoudt.

In hoofdstuk V treed ik opnieuw in de voetsporen van Fleck met een nieuwe analyse van zijn voornaamste casus, de totstandkoming van de Wassermann-reactie als een praktisch bruikbare serologische test voor het aantonen van syfilis. Het "feit dat de Wassermann-reactie verband houdt met syfilis" was volgens Fleck "één van de best gestaafde medische feiten". Hij ziet de totstandkoming van dit feit als het uiteindelijke resultaat van een gezamenlijke inspanning van het zogenoemde 'serologisch denkcollectief'. Onder leiding van August Wassermann werkte dit collectief, gestimuleerd door de sociale urgentie van het syfilisvraagstuk en door oude ideeën over syfilitisch bloed, gestaag en onophoudelijk aan het verbeteren en vervolmaken van de test totdat tenslotte een praktisch bruikbaar diagnostisch instrument werd verkregen. In dit hoofdstuk kijk ik kritisch naar de empirische en theoretische adequaatheid van Flecks analyse. Ik heb problemen met verschillende aspecten van zijn uiteenzetting, maar mijn hoofdbezwaar is wel dat hij voorbijgaat aan de 'klinische connectie' en de ontwikkeling van de Wassermann-reactie beschrijft alsof deze uitsluitend binnen de vier muren van het laboratorium heeft plaatsgevonden, waarbij de serologen niets anders deden dan "aan hun apparaten draaien". In mijn alternatieve weergave van de hele episode leg ik veel meer nadruk op de interactie tussen serologen en klinici. Die uiteenzetting is geïnspireerd door Bruno Latour's ideeën over recrutering (enrollment) en het vertalen van belangen. Mulkay, Pinch en Ashmore hebben hieraan met hun notie van 'toepassingsdilemma's' (dilemmas of application) een meer toegespitste formulering gegeven. Bij hun poging klinici te overtuigen van de waarde en betrouwbaarheid van de Wassermann-reactie, zaten serologen aanvankelijk gevangen in een 'toepassingsdilemma' in de zin van Mulkay c.s.: indien de uitkomst van de test overeenkwam met het eigen oordeel van de klinici, zou deze hen niets nieuws vertellen; maar als hij daarmee strijdig was, zou zijn validiteit en betrouwbaarheid zeker in twijfel worden getrokken. Door klinici actief erbij te betrekken ('enrollment') kon het dilemma worden overwonnen, enerzijds door de technische uitvoeringswijze en klinische betekenis van de Wassermann-reactie te veranderen en anderzijds door een herdefiniëring te geven van de diagnostische en therapeutische belangen van de klinici welke de test zou dienen. Uiteindelijk werd dankzij een gezamenlijke inspanning van serologen en klinici een praktisch bruikbare serologische test voor het aantonen van syfilis verkregen. Deze analyse beoogt niet enkel empirische en theoretische tekortkomingen in Flecks uiteenzetting recht te trekken, zij wil zich hier en daar ook voorzichtig wagen aan radicaal-constructivistische exercities. Behalve de reeds genoemde Latouriaanse noties van 'enrollment' en vertalen van belangen, heb ik ook inzichten van Pickering en Rouse met betrekking tot het praktische karakter van wetenschappelijke activiteit verwerkt, in het bijzonder wat de realisatie van experimentele systemen en de omgang met grondstoffen, proefdieren en ook 'patiëntenmateriaal' aangaat. Anders dan deze radicaal-constructivistsiche auteurs zie ik echter geen aanleiding de door gematigde constructivisten gebruikte notie van 'belangen' principieel van de hand te wijzen. Naar mijn oordeel komt aan de professionele belangen van serologen en klinici juist een bescheiden rol toe bij het verklaren van de ontwikkeling van de Wassermann-reactie. Verklaringen in termen van belangen zijn mijns inziens zeer wel verenigbaar met het door Latour beschreven verschijnsel van het vertalen van belangen.

Hoofdstuk VI is gewijd aan een analyse van de strijd over het intellectuele eigendom van de Wassermann-reactie, die in het kielzog van de ontwikkeling van deze serologische test losbrandde - als een soort bittere epiloog. In 1921 raakte August Wassermann verwikkeld in een polemiek met onder andere zijn voormalige medewerker Carl Bruck en zijn voormalige criticus Eduard Weil over wie zich met recht de geestelijke vader van de Wassermann-reactie mocht noemen. Ik wijd een afzonderlijk hoofdstuk aan deze polemiek, die destijds als een 'onverkwikkelijke' affaire werd beschouwd, omdat zij ons de unieke gelegenheid biedt om de dikwijls gehekelde 'collectivistische' of 'anti-individualistische' stellingname van Flecks benadering kritisch te waarderen aan de hand van historisch materiaal. De conclusie van mijn analyse is dat de 'collectivistische' karaktertrek van Flecks kennissociologie hem inderdaad slecht heeft toegerust om de strijd over het geestelijk eigendom van de Wassermann-reactie adequaat te kunnen behandelen. Hij accepteert kritiekloos de beweringen die door de betrokkenen in de loop van deze strijd zijn gedaan alsof zij zonder meer hun visie op de ontwikkeling van de Wassermann-reactie zouden weerspiegelen, zonder dat hij rekening houdt met de strategische context waarin deze uitspraken werden gedaan om eigen claims op intellectueel eigendom te ondersteunen of die van anderen te ondermijnen. Mijn eigen weergave van deze 'onverkwikkelijke' episode is geïnspireerd door Robert Mertons wetenschapssociologie, die uitgaat van een evenwichtiger visie op de relatie tussen individu en collectief. De strijd over geestelijk eigendom tussen de leden van een zelfde team is voor de Mertoniaanse wetenschapssociologie een nog ongeëxploreerd thema (de meeste aandacht is uitgegaan naar prioriteitsstrijd tussen onderling onafhankelijke wetenschappers). Ook hef ik Mertons uitsluiting van de inhoud van wetenschappelijke kennis uit het domein van wetenschapssociologische analyse op: de vraag *wie* een creatief aandeel heeft gehad in een ontdekking is onlosmakelijk verbonden met de vraag *wat* er precies is ontdekt. In de strijd om de Wassermann-reactie konden betrokkenen hun zaak alleen bepleiten door op beide punten positie te kiezen. Op deze wijze probeer ik Mertoniaanse inzichten in een constructivistische benadering te integreren. Gezien het feit dat Mertons sociologie uit constructivistische hoek indertijd een spervuur van kritiek heeft ontvangen, kan mijn poging als een pleidooi voor rehabilitatie worden beschouwd.

Hoofdstuk VII behandelt de ontwikkeling van een effectief chemotherapeutisch geneesmiddel tegen syfilis door Paul Ehrlich en zijn medewerkers. Flecks monografie bevat hierover slechts enkele incidentele opmerkingen. De reden om een hoofdstuk over dit onderwerp op te nemen, is, afgezien van het feit dat het een belangrijk knooppunt in het zich uitbreidende conceptuele netwerk van de syfilologie vertegenwoordigt, vooral hierin gelegen dat Ehrlichs werk zich uitstekend leent voor een type analyse dat in recente vormen van (radicaal) constructivisme steeds prominenter is geworden, namelijk één met het accent op 'wetenschap-als-praktijk' in plaats van 'wetenschap-als-kennis'. Hoewel Andrew Pickering van deze tendens de meest uitgesproken exponent is, kan zij ook worden aangetroffen in het werk van de filosoof Joseph Rouse en in het al wat oudere werk van Karin Knorr-Cetina. Ook bepaalde aspecten van het werk van Latour kunnen hieronder worden gerangschikt. Met name de door hem zo nadrukkelijk opgeworpen vraag hoe laboratoriumresultaten toepasbaar kunnen zijn resp. toepasbaar kunnen worden gemaakt in de wereld buiten het laboratorium (m.a.w. hoe de 'wetenschap' uit het laboratorium komt) kan in het kader van een analyse van 'wetenschap-als-praktijk' een plaats worden gegeven. Een dergelijke analyse is al gedeeltelijk in hoofdstuk V beproefd, maar wordt in dit hoofdstuk op grotere schaal uitgevoerd. Centraal in dit hoofdstuk staat Paul Ehrlichs praktijk van 'experimentele therapie' (of 'chemotherapie') welke door moderne farmacologen vaak beschouwd wordt als het eerste begin van het rationeel ontwerpen van geneesmiddelen. Hiertoe moest Ehrlich een omvangrijke 'constructiemachinerie' (Knorr-Cetina) opbouwen door fondsen te verwerven en materiële en menselijke hulpbronnen bij elkaar te brengen in nauwe samenwerking met de Duitse synthetische kleurstofindustrie. Om deze hulpbronnen vervolgens productief aan het werk te zetten, ontleende hij aan diezelfde industrie een model van research-management en wetenschappelijke arbeidsdeling ('chemische Massenarbeit') dat hij verder toesneed op zijn eigen behoeften door het chemische werk te combineren met het op grote schaal uittesten van chemische preparaten op proefdieren. Het geheim van Ehrlichs succes was in feite de combinatie van 'chemische massa-arbeid' met 'biologische massa-arbeid' en de creatie van handige 'experimentele systemen' (Rouse) door de keuze van geschikte proefdieren. Uiteraard moesten Ehrlich en zijn medewerkers tal van problemen met grondstoffen en proefdieren overwinnen. Ook was de hele onderneming niet van meet af aan op het vinden van een middel tegen syfilis gericht; de wending naar deze ziekte vond pas tijdens de rit op grond van 'contextueel opportunisme' (Pickering) plaats. Aanvankelijk had Ehrlich gepocht dat door zijn gebruik van dierproeven op grote schaal de meest 'optimale' geneesmiddelen konden worden ontwikkeld en geselecteerd, zodat de uiteindelijke test op de mens niet meer dan de spreekwoordelijke proef op de som zou zijn. Dat zou echter heel anders uitpakken. Nadat in het laboratorium een effectief middel tegen syfilitisch besmette konijnen was gevonden, moest de stap naar de wereld buiten het lab worden gezet en die stap had heel wat voeten in de aarde. Een constructivistische analyse volgens de 'wetenschap-als-praktijk' benadering laat zien welk soort problemen moeten worden overwonnen opdat laboratoriumproducten daadwerkelijk hun weg vinden naar kliniek en maatschappij. In de analyse van de casus blijkt ook de ontoereikendheid van Latours antwoord op de door hem zelf opgeworpen vraag: de klinische introductie van Salvarsan omvatte veel meer dan het overplanten van laboratoriumcondities naar de buitenwereld; het omvatte ook de 'normalisering van het object' (Knorr-Cetina), juridische, sociale en politieke interventie, en voortgaand experimenteren met het middel na de commerciële introductie buiten het lab (de these van de 'maatschappij als laboratorium').

Hoofdstuk VIII behandelt niet een bepaalde episode uit de geschiedenis van de syfilologie, maar presenteert een reconstructie van de zogenoemde 'serologische denkstijl' die volgens Fleck het denken en handelen van de leden van het serologencollectief onder leiding van Wassermann zou hebben bepaald. Anders dan Fleck wijzen hedendaagse constructivisten, onder andere op grond van finitistische argumenten, een dergelijk verklarend gebruik van het begrip 'denkstijl' af. Dat laat nog steeds de mogelijkheid open dat deze term verwijst naar een als explanandum te onderzoeken fenomeen. Ook Flecks descriptieve karakterisering van de serologische denkstijl roept echter vragen op. In navolging van de wetenschapshistoricus Jonathan Harwood betoog ik dat het stijlbegrip alleen zinvol kan worden gebruikt in een comparatieve opzet. Het heeft geen zin te spreken van de denkstijl van het serologencollectief, als diezelfde stijl niet ook kan worden herkend in andere sectoren dan de serologie en als zij niet met tenminste één alternatieve stijl kan worden gecontrasteerd. Bij het uitvoeren van dit comparatieve onderzoek heb ik kunnen teruggrijpen op de in voorgaande hoofdstukken beschreven episoden uit de geschiedenis van de syfilologie die tezamen verschillende gebieden van de medische wetenschap dekken (nosologie, aetiologie, serologie, therapie). De eenheid van Flecks serologische denkstijl wordt gevonden in het basisidee van specificiteit. Zo gezien representeert zij de door Pauline Mazumdar geanalyseerde 'pluralistische' stijl, welke tegenover de 'unitaristische' stijl kan worden gezet. Aldus kan aan Harwoods eisen voor het gebruik van het stijlbegrip worden tegemoetgekomen. Tenslotte laat ik zien dat de 'macht' van de pluralistische stijl (dus van het idee van specificiteit) gedeeltelijk kan worden herleid tot de ongeëvenaarde machtsstructuur die de Koch-Ehrlich groep in de Duitse geneeskunde rond 1900 had weten op te bouwen.

In hoofdstuk IV heb ik tenslotte, voortbouwend op de resultaten uit de voorgaande hoofdstukken, geprobeerd een genuanceerde (althans doordachte) positie te ontwikkelen ten aanzien van de twee grote, fundamentele problemen waarmee het constructivistisch wetenschaps- en technologie-onderzoek permanent wordt geconfronteerd: het zogenoemde realismeprobleem en de vraag naar de aard van 'het sociale' en de relatie tussen individu en collectief. Het tweede probleem omvat ook de vraag naar de meest adequate conceptualisering van de notie van 'sociale praktijken' in het algemeen en van 'wetenschappelijke praktijken' in het bijzonder. Het gematigde en het radicale constructivisme nemen met het oog op beide fundamentele problemen zeer uiteenlopende posities in. Radicale constructivisten trekken de constructiemetafoor zover door dat in hun visie niet alleen plastics of genetisch gemodificeerde organismen, maar ook microben, electronen en quarks als door de wetenschap geconstrueerde objecten worden opgevat. Dikwijls speelt hierbij het 'splitsing-en-omkering' model van Latour en Woolgar over het ontstaan van feiten op de achtergrond mee. Deze mijns inziens onhoudbare visie op de constructie van objecten (en feiten) brengt het radicale constructivisme in direct conflict met gangbare realistische opvattingen, ofschoon de radicale constructivisten zelf van mening zijn dat ze het hele debat tussen realisme en anti-realisme hebben overwonnen. Bij gematigde constructivisten heeft de term 'constructie' alleen betrekking op vorming van kennis van de natuurlijke werkelijkheid, niet op die werkelijkheid zelf of de objecten die haar bevolken. Aanhangers van het sterke programma stellen zich zelfs op als common-sense realisten tegenover die werkelijkheid. Ik acht een dergelijke positie zeer goed verdedigbaar. Zij verdient de voorkeur boven het zogenoemde 'wetenschappelijke realisme' dat te sterke ontologische commitments heeft ten aanzien van de op dit moment door de natuurwetenschap gepostuleerde theoretische entiteiten. Bovendien redeneert deze stroming op nogal problematische wijze van het praktisch succes van toepassingen naar de waarheid van de toegepaste theorieën en miskent ze de flexibiliteit van wetenschappelijke begrippen zoals deze door het finitisme wordt benadrukt.

Wat het tweede fundamentele probleem betreft, deel ik niet de hyperkritische scepsis die de radicale constructivisten ten aanzien van 'het sociale' tentoonspreiden. Ik heb geprobeerd de verschillende bezwaren die zij tegen 'sociale' verklaringen van de inhoud van wetenschappelijke kennis naar voren brengen stuk voor stuk te ontzenuwen. Zo is Latours argument dat de maatschappij geen basis levert voor dergelijke verklaringen omdat techno-wetenschappers zelf als maatschappijveranderaars optreden niet meer dan een halve waarheid: ook in die hoedanigheid vindt hun handelen immers plaats onder bepaalde, niet door henzelf gekozen of beheerste maatschappelijke verhoudingen. Verder zet ik mij kritisch uiteen met de door Rouse geuite kritiek dat de gematigde constructivisten de validering van kennisclaims te veel laten plaatsvinden binnen relatief afgeschermde wetenschappelijke gemeenschappen en de door Knorr-Cetina betrokken stelling dat de validering van zulke claims geen sociale locus van consensusvorming boven en buiten het laboratorium behoeft. Ook het door Woolgar geformuleerde verwijt dat elke poging om de sociale bepaaldheid van wetenschappelijke kennis aan te tonen neerkomt op het afschilderen van competente en handelingsbekwame individuen als passieve marionetten of 'cultural dopes' wijs ik van de hand. Wel geef ik toe dat het moeilijk is de juiste balans te vinden tussen de spontaneïteit en handelingsbekwaamheid ('agency') van individuen enerzijds en de werking van maatschappelijke structuren ('constraints') anderzijds. Een theoretische benadering die aan beide aspecten recht doet, zou zeer welkom zijn. Daarom ga ik tenslotte na of de bekende 'structuratietheorie' van Anthony Giddens wellicht aan deze desiderata voldoet. De door William Sewell voorgestelde modificatie van deze theorie lijkt uiteindelijk redelijk te voldoen. Wordt bij Giddens sociale structuur als samenstel van regels en hulpbronnen ('rules and resources') gezien als een virtuele orde die in concrete praktijken wordt gereproduceerd, bij Sewell wordt de dualiteit van structuur geherformuleerd als een dualiteit van virtuele elementen, namelijk regels oftewel culturele schema's, en actuele elementen, namelijk hulpbronnen. Het inzetten van (materiële en menselijke) hulpbronnen wordt geïnformeerd door culturele schema's; omgekeerd zijn deze laatste voor hun reproductie aangewezen op actueel gebruik bij de accumulatie van hulpbronnen. De handelingsbekwaamheid van individuen ('agency') wordt in dit kader gedefinieerd als toegang tot hulpbronnen en de competentie om bestaande culturele schema's in nieuwe contexten toe te passen. Sewells nadruk op de transponeerbaarheid van culturele schema's naar nieuwe situaties vertoont grote gelijkenis met het finitisme. Ook vat hij 'agency' als door-en-door sociaal op. Zijn conceptualisering van de notie van sociale praktijken, zo betoog ik tenslotte, is in staat om de waardevolle elementen uit de 'wetenschap-als-praktijk' benadering van Pickering en de 'praktische hermeneutiek' van Rouse in zich te integreren zonder de dubieuze posthumanistische en 'anti-sociale' lading van deze beide laatste mee aan boord te nemen.

Curriculum vitae

Henk van den Belt was born 7 January 1953 in Zwolle, The Netherlands. In 1970 he finished his secondary education at the Rijks Hogere Burger School (B-orientation) in Zwolle. He studied sociology at the University of Groningen from 1970 to 1978, specializing in the sociology of work and organization, theoretical sociology, and historical sociology and social philosophy. After graduating cum laude, he fulfilled his military obligations as reserve officer candidate and lieutenant in the medical staff of the army. From 1979 to 1983 he participated as a research assistant in an interdisciplinary project on the history of the synthetic dye industry at the Catholic University of Nijmegen. Since then he developed a special interest in the philosophy, history and sociology of science and technology, of which several publications in international books and journals bear witness. In 1989 he became a university teacher affiliated with the Department of Applied Philosophy at Wageningen Agricultural University.