Towards a soil fertility in dimensions[1]

TH. J. FERRARI
Institute for Soil Fertility, Groningen, The Netherlands

Summary

This paper deals with the historical development of the methodology on which the research of the Institute for Soil Fertility is based. By means of examples of the study of relationships between fertilization or soil management effects and soil factors the "why" and "how" of this Groningen school are given.

"We have had to familiarise ourselves with the idea that for an understanding of nature we may have to work with non-euclidean geometry and more than three dimensions."

J. Huizinga in "In the Shadow of To-morrow"

1. Introduction

In order to prevent misunderstandings the definition of what henceforth is meant by research into soil fertility is started with: all intellectual and other activities which have as their purpose a better understanding of the interdependence between plant growth under farm conditions and soil factors and also of control of growth by means of influencing those factors. The cardinal point is the influence of the soil on the crop. The biological, chemical and physical research which is necessary in order to study the ecological factors themselves will not be considered. Our kind of research is more interested in relationships than in hidden mechanisms.
The practitioners of agricultural research of the early period were mostly people with a biological, chemical or physical background. Their specialized knowledge was accompanied by the methods which were accepted as the correct ones in their sciences. An agricultural science aware of its own problems did not yet exist. In the natural

[1] Paper on the occasion of the 75th anniversary of the Institute for Soil Fertility, Groningen, The Netherlands.

Received for publication: 26th October, 1965.

sciences it has been found particularly profitable to test the hypothesis against data which have been obtained artificially by changing one factor, keeping the others constant (*manipulative control* and the *principle of ceteris paribus*). At the beginning there was little experience, but a firm conviction: that increase of knowledge, including of soil fertility, could only be obtained by interference, for instance by changing one factor and keeping the others constant. This principle has been the background of using experimental fields with interference.
However, the experience has been that the results of investigations of a soil fertility problem may diverge widely, that different researchers reach different conclusions about the same phenomenon, that the generalization of the results of research is a matter of difficulty, etc. The experience of this early period of research into soil fertility confronted the researchers with the problem of the methodology of agricultural research in general and of research into fertility in particular. Two schools of thought began to take shape. The first school blamed the difficulties on faulty trial techniques and saw the solution principally in obtaining more accurate data by better designed experimental fields and later achieved a considerable following. The researchers on the subject of soil fertility in Groningen rather tried to find the solution in explaining the variations between the experimental fields and have developed a method for that purpose. In this respect they diverge markedly from the opinions of the other centers.
In the following the actual development of this Groningen school will be used as a frame, in which the present methodological opinions will be discussed and on the basis of which the development itself can be judged once more.

2. The start of the dimensional idea

The attack on specific soil diseases, that is to say the peat-colonies oat sickness and the Hooghalen sickness has, in retrospect, been the beginning of a Groningen school. At the beginning of the twentieth century symptoms of disease occurred increasingly in oats on the soils in the peat colonies. Sjollema and Hudig gained practical experience, showing that the so-called alkaline manuring might be a cause. This led to an investigation into the behaviour of acid and alkaline fertilizers in field experiments. Soon it was clear that the sickness was indeed related to the alkaline reaction of the soil. Shortly afterwards, about 1913, Hudig was confronted with the occurrence of the Hooghalen sickness. He found that the use of alkaline fertilizers constituted the remedy. Undoubtedly it was a major achievement of Hudig that already at that time he established the relation between the two diseases. In their study of the results of an old experimental field Hudig and Meijer demonstrated this connection and the importance of the lime status by arranging the yields expressed in percentages of the highest yield by an increasing lime status of the soil. In this way the table shows the optimum curve which has become so well-known. This report has historical importance because for the first time a quantitative two-dimensional relationship has been given between a soil factor and the yield.
De Vries made the next step by propounding clearly that yield is a function of many factors and that the fertility of the soil must be regarded as a multidimensional complex. Researchers must start from this point of view. He developed a working programme in a paper read in 1934 *Questions of manuring in their mutual relationships seen as a multidimensional problem.* He submitted not only that the yield depends

on a large number of factors but also that the action of one factor on the yield may be influenced by another factor. In consequence the experimental fields must be laid out in such a way that two or more factors can be investigated simultaneously.
In this connection the example elaborated by Visser on the factors which determine the influence of nitrogen dressing in the soil as it increases or decreases the pH has been of fundamental importance. In this example an interaction was investigated but one factor in the investigation was not artificially changed by the researcher. On account of the great importance which this approach has had for later developments the evaluation will be briefly reproduced.
This was the problem. From a number of perennial experimental fields the ultimate increase or decrease of pH, brought about in the soil by continued manuring of sulphate of ammonia or sodium nitrate, was known. The analysis showed that these increases and decreases were not equally large on the different fields. On some experimental fields the pH sharply increased by sulphate of ammonia, but on others it only showed a small increase. The old problem of the variable results came to the fore again, which always had confronted the researchers. It has been an ingenious thought to accept one particular characteristic of the soil as a cause of these differences between experimental fields and to correlate the results of the experiments in a certain manner to this point of view. In this case Q_n was taken as the explanatory factor, that is to say the relation between the number of kilograms of nitrogen per hectare and the number of tons of humus per hectare op top layer. On account of some special considerations on the pH finally reached, an influence of that kind could have been expected. In fact a clear relationship between Q_n on the one side and the increases and decreases of pH on the other side was found; this was demonstrated by means of a three-dimensional graph. In this way the hypothesis was confirmed and an estimate of the size of the influence was obtained. It was now possible to predict the changes in pH, influenced by nitrogen dressing.
In four articles (1936–1939) *The serial principle in field experiments* de Vries reviewed the ideas which had gradually developed within the State Agricultural Research Station, Groningen.
In the first place he drew attention to the importance of the so-called serial principle in field experiments. This he meant to be the method long since known in the physiology of studying the influence of one factor with increasing or decreasing levels (e.g. increasing quantities of a manure). The relationship between this growth factor and the change thus caused may be represented by means of a curve. The maximum, which is of outstanding importance, can be found in this way. He emphasized that the extension of the number of treatments necessary for this purpose may be made at the expense of the number of replicates; the number of plots required need not be increased to achieve the same accuracy in the conclusions. Here de Vries had chosen a direction which deviates markedly from the then prevailing notion: "repetitions above all in order to achieve great accuracy". How important this choice has been for agriculture in The Netherlands may be demonstrated by the following citations. Peterson submits in his article *Examples of the influence of soil science on agronomic practice* (1957) that agricultural research in the United States had failed because no attention was paid to the ideas of Gile which were comparable to those of de Vries. He writes: "Had these admonitions been followed, I believe we would have been using higher rates of fertilizer much earlier and we would not have been penalizing row crops so heavily for so long in land-use recommandations." Boyd (Rothamsted) expressed a complaint of the same type in a

paper read before The Fertiliser Society entitled *Current fertiliser practice in relation to manurial requirements:* "An unfortunate feature of a large part of the experimental work on fertilisers done since the war has been the restricted range of rates tested. Timidity tends to be the besetting sin of the experimenter, and it is hardly surprising that, in a period of such rapid increases in fertiliser use, the rates of fertiliser used in the experiments should have lagged behind those being applied in practice." He concludes: "..... and it was not until the new series of sugar experiments began in 1957 that anything like the full range of fertiliser rates being applied on British farms was tested in practice." It should be noted here that the absence of sufficient levels is a consequence of the English system of field experiments.

De Vries has pointed out that the serial principle is important in order to compare the different forms of a nutrient. He wrote that the current method of comparing dressings in one definite quantity is erroneous: "A comparison of different manures, each of them in one object must in those cases be condemned and rejected equally decisively and consciouslly as until now a field experiment without replications has correctly been condemned and rejected." Recently, the method has also been used in comparing the manner of manuring (broadcast and in rows) and the effect of organic manures.

The system of points in series (levels) may also be extended in other directions. With the aid of an example de Vries demonstrated that more information from a field experiment is obtained by omitting to combine the parallels, using these as separate objects. This was demonstrated by determining in a phosphate field experiment the phosphate content in the soil of each parallel plot and by relating these contents to the yields obtained. The increase in knowledge compared with the old method is indeed striking. Later on it was found that this mode of thought was particularly fruitful.

Within the scope of the serial principle Visser made another step forward by disregarding systematically chosen objects. For this purpose he took the data of four perennial experimental fields on which totally different problems had been investigated (comparison of types of nitrogen manures and stable manure under different conditions, lime status levels and comparisons of phosphate and potash manures). He used all 130 plots to check the relationship between yield and pH under different farm conditions.

Of great importance were van der Paauw's investigations into the potash problem on sandy and residual peat soils, of which the results were published in 1936. By applying the serial principle and by using the experience and ideas of the State Agricultural Research Station he could relate the potash status of different trial fields and the yield responses to an application of potash fertilizer. He simplified the problem into an interdependence of relatively few dimensions by expressing the yields of the treatments in percentages of the highest yield of every experiment. These relative yields van der Paauw related to the corresponding potash number (being the standard for the potash status of the soil); he plotted these data against each other in a graph and drew a mean line. In this way he obtained the well-known curve which showed up in numerous more recent investigations. In the investigation of de Vries already discussed above there were only differences of the phosphate conditions within one field, but in this case differences in potash status between the fields were incorporated in the evaluation which had not been created by the researcher. This investigation is also a demonstration of the development of the notion of "series" in a relatively short time.

Nowadays we know this word rather more in the meaning of series of experimental fields. The step towards this system was obvious. In this case a series of relatively simple manure experimental fields (annual or perennial) is set out. The number of replications is few, the treatments are chosen in such a way that the whole trajectory of the yield curve is obtained, and all the time attempts are made to achieve at least the maximum yield. This applies both to comparisons within one experiment and those between experiments. The latter means that the experiments are set out on fields poorly, moderately or well provided with the elements to be investigated. This also applies to factors which may influence the response. In this way it is possible to investigate the yield response under all conditions. The first investigation which was performed in this way and which was the example for later investigations, is Visser's into the potash and phosphate regime of the Groningen clay and sandy clay soils in 1938 and 1939.

3. Graphic analysis techniques

The usefulness of these methodological ideas has been much promoted by using the graphic analysis technique. In the early thirties there was, in economics, already a considerable fund of experience of this graphic method. Visser saw the possibilities of this method in the investigation of soil fertility and has contributed largely towards the improvement of the foundations. It must be considered an outstanding merit that he applied to this graphic method the principles of fitting lines to which van Uven had already added new and important elements.

The opinions within the State Agricultural Research Station and a large number of available data in the early thirties pushed the researcher in the direction of the graphic analysis. In two articles of 1942 and 1943 Visser gave a motivation which continues to be valid. In the first publication he wrote: "It is not known how the response formula reads. A fundamentally correct analysis is therefore impossible. Even the most time-consuming calculation can therefore never be more than an exact treatment on shaky foundations, a circumstance which is often met with in applied scientific work. There is no reason for preferring on these grounds the mathematical treatment of this kind of problems rather than a graphic analysis." From these words it is clear that with him too the choice of the graphic method was not based on foundations of principle; there was nothing better to be had. Only recently the first attempts have been made to obtain a mathematical formulation of yield curves based on the principles of biology.

This was an important choice. Formerly as well as nowadays mathematical descriptions of most phenomena on the basis of physiological knowledge were lacking. Given this fact the investigator may choose between waiting for that knowledge and foregoing an evaluation of these relationships or he may take another direction. The first choice is a rather unprofitable one from an agronomic point of view. Outside The Netherlands many of the investigators in soil fertility research have turned this direction and faltered in calculating the averages, which also happened during the early period in The Netherlands. In the second choice some definite relationships are assumed and it is investigated heuristically which types of curves and comparisons best describe the relationships in question. There is no great difference between the mathematical and graphic methods in these matters. The logical restrictions of the graphic analysis are comparable to those which apply to the choice of the mathematical

one. The investigators of Groningen stood aside in the conflict of the correct equation: they have taken their own course. The dangers of describing the response to manures by any given equation was clear when Crowther and Yates used the exponential function in England in 1940. Boyd writes about this subject in *Fertiliser responses of manuring potatoes: a re-examination of the experimental evidence* (1961): "These experiments have been valuable in showing that the exponential type of response curve, in which responses to increasing dressings rise asymptotically to a maximum, can give an erroneous impression of the returns from high levels of manuring."

The application of the graphic method does not mean, however, that this method does not contain any dangers. The often heard criticism about the arbitrariness of the graphic method is not important. It rests principally on the fact that a judgment is made of a method without experience of one's own. The multidimensional evaluation leaves little room for arbitrariness. The criticism that an objective judgment of the reliability is lacking is more important from the methodological point of view as well as from the mathematical-statistical point of view.

It has already been shown that the formation of hypotheses is of considerable importance for the increase of our knowledge. It is clear that an investigator, applying the graphic method already has a hypothesis in some form. He assumes a relationship between variables, with vague notions on the ascending or descending branches, the shape and position of the maximum, etc. It is, however, difficult to define this hypothesis further. As a result the investigator is more inclined to await the suggestions offered by the data than to evolve and test a further elaborated hypothesis. The best solution would be to test such suggestions, obtained in the shape of a curve, once more as a new hypothesis with new data. Experience shows, however, that this is not often done, not least on account of the extent and costs of the investigation.

The absence of the possibility to test the relations statistically has met with severe criticism for a long time. This has been stimulated, because the scatter of the points around the line may easily lead to an underestimation of the reliability of a line. It is not realized that the points may be more scattered around a statistically reliable average of which only the standard deviation is given. After Ferrari introduced the reliability calculation in 1952, a calculation of that kind is always applied. The criticisms have disappeared and the graphic method is universally applied as well as in other sciences. Since the electronic calculating machines were developed, there is a tendency, however, to process the evaluation as much as possible on mathematical lines.

4. The other direction

The importance of the Groningen development may best be judged by comparing with the ideas outside The Netherlands. The development in other centres of soil fertility research – of which the experimental station of "Rothamsted" was the most influential – was totally different. A short summary of these ideas is given here for the purpose of comparison. First I must revert for a moment to the opinions on the value of experimental fields in soil fertility research during the period 1925–1930.

It has been shown that experience with field experiments was rather disappointing. It was found to be difficult to draw universally valid conclusions. It was the current opinion that this was related to the unreliability of the results. Now this unreliability

has two sides, and it is well to distinguish between them. In the first place there is an unreliability related to the wide variation in results between experiments. The average result is mostly of little importance for individual cases. It has been shown that the Groningen school considers these sizeable differences to be true; and an attempt must therefore be made to trace the causes of these differences. Others have tried to find the solution by means of obtaining a small variation in the results by a homogeneous random sample of experimental fields. By doing this the deviation from normal conditions is so considerable that it is questionable if this method can lead to good results. And so Sanderson came to write in 1959: "A lower between-trial error, leading to significance from fewer trials, is not necessarily a matter of a congratulation, but suggests that the trial centra or seasons may not have been sufficiently representative."

The second aspect of the reliability is related to the accuracy within only one experimental field. Experience has shown that the heterogeneity of the soil on one experimental field is, generally speaking, considerable; hence it is often difficult to establish whether a difference in yield between the treatments has been caused by the manipulation, or by the difference in soil conditions or by both. Methods have been developed in order to eliminate these disturbing influences as much as possible. Amongst others the methods of Holtmark and Larsen, Mitscherlich and Knut Vik are well-known. Best known is undoubtedly the method based on the analysis of variance with which the name of R. A. Fischer is connected. Its leading principle is that any kind of attempt is made to achieve a sufficient separation between the several causes of variance and in doing so the most reliable conclusion possible is drawn. By a correct combination of replications and experimental design it is attempted to eliminate the influence of the heterogeneity of the soil by a calculation. Conditions are made as favourable as possible to determine an influence reliably. The number of levels of the factors to be varied is sacrificed to the number of replications. This means that there are too few levels to define a proper relationship between factor and yield represented in a curve. In most cases it is impossible to determine whether the maximum has been achieved or not. This impossibility is all the more important by reason of the necessity to investigate interactions because on that account the number of combinations of treatments must be considerably increased.

The insistence to acquire reliable conclusions and the successes achieved in this field, recently and formerly, undoubtedly are the background of using this method also in summarizing data from different experimental fields. More attention was paid to the significance of the conclusions obtained by summarizing evaluation than to a possible explanation of the differences themselves. Here lies a considerable difference with the opinions held in Groningen. The evaluation of series of field experiments is also based on the principle of the analysis of variance with the result that conclusions were obtained such as "the difference in response to an application of potassium found between several experiments is significant", which in agricultural respect is pretty well worthless. Only much later people have started to indicate the variance influences more precisely by means of a separate evaluation of groups, for instance, classified according to pedological units. In this case no attempt is made to arrive at an explanation. In this respect there is not much difference with the evaluation in the beginning of the present century. A publication by Reith and Inkson *Effects of fertilisers and farmyard manure on swedes and turnips* (1963) is illuminating. In the summary they write: "Phosphorus increases the yield of roots in all

but four of the experiments and this nutrient generally had a greater effect than either nitrogen or potassium. The response to potassium was significant in only half of the experiments and its effect on yield was normally less than that of nitrogen." Compared with the findings of 60 years ago in The Netherlands progress is not great; very little is noticed of a method of relating the differences between experimental fields systematically with particular causal factors. Only in recent years a direction is coming to the fore, especially in the U.S., which can be compared with the work of the Groningen school.

The emphasized importance of acquiring reliable conclusions which resulted of course in many replicates and few levels prompted the investigators to draw qualitative conclusions rather than quantitative ones. In fact the opinions did not constitute a sound basis to start thinking of a summarizing treatment of experimental data into the direction of functional relations. The one is interrelated to the other. In addition conditions were favourable for the ideas of Fischer and his co-workers to be well-received, that is to say the undoubtedly excellent mathematical qualities of the analysis of variance, the possibilities of eliminating the influences of disturbing factors and of expressing the reliability of conclusions into a number, the many successes of the method which were obtained also in other sciences, the possibility of applying the method relatively schematically, and finally the central position which Rothamsted occupied in the British Empire.

The result is to be seen in the present statistical textbooks: the extensive discussion of the analysis of variance and the experimental design based on it and the relatively minor attention given to regression analysis. In his article *Causal inference from observational data* (1955) Wold discusses the modern methods for the description and explanation of phenomena. He writes: "A fourth broad area remains: explanation on the basis of observational data. In this field, which embraces amongst other things a large proportion of social research, progress has been less systematic and spectacular. Current textbooks reveal the stepchild treatment which this sort of problem has received from professional statisticians." The extensive application of the analysis of variance together with the manipulative experiment has notably delayed the practical importance of soil fertility investigations outside The Netherlands. This is related to the fact that determining the reliability and increasing that reliability was more regarded as an aim than as a means. On this point Yates' words in his article *Fisher and the design of experiments* (1964) written on the occasion of Fisher's death are significant. He ends as follows: "The most commonly occurring weakness in the application of Fisherian methods is, I think, undue emphasis on tests of significance, and failure to recognize that in many types of experimental work estimates of the treatment effects together with estimates of the errors to which they are subject, are the quantities of primary interest." And finally: "And some of them, indeed, came to regard the achievement of a significant result as an end in itself." It seemt to be highly desirable that all researchers are made aware of these words of Yates.

5. Increase of multidimensional knowledge

In itself it is rather remarkable that the Groningen school, with completely contrary opinions on some points has developed next to the school of Rothamsted and has been able to maintain itself against the popularity of that other. For this growth

obviously a definite constellation of factors has been necessary. This development was only possible by a number of investigators with the vision, expertise and courage to pose a problem and to find a solution in their own way. The subsequent development has also made it clear that personal factors are often the most important ones for growing in a certain direction.

The thesis of regarding the soil fertility as a multidimensional problem in which yield is the function of a number of factors, the experience that differences in response between experimental fields were of such scope that they had to be explained by systematic differences of soil factors and not by inaccuracy and finally the necessity of explaining these differences causally in order to be enabled to provide useful advice to the farmers, all these aspects together induced the researchers to take account of their possible influence already in the choice of experimental fields. Having the future analysis in mind the attempt is made to obtain the largest possible differentiation between the experimental fields. It was not necessary to await the results of the work of specialists in relation to the mathematical formulation of this relationship; the graphic method offered possibilities in plenty to describe these multidimensional relations. The first investigation which was planned in this way is the one already mentioned before into the phosphate and potash regime of the Groningen clay and sandy clay soils.

In a paper of 1942 van der Paauw described "The solution of agricultural problems by means of some large and series of smaller experimental fields." In a concrete example he starts from the hypothesis that there is some kind of relation between the size of the response to manuring and the potash status of the soil: the poorer the soil, the greater the response. When he plots the K-content as a standard for the supply of potash against the yield response of the several experiments, the relation is found to be indistinct. His second assumption is that some other soil factors influence the response. For reasons of soil chemistry it may be expected that humus is a factor of that kind. In plotting the positive and negative deviations from the average line showing the relation between K-content and the response, against the humus content he does in fact find a relation. Van der Paauw in this way obtained not only confirmation of the rightness of the hypothesis on the influence of the humus, but also an estimate of the size of the influences of potash and humus. Here it must be emphasized that in this way the influence of a factor is established which in practice cannot be changed. The principle of interference is impossible in this case. The method can be extended to several factors.

The favourable experience in the first period with series of experimental fields is the reason that most problems of manuring in subsequent years until now have been approached in an identical manner. The technique of evaluation, however, could be improved on several points in the course of time. The later investigations were not only concerned with the macro-elements, but also with the micro-elements. These investigations have provided the data for the contemporary advise-scheme on manuring as it is given on the basis of soil tests in The Netherlands. The method was later on also used in the investigation of other problems.

Seen in retrospect the use of experiments on farm fields without immediately obvious deficiencies must be considered to have been a happy choice. In this manner the yield levels of the experimental fields were approximately equal with the result that it was possible to express the yield response in percentages of the maximum output. In doing so the elimination was achieved of the disturbing influence of several factors

in themselves unimportant for the size of the response, while the number of dimensions was reduced sharply.
This is much less the case or even not at all if the investigator explains the differences in mineral composition of the crop or, as the case may be, in yields between the fields. It might be expected that, generally speaking, the quality of a crop depends on less factors than the yield does. After his experience with grassland research some years previous van der Paauw took the first step in 1943, with his investigation into the value of chemical soil analysis for phosphate and potash with the aid of the chemical composition of the crop. On fields of rye and grass from all over The Netherlands samples were taken of the soil as well as of the crop. Until then data had always been used obtained partially with and partially without interference, but now only observations were used obtained without interference. Elimination of the influence of some factors by manipulation was omitted altogether; this must be done by the analysis. In this respect the data are comparable to those with which econometrics mostly deals. The idea of operating in this manner was not new to the investigation into soil fertility. It was not, however, a phase in the development of thought processes and the performance thereof did not take place or only with very little result.
Improved methods of evaluation and the experience obtained by the treatment of multidimensional problems enabled the researchers to investigate the factors on which the contents of potash and phosphate in the plant depended. Because of the nature of the problem the number of dimensions in which the relationship can be described is still relatively small. Starting from the hypothesis that yield is a function, albeit an as yet unknown function of a large number of factors, Visser finally took the last step. With the aid of the so-called polyfactor analysis in which all experience and ideas are collected which are used in the unravelling of multidimensional relationships by the graphic method, he attempted to establish those factors and the nature of the relationships. The necessary data were collected on trial spots. The results of the first polyfactorial investigations were published by him in 1949. Similar investigations have subsequently also been conducted by others with more or less success. The great importance was that in this manner the influence of a number of factors on the yield was determined quantitatively. In another way this could not have been done or only with great difficulty. This applies in particular to the influence of agriculturally important factors which cannot, or hardly, be varied such as humus content, soil structure, watertable, etc. Most of the knowledge in The Netherlands of the influence on certain characteristics of the profile and on the optimum watertable has been obtained in this way. It is possible to provide a quantitative explanation of the differences in yield between the fields. In 1952 Ferrari succeeded in explaining by this method 88 % of the differences in the yield of potatoes with the aid of thirteen factors. In this respect the not-explained residual variance gives an impression of the measure in which we can explain differences in yield by factors familiar to us.
For many people it remained questionable whether it would be possible to determine the influence of the separate factors only by using the variation present in nature. To what extent would it further be possible to sufficiently eliminate the correlations existing in nature between the so-called independent factors? Opponents talked about a "correlative or statistical method" in order to express their doubts in that manner. In a study *Comparison between experiments with and without interference* Ferrari in 1960 compared the possibilities of both methods. It was shown that the explanation

of a phenomenon is related to the formation of a hypothesis and to the testing, subsequently carried out, of that hypothesis with reality. In that case it does not matter whether the test is carried out with data obtained without or with interference. The difference is not fundamental and both methods have their advantages and drawbacks. The interference aims at the elimination of the influence of other factors which disturb the conclusion: actually, however, the ceteris paribus assumption is often incorrect. In the experiment without interference it is attempted to eliminate the disturbing influence of other factors by the mathematical analysis. It is found that this elimination is often difficult. With that in mind the researcher will always have to try to check the elimination in other ways.

The experiment without interference has the consequence that the drafting of a hypothesis is often omitted or that it is put forward with insufficient precision. Before now we have already submitted that soil fertility research (and this applies to all agricultural research) is particularly concerned with the study of relationships. It is our experience that hypotheses on these relationships can be adequately represented in so-called models. These models are simplified abstractions of the phenomenon which is being investigated. In our case these abstractions are first expressed in diagrams and next in mathematical equations. Further thoughts on this subject (Ferrari, 1964) show that most models used up to now are based on the assumption that the manipulation of an independent factor does not cause any change in other independent factors. This principle of ceteris paribus is assumed in experiments both with and without interference, but this assumption mostly lacks reality. In nature one has to deal with chain processes and feedbacks. This means that the assumption of a ceteris paribus situation must often be rejected as being incorrect also in the experiment with interference. In that case the manipulative experiment is based on a model too simple; the reality is more complex by which a ceteris paribus situation cannot be achieved, not even by interference. The investigation will therefore have to use more actual models which cannot any longer be evaluated by the usual methods. The model of the aspect analysis (Ferrari, 1957) already develops in that direction but is as yet insufficiently specific. In 1962 Ferrari introduced the method of path coefficients in fertility research by which it is possible to test and quantify more worked out models with chain processes and in which instead of one equation a whole system of equations must be drawn up. This method was used to determine the influence of a number of factors on the magnesium content of grass. Two of these factors are the percentage of weeds and the crude protein content in grass. In the original regression model these variables were assumed to be independent factors; it is clear that this is not a realistic assumption. As a result it became possible to test and quantify a model in which the two variables were both cause and effect.

Information on the size of the unexplained residual variances and the need to specify more precisely the relations between variables are so to speak a *challenge to the specialists.* On the other hand the development just discussed towards ideas on soil fertility as a multidimensional system with the correspondingly increased use of models leads to the conclusion that the mathematical treatment of soil fertility problems will increasingly become important. This is the *challenge to agricultural researchers.*

The Groningen school has been supported in the conviction that the development has been correct not only by the results of comparable methods in other sciences,

for instance in econometrics, but also by the break-through of the opinion in other sciences that the generally accepted manipulative approach does not satisfy in all cases. For instance after his lecture *Task and method of biochemistry* Veldstra answered a question in the following manner: "One must indeed be alive to the fact that as problems become subject of research in which in fact manyfold factors play a role simultaneously we cannot manage any longer with the usual method of: removing one factor and keeping the remainder constant."
Complaints which have recently been heard about the disappointing results of one fertility research in England, United States and Australia forcibly speak for themselves. In a paper in 1961, entitled *A quarter century in soil fertility research and a glimpse into the future* on the occasion of the 25th anniversary of the Soil Science Society of America Bradfield complains: "It seems to me that we cannot escape the conclusion that the reason we continue to investigate and reinvestigate certain aspects of soil fertility is, that we are not yet able to predict with confidence what will happen in the case we are interested in, because there are too many parameters involved which we are still unable to identify, measure or interprete." Somewhat further on he already indicates the way which seems familiar to us: "The old classical dictum of experimental science of studying one factor at the time by holding all factors constant except the one under study and then vary it systematically, is very good in the laboratory; but it is always difficult, and in many cases almost impossible, in the soil fertility studies involving the growing of crops and soils in either field or greenhouse."
In the Groningen school a methodology has been developed from which it may be expected that it will lend vigorous support towards the solution of agricultural problems.

LITERATURE

BOYD, D. A. 1961a Current fertiliser practice in relation to manurial requirements. *Proc. Fertil. Soc. 65*, 3–40.

BOYD, D. A. 1961b Fertiliser responses of maincrop potatoes: a re-examination of the experimental evidence. *J. Sci. Food Agric. 12*, 493–502.

BRADFIELD, R. 1961 A quarter century in soil fertility research and a glimpse into the future. *Soil Sci. Soc. Am. Proc. 25*, 439–442.

COLLIS-GEORGE, N. and DAVEY, B. G. 1960 The doubtful utility of present-day field experimentation and other determinations involving soil–plant interactions. *Soils Fertil. 23*, 307–310.

DEMING, W. E. 1946 Statistical adjustment of data. New York.

EDEN, T. 1929 Studies in crop variation. VI. Experiments on the response of the potato to potash and nitrogen. *J. Agric. Sci. 19*, 201–213.

EZEKIEL, M. 1930 Methods of correlation analysis. New York.

FISHER, R. A. 1935 The design of experiments. Edinburgh.

FERRARI, TH. J. 1949 Stikstofbemesting en bodemfactoren. *Landbouwk. Tijdschr. 61*, 111–120.

FERRARI, TH. J. 1952 Een onderzoek over de stroomruggronden van de Bommelerwaard met als proefgewas de aardappel. *Versl. Landbouwk. Onderz. 58*, 1.

FERRARI, TH. J. 1960 Vergelijking tussen proeven met en zonder ingreep. *Landbouwk. Tijdschr. 72*, 792–801.

FERRARI, TH. J. 1963 Causal soil–plant relationships and path coefficients. *Plant Soil 19*, 81–96.

FERRARI, TH. J. 1964 Auswertung biologischer Kettenprozesse mit Hilfe von Pfadkoeffizienten. *Biom. Z. 6*, 89–102.

FERRARI, TH. J. 1965 Models and their testing in agricultural research. *Neth. J. Agric. Sci. 13*, 366–377.

FERRARI, TH. J., PIJL, H. and VENEKAMP, J. T. N. 1957 Factor analysis in agricultural research. *Neth. J. Agric. Sci. 5*, 211–221.

FERWERDA, J. D. 1951 Over de werking van stalmest op bouwland. I. *Versl. Landbouwk. Onderz. 57*, 13.

GILE, P. L. 1914 On the plans of fertilizer experiments. *J. Am. Soc. Agron. 6*, 36–41.

HUDIG, J. 1918 De Hooghalensche ziekte; een nieuwe bodemziekte op zand- en veengronden. Den Haag.

HUDIG, J. en MEIJER, C. 1922 De invloed van zure en alcalische bemesting op den groei der gewassen (zes jaren praktijk op het proefveld te Spitsbergen). *Versl. Landbouwk. Onderz. 26*, 60–86.

HUDIG, J. en MEIJER, C. 1925 Het proefveld te Spitsbergen in 1925. Bericht van het Rijkslandbouwproefstation, Groningen, *24*.

HUIZINGA, J. 1936 In The Shadow of To-morrow. London.

PAAUW, F. VAN DER 1936 Het kalivraagstuk op zand- en dalgronden. *Versl. Landbouwk. Onderz. 42*, A 393–448.

PAAUW, F. VAN DER 1938 Over den samenhang tussen de groeifactoren en opbrengst, en de principes, die dit verband bepalen. *Landbouwk. Tijdschr. 50*, 795–830.

PAAUW, F. VAN DER 1943a Agronomie en Botanie. *Vakblad voor Biologen 24*, 107–116.

PAAUW, F. VAN DER 1943b Het oplossen van landbouwkundige vraagstukken door middel van enkele grote en series van kleine proefvelden. *Landbouwk. Tijdschr. 55*, 150–162.

PAAUW, F. VAN DER 1962 Effect of winter rainfall on the amount of nitrogen available to crops. *Plant Soil 16*, 361–380.

PAAUW, F. VAN DER en LANDE CREMER, L. CH. N. DE LA 1951 Toetsing van grondonderzoek naar fosfaattoestand op nederlands grasland. *Versl. Landbouwk. Onderz. 57*, 15.

PETERSON, B. J. 1957 Examples of the influence of soil science on agronomic practice. *Agron. J. 49*, 651–659.

PRUMMEL, J. 1957 Fertilizer placement experiments. *Plant Soil 8*, 231–253.

REITH, J. W. S. and INKSON, R. H. E. 1963 Effects of fertilisers and farmyard manure on swedes and turnips. *J. Agric. Sci. 60*, 145–157.

SANDISON, A. 1959 Influence of site and season on agricultural variety trials. *Nature 184*, 834.

SJOLLEMA, B. 1909 Onderzoek naar de oorzaken der vruchtbaarheidsafname van enkele gronden in de Groningsche en Drentsche Veenkoloniën. *Versl. Landbouwk. Onderz. 5*, 29–157.

UVEN, M. J. VAN 1935 Mathematical treatment of the results of agricultural and other experiments. Groningen.

VELDSTRA, H. 1962 Taak en methode van de biochemie. *Meded. Dir. Tuinb. 25*, 552–561.

VISSER, W. C. 1938 Kalktoestand en oogstopbrengst. *Versl. Landbouwk. Onderz. 44*, A 313–359.

VISSER, W. C. 1942 Over de bruikbaarheid van de grafisch-statistische bewerkingstechniek. *Landbouwk. Tijdschr. 54*, 403–416.

VISSER, W. C. 1943 Over enige grondslagen van het grafisch-statistisch onderzoek. *Landbouwk. Tijdschr. 55*, 163–170.

VISSER, W. C. 1949 De samenstelling van produktiviteitsschattingen op grond van vruchtbaarheidskenmerken. *Landbouwk. Tijdschr. 61,* 321–334.

VRIES, O. DE 1934a Düngungsfragen in ihren gegenseitigen Beziehungen als polydimensionales Problem. *Die Ernährung der Pflanze 30,* 373–382.

VRIES, O. DE 1934b De veranderingen in den grond door lang voortgezette bemesting met enkele stikstofmeststoffen. *Versl. Landbouwk. Onderz. 40,* A 505–541.

VRIES, O. DE 1934c Enige aspecten van het kalktoestandsvraagstuk. *Landbouwk. Tijdschr. 46,* 677–705.

VRIES, O. DE 1936/1937/1938a Das Serienprinzip in Feldversuchen. I, II, III. *Z. Pflanzenernähr. Düng. 43* (1936), 83–93; 4(49) 1937, 291–294; 8(53) 1938, 73–77.

VRIES, O. DE 1938b Het serieprincipe bij veldproeven. *Landbouwk. Tijdschr. 50,* 340–358.

VRIES, O. DE 1939 Opbrengstcurven en opbrengstwetten; de wisselwerking der groeifactoren. *Landbouwk. Tijdschr. 51,* 727–763.

WOLD, H. 1956 Causal inference from observational data. *J. Roy. Statist. Soc., Ser. A 119,* 28–60.

YATES, F. 1964 Sir Ronald Fischer and the design of experiments. *Biometrics 20,* 307–321.