CWI
P.O. Box 4079, 1009 AB Amsterdam, The Netherlands
Telephone 31 - 20 592 9333, telex 12571 (mactr nl),
telefax 31 - 20 592 4199

CWI is the nationally funded Dutch institute for research in Mathematics and Computer Science.

CWI Tract 87

The way of mathematics and mathematicians

From reality towards fiction

A.F. Monna

Centrum voor Wiskunde en Informatica
Centre for Mathematics and Computer Science

ISBN 90 6196 415 6
NUGI-code: 811

Printed in the Netherlands

PREFACE

In the preface of this essay I think a few remarks on the way by which I came to this subject should be given. After more than half a century of activity in the world of mathematics I feel a growing want of motivation. Half a century ago - at the beginning of my career - modern mathematics was still in its early years and in any case university programmes were mainly classical. Just mathematicians of these generations have observed a gigantic development. They have experienced the transition from the old phase into the modern one. This development raised the question in me what the sense of this all would be and what mathematics is after all.

I feel a growing need of seeing structure in the evolution. Such questions ask for historical foundation. I see a way in research of fundamental historical kind in connection with philosophical reflections. Earlier I already published some studies of a somewhat more special kind in this area. The present study gives a broader picture of the general way of the evolution, ultimately leading to a tentative to formulate a standpoint with respect to the nature of mathematics. In this area standpoints grow gradually and they can change. Therefore they are of a subjective character. Just this makes discussion with other mathematicians desirable. Here especially my discussions with D. van Dalen, which appeared to be very fruitful, must be mentioned.

Contents

Preface to the second edition

This essay is an essentially enlarged and revised edition of the first edition from 1984. The publication of a book written by Professor S. Dresden entitled *Wat is creativiteit*? (Amsterdam, 1987) stimulated the author to write this new edition. Although Dresden is mainly concerned with reflections on creativity in the area of Arts - the alpha-culture - science is not entirely excluded, and it was most exciting to observe that many aspects discussed in this book find parallels with facets in the evolution of mathematics. Indeed, creativity is a most important feature of mathematics. This essay and Dresden's book are in some way of the same character. The essay is not mathematics, it is a writing about mathematics. Special emphasis is laid on the influence and various forms of creativity in mathematics. In an Appendix I add some remarks on the fundamental concept of "proving" in mathematics: "proofs" are in some way the closure of acts of creativity.

As a consequence the style of writing is rather unusual for mathematicians, being more contemplative and speculative, not always verifying the strong criteria for mathematical writings. It must be taken into account that strong and sharp definitions of the various concepts, as is necessary in mathematics itself, can hardly be formulated. Who can exactly define "making" or "doing" mathematics, "inventing" respectively "discovery", "creating" or "creativity"? The many interrelations between such concepts make it very difficult to seperate them exactly in a discussion, although their significance is different. Therefore it is difficult to treat the subject in the traditional mathematical form of chapters, paragraphs, sections, under-sections, etc. Reading Dresden's book, one will perceive the same style. For these reasons I designed the book as an "Essay", because it is perhaps closer to Alpha-science than Beta-science, where essays are not customary. In this context I point to the relations with and the influences by the discipline Signifika, a philosophical-linguistical domain that is concerned with acts of communication by languages, initiated in the first decades of our century by the Dutch mathematician-philosopher G. Mannoury, who was active in the group of L.E.J. Brouwer, D. van Dantzig, Fred. van Eeden and some others. I found an opportunity in this essay to give some more details. A more complete introduction should have taken too much place. In fact parts of this book are in the domain of Signifika, rather than in mathematics.

Finally a more personal remark. After 60 years of uninterrupted intensive working in mathematics, my interest in new concrete mathematical theories shifted to more philosophical contemplative aspects. At an old age, one asks himself what after all would be the deeper sense of what one has been doing: the way towards philosophy and human culture. I hope that this essay, which was written as it came up in my mind, may stimulate others to continue in the same field, since there seems to be a growing interest in this subject among mathematicians.

My collegue A.W. Grootendorst from the Technical University at Delft and my sons A.D.A. and W.A.A. Monna have critically read the manuscript. Because of my age it would have been impossible to publish this essay without their assistance.

A.F. Monna

De Bilt/Utrecht
The Netherlands
Summer 1991

The Way of Mathematics and Mathematicians

From Reality Towards Fiction

Prof. Emeritus A.F. Monna
State University of Utrecht, Utrecht, The Netherlands

I. Introduction

I.1. The aims of this study

In previous papers I published some reflections on general trends in the progress of mathematics. These reflections were of a mixed character: partially of historical kind, partially mathematical-philosophical [1]. The subjects concerned:

a. History and analysis of the phenomenon of tendencies towards algebraization of mathematics;
b. The introduction of sets as a methodological apparatus, and their influence in mathematics and in particular in analysis (the notion of existence in mathematics);
c. Reflections on external, respectively internal influences in the evolution of mathematics;
d. Reflections on the gigantic growth of mathematics in modern times.

These studies differed from the most common historical papers. Most of these are studies on the history of special domains, investigations on the work of mathematicians in the various periods of the past, etc. My aim was a different one. I tried to eliminate special results and theories in order to find the general way of mathematics. So I considered the evolution of mathematics as a whole. In studies of this kind conclusions must be based on results and theories, on facts. But their role is only to support opinions on the general evolution. What I want is a kind of synthesis, a total-history, a history of mathematics finding a place in the general *History of Ideas*. Themes may be: "The development of mathematical concepts"; "The rise and ways towards abstractness"; and as a main theme the Idea to form a *theoretical model* of mathematics. There are not many studies of this type but I think there are numerous more special studies on which such a general history can be based.

In the present study some ideas on a synthetical study are formulated. It has the intention to serve as a program for a research project in this field. I shall discuss problems on causal relations in the great line of the evolution and suggest to see the evolution of mathematics as a chain of phases, etc. A study of this kind is not merely descriptive. It does not concern the history itself but a *Theory of the evolution*. Such a Theory is not a natural or logical consequence of the facts and therefore of a new level. I think it is important to consider another aspect: how were fundamental developments realized? Under what conditions and situations did mathematicians create new ways? How did they come alive? Can all be explained by purely historical processes or was there more? The attitude and the way of working of mathematicians must find a place; it is a psychological aspect of the progress of mathematics.

A study of this kind should be concerned more with *explanation* or better understanding of the rise of developments than with a description of facts. This is a difficult domain. When we ask how certain developments came to life the answers will have the danger of subjectivity. Arguments are difficult, more difficult I think than when it concerns description. In the present paper one will find some personal ideas on some of the essential problems. They should be seen as first attempts and more profound study will be necessary in the framework of a research project. In an Appendix some concrete examples will be given of the problems which I have in mind, including some personal remarks. Such a project can presumably not be the work of one man and will take several years.

I.2. Subjectivity and objectivity

I will make some remarks on the question of subjectivity and objectivity for such a project. When it concerns reflections on the great line of the evolution of mathematics it is not easy to avoid personal opinions. Does strict objectivity exist? It should be kept in mind that the historian already knows what has happened after the developments he wants to analyse. In some way he has an overall picture of the field. Can he escape from the danger of taking too less distance from his complete knowledge when he wants to describe and explain certain developments? Will he not be inclined to project his own opinion on the facts of the past, and to take them as the base of his judgments? This is a danger when interpreting the evolution. I think I did not escape myself. In my publication [1c] on external, respectively internal influences in the evolution of mathematics, I saw the classical period as one with mainly external influences. However, I looked on modern mathematics as a science which is developing mainly in an autonomous way, a science that is produced by internal factors. In my view there is an essential gap between these periods which I consider as a discontinuity in the evolution. But probably this view is based on my personal concept of mathematics as a free mental activity. In this view internal factors lie at the base of the explosive developments of modern time. What can be said about them? Modern mathematics as an "idea"? Probably my previous studies on strong and weak existence in mathematics are not free from personal concepts. Earlier I described the phenomenon of the algebraization of mathematics [1a]. This is also a personal interpretation. Surveying the history as a whole I came to my interpretation as the way of "algebraization". Starting from this concept I tried to find more examples which confirm this point of view. But is this an objective way of research? For modern theories and results algebraization is perhaps a feature which is quite evident and it can be accepted as an objective fact when thinking of the all penetrating influence of algebraic structures. But is it allowed to describe a systematic algebraization as an essential aspect beginning with Descartes? How did mathematicians as Bourlet and Drach look on their attempts to "algebraizise" the notion of the derivative of a function? (see [1a]). Was there a conscious strive towards algebraization, or is there a projection of personal concepts on the course of history? In the same light one has to see my reflections in the present paper on the question whether there exists what I shall call a "natural progress" in the evolution of mathematics.

Should a so subjective historiography be rejected? I think there is no reason to do so. Just by means of interpretations from different points of view it is possible to get more insight in the nature of mathematics and this seems to be especially useful in a time of explosive growth.

II. The Way of Mathematics

II.1. The evolution of mathematics: a natural development?

In my previously mentioned papers on the great line of the evolution it was stated that there are certain essential aspects in the evolution which deserve special attention. I designed them as *discontinuities* in the evolution [1d]. Examples were given as for instance the arithmetization by Descartes, the developments around Leibniz and Newton, Cantor and the theory of sets, and in particular the transition from the classical to the modern era.

Such discontinuities should be considered as a breakthrough in the course of the evolution in the sense, that after such a point, or after such a period in the developments, the image of mathematics changed considerably and essentially new aspects were added. They are not always connected with the work of one special mathematician and they cannot always be fixed exactly in time. Sometimes the realisation took several years and needed a long time to come fully in development.

This notion of "discontinuity" will be a major topic in this paper. It is related to the way of progress in mathematics. Is there an aspect in the evolution which can be called "continuity", without giving for this moment a definition of what should be meant by it? Certainly there is something like continuity in the sense that mathematics is steadily growing, for instance in such a way that old areas are incorporated in new broader areas by activities of generalization or otherwise. There is the tendency towards "general theories". There is also the phenomenon that subjects are pushed to the background but in later phases return in a new form. It is a kind of global continuity. But, when speaking on discontinuities, that is not the feature I mean. I think of a psychological discussion on how progress is realized and on the connections with the past.

Let me illustrate what I have in mind by some examples. In a historical study about the axiom of choice [2] Cassinet constructs a reasoning leading to the conclusion that the axiom of choice is a natural product ("produit naturel") in the evolution of mathematics. This conclusion is based on several examples from the history of mathematics in which acts of choice have played a role. There are examples from the period before as well as after Cantor in which, however, sets as such do not play a role. In particular he mentions some papers from 1896 by Bettazzi, a predecessor of Zermelo.

Here the question must be posed what should be meant by a "natural" process, a "natural" product, in mathematics. Should it be a process that finds its base in the nature of objects, an issue from the nature of things? Or is it simply to be seen as a process with obvious, plausible inferences, results that are obtained by skilful applying the available apparatus such as it exists? Is this the way in which progress in mathematics - fundamental progress by which the borders are essentially extended - is realized? Is the progress in mathematics a "natural process", whatever that may be? This is a question that occupies me in the present paper.

Another example, an older one. In a paper from 1896 Volterra studied integral equations. To solve the equation he reduced the integral equation by means of a process of discretization to a linear algebraic equation with infinitely many unknowns. He then finds the solution of the integral equation by passing to the limit. Volterra says that this method is based on the "idée toute naturelle" to consider the integral equation as the limiting case of algebraic equations (see [3]).

Is this really a method of treating the problem which comes from its nature? Here it concerns the transformation from the continuous into the discrete. The relation between these two aspects is not simple. Perhaps Volterra had the idea that this method was obvious, in some way trivial. Perhaps he had thought of Riemann's definition of an integral as a limit of finite sums, although he gives no references. But in that case the situation was a different one. Riemann wanted to give a definition of the concept of an integral (I return to this later on). Was discretization a normal tool in the classical period? In Antiquity there was Archimedes and the method of exhaustion, but in the ancient geometric period this had no direct follow up. I think that it cannot be said that in 1896 it concerned a quite "natural idea". Volterra approximated given equations. Further research may give more and new light on the role of discretization in the development of mathematics. Much more can be said on the relation between discrete and continuous. It concerns here basical problems, problems at the origin. I mention for instance non-archimedean analysis, i.e. analysis over non-archimedean valued fields, in which there are aspects of discreteness that cannot be given here in a detailed way [4]. I only point to the non-archimedean valued field of the p-adic numbers; the set of the values of its elements is the set of the powers $p^n (n \in \mathbb{Z})$ for a fixed given prime number p. The ordinary analysis, however, i.e. analysis over $\mathbb{R}$ or $\mathbb{C}$, "archimedean" analysis, is not a sort of limiting case of non-archimedean analysis ([5]. See also [6]).

In my opinion there is in the evolution of mathematics something more than a skilful application of the available tools, and there are aspects that cannot be explained by something like causal relations with the past and with existing areas. This can be illustrated with examples from my own work. They concern two areas: potential theory and non-archimedean analysis.

In the early forties I started with non-archimedean analysis, in particular non-archimedean functional analysis. There were only few publications in this area. There was an example in Banach's book *Théorie des opérations linéaires* (1932), where there is a reference to some studies over the complex numbers. So the idea of starting the theory over the p-adics was not so far away. That essentially new ideas appeared to be necessary in the development of the theory is quite another matter. That is a situation one always meets when new theories are developed.

There is a very different aspect to be found in my work on potential theory. In the late thirties potential theory was still very classic. There were important publications by Brelot opening new ways; topological elements entered the scene. I could also add some results. But application of the theory of linear operators did not find a place in this theory at that moment. I formulated a certain problem in the theory of what is called "Dirichlet's problem" which concerned a question of unicity and had the idea to attack this problem with methods of the theory of operators. I remember studying chapters in the book by M.H. Stone, *Linear transformations in Hilbert space and their applications to analysis* (A.M.S. 1932), hoping to find there the possibility of applying theorems on the extension of operators. But I did not find there the right way, and could only give a partial answer to my problem. Later a complete solution was given by other mathematicians (Brelot, Keldych), partially independently from my work, and the theory was further developed. I think the psychological side of this piece of research is different from the situation in non-archimedean analysis. Here the idea was to formulate a new problem in such a way that the theory of operators and the at that time still rather young functional analysis, a domain until then without contacts with potential theory, could now be introduced. Was this something like a "natural" development, an obvious way?

Before proceeding in a more systematic way let me add here a last example which is a good illustration for the situation. Later on some more examples of wider extent will be given. In his thesis from 1931 (Groningen) *Studiën over topologische algebra*, D. van Dantzig introduced topological algebras as a combination of topological and modern algebraical properties, areas that were in full development, but in origin conceptually separated. In his introduction he posed the question: "Waarin is dan echter de eigenlijke oorzaak gelegen van de fundamentele rol, die het getallen continuum in de gehele wiskunde vervult?" [7]. Can such a question, being at the origin of a new domain, rise in a "natural" way? I do not think so. In my view it is the result of profound thinking on the background of the areas, and there may have been a spontaneous idea. The introduction of such a new field cannot easily be seen as an obvious consequence in existing theories: a new idea was necessary.

I believe there is more in the evolution than a skilful application of results and methods. In my opinion such developments are not determined by historical processes. They grow under the influence of the past developments, but they are not determined by them.

After these examples I shall now formulate and comment several problems which I propose as a subject of further research. These problems do not concern the facts, the theories, etc. themselves; they are beyond these facts. I see them as problems in the domain of Theoretical History. They all concern problems on *necessity*, *causality*, in the creation of essentially new areas and the relations between new developments and the past.

Is there in the succession of theories and results an evolution which to some extent can be considered as a kind of necessity?

It will be clear that in the first place I think here on the really great results in the evolution, the facts that involved essential progress or changes, not on the succession of theorems in existing theories. On this smaller scale there are evidently developments which find their origin directly in the previous history. It is the way of constructing a theory on the base of new concepts and ideas. Sometimes something as "natural" patterns can be indicated, patterns which seem to be inherent with the nature of the objects. There are analogies, imitations, generalizations, algebraic methods, etc. In this sense there is a kind of necessity or, weaker, obvious inference.

But thinking of the really great activities, the performances that essentially changed the direction, the creation of new concepts, I believe that the situation is quite different. Earlier I used the term "natural process" and illustrated the idea in my examples, but it is not easy to give a precise definition of such a concept. It is connected with the existence of a law in the evolution, deductive ordering of the area, necessity in the evolution, processes that are direct issues from the past and proceed along more or less fixed lines. I think in general large scale progress cannot be explained as a "natural process". When it concerns the creation of really new areas and new ways, there is "more" that is needed. It is not easy to say what "more" means. One can get some idea of what I mean from the foregoing examples. It concerns mental activities, creative powers that are connected with deeper layers of thinking and knowledge. This "more" is in some way connected with the discontinuities in the evolution, mentioned before, and what happened after such discontinuities. It is the *freedom in creation* as cognitive activity from which they result. I believe that this is one of the essential aspects of the evolution of mathematics. It is this "freedom" by which continually higher levels could be reached [8]. But the situation is still more complicated. Is there a necessity in the sense that theories in some way would necessarily have been created in the course of the evolution? This leads me to the next question:

To what extent are the great developments determined by the previous history?

Here again in my opinion the freedom in creation plays a role.

It is of interest to compare the situation in mathematics with the circumstances in physics. In a lecture on the tendency towards abstraction in modern Art en Sciences, given in Salzburg in 1969 [9], W. Heisenberg said that in modern natural sciences the questions are always determined by a historical process and the attempts of the scientists were always directed towards answering these questions.

I do not know exactly what Heisenberg meant with the word "determined", but I suppose it should be understood as something of a causal relation. Whatever this may be, in my opinion there is a different situation in mathematics. I think it goes too far to say that in mathematics questions and theories are determined in a causal way by a historical process. I do not believe that, in a general sense, the activities of mathematicians were and are in a strict causal sense directed by historically determined fields of problems. Mathematics was, and still is, developed under the influence of the past, but I think we cannot say that it was in a strict causal way *determined* by the past developments. The relations with the past should be expressed in a more restricted sense: theories are in some way prepared by the past, they are initiated by older theories, but not always in a causal way. In fundamental developments someone had to do a final step, perhaps in cooperation with others, to cross a border, to open a door to a new area. In my view "free creation" is an essential aspect of mathematics, "creation" in non-platonic sense.

To give an example on the large-scale development of mathematics: can the thesis be defended that the "infinite" as an idea in mathematics, with all what happened around it, was settled ultimately on the basis of the "finite" in some causal way? Does it find its final roots in deepest sense in the "finite"? Is the idea of the "infinite" an extreme consequence of the "finite"? Evidently the natural numbers to some extent have *prepared* the way, just as in all mathematics - perhaps with the exception of geometry - but I have some difficulty in seeing this in a causal relation, as an aspect of determinism.

In a recent book of L. Young, *Mathematicians and their times* [10], the author writes in another sense about causality in mathematics. In his Introduction he states that "in history, and generally in science, our main interest lies in a supposed causality". He remarks that in some sciences causality can be checked to some extent by experiment but that in history "*supposed* causality is all we can aim for". It will be clear from the present study that I do not agree with him, at least with respect to mathematics. It can be one of the aims of historical studies - and for mathematics it is - to see whether there is causality in the line of the evolution and to what extent. But writing history *supposing* causality means forcing history in a framework which is not always in agreement with the developments of history. Does he mean a law a posteriori? It is not the way to *suppose* causality. On the contrary, one of the things that make historical writings interesting is to observe that in the evolution there are developments that can not be explained by causality, and in my opinion such developments exist in mathematics. Just these facts emphasize the freedom in creation in the human mind and they may be a subject of psychological investigation. *How* do new ideas arise? Compare Hadamard's book *An essay on the psychology of invention in the mathematical field* (Princeton, 1949). It is another matter that I have the impression that the general history of mathematics is often treated in a way as if mathematics consists of a sequence or sequences of results in succession, suggesting in some way an evolution of causal relations in theories and results, a matter of deductive ordering of the area. The episodes of strain in the lines of evolution could have more emphasis. Is this supposed causality? Is history commonly written in a way of causality a posteriori? Is there something like an intellectual need of causality? Is this what Mie (1893) meant when he wrote in a treatise on differential equations about "unser Causalitätsbedürfniss"? [1c, p. 15]. That factual accuracy would lead to a "primitive and childish view" ([10], p. 12) is not my opinion, provided that this is accompanied by attempts to explain developments. I think this is just one of the aims of historical research.

To finish this chapter another, rather speculative, question suggested to me by D. van Dalen: *Does the present stage of mathematics suggest its future?*

Reflections on the past evolution in relation to the present stage may be a contribution to a discussion about this interesting question.

II.2. Basic discontinuities

As a contribution to a first answer to these problems some examples of fundamental developments will be given which I consider as "discontinuities", developments that are not "natural". They concern the introduction of new ideas and concepts which fundamentally changed the image of mathematics. In my opinion they are not - or at least not strictly - connected with previous developments by arguments of necessity. In a research project these examples will certainly need further elaboration. Afterwards I shall use these examples to support an idea of writing the great line as a succession of phases.

(i). An important development which in my view must be considered as non-natural, a development that cannot be explained by reasons of necessity, took place in the 17th century. There is no need for more exact dating. All the more, as I observed before, discontinuities cannot always be strictly fixed in time. In this period the transformation took place from the towards geometry directed mathematics of lines, triangles, remarkable points, planes and all kinds of curves and other geometrical figures with geometric methods (elementary geometry, geometrical maps for instance the inversion), into the mathematics in which the notion of number is going to play a dominating role. It is an aspect of connecting the continuous with the discrete, the *arithmetization of mathematics*. This is a first step towards the *algebraization*, which is an important aspect in modern mathematics. Of course Descartes must be mentioned here (see [11]). It can be said that the geometric period with geometric arguments came to an end, without entirely disappearing (Monge, Poncelet).

Algebraic methods in geometry began to prevail and this direction is continued up to modern times. Algebra entered the picture as a method for solving problems outside algebra itself, for instance in geometry. Later algebra created ways of formulating problems *in itself*, and in a more recent period algebra penetrated analysis. I think here it is difficult to see elements of necessity in the developments. This is an example of the freedom in creation as source of the evolution.

(ii). In the 17th century we see, besides the work of Descartes, the introduction of infinitesimal calculus connected with the names of Leibniz and Newton, a development which I see as an important discontinuity, although there was some preparatory work (Fermat, Cavalieri). But I want to go further backwards in the line of the evolution. There are two elements that should be discussed in this framework.

First there is the question of the introduction of variable numbers in mathematical problems and reasonings. Next to arithmetical and purely algebraical problems, where problems with fixed numbers are studied, the element of change, expressed by numerical variables, entered in mathematics and this opened new areas. Variables replaced the dominating role of fixed numbers. I shall not try here to trace the origins of the introduction of variable numbers in mathematics. In a more complete study one should perhaps go back to Greek philosophy (Heraclitus, Parmenides). Was there some necessity in this development?

Secondly there is the introduction of the concept of numerical functions. Both subjects were in some way related to physical reality and are historically connected. In history this has been a very fundamental step in the development of nearly all areas of mathematics. Were there elements of a "natural" development, of a causal relation with the past in the first appearance of this subject? I have some doubt, because in essence there is an abstract background for a general idea of functional relation - in any case in somewhat later stages - but a study of the early years of the rise of the idea of function will be necessary to bring more light. The history of the idea of a function is a long one. It goes from functions as analytical expressions, especially for making calculations, via Dirichlet's general definition to the modern notion of a map [12]. See how Dirichlet formulated a definition of a continuous function for emphasizing the difference with a function as analytical expression: "Man denke sich unter a und b zwei feste Werthe und unter x eine veränderliche Grösse, welche nach und nach alle zwischen a und b liegenden Werthe annehmen soll. Entspricht nun jedem x ein einziges, endliches y, und zwar so, dass während x das Intervall von a bis b stetig durchläuft, $y = f(x)$ sich ebenfalls allmählich verändert, so heisst y eine stetige oder continuirliche Function von x für dieses Intervall. Es ist dabei gar nicht nöthig dass y in diesem ganzen Intervalle nach demselben Gesetze von x abhängig sei, ja man braucht nicht einmal an eine durch mathematische Operationen ausdrückbare Abhängigkeit zu denken".

How did Dirichlet come to this quite new definition? I do not know indications in the works of preceding mathematicians tending in this direction. I think it is not easy to see in his definition an obvious consequence of previous developments, a kind of necessity, a causal relation. Here is the very beginning of the general notion of a map, functions as *mathematical entities*, and, when we think of intervals as domain of definition in a somewhat later phase, I perceive here a first small step on the way of forming collections. It is a hierarchical process that will find its continuation - but neither by means of a "natural" process - in the work of Cantor. The notion of a limit - inexact as it was in the beginning - could develop on this base. In this period analysis gradually was developed; its beginning - a discontinuity - is connected with the names of Leibniz and Newton. This period was not free from more discontinuities, but were they of an equally fundamental importance as the very beginning of this development: the introduction of numerical variables and functions? Without derogating from inventiveness at that time - good ideas and inventivity are indispendable in any development - I am inclined to see that period of analysis, based on Leibniz's and Newton's work, in some measure as one of a technical development, a technical process. For the birth of the concept of functions see [1f] and [12].

However, looking at this period as a whole and comparing it with the earlier geometric period, I think it must be considered in its *totality* as a discontinuity in the evolution; this took many years and the work of many mathematicians.

(iii). Now we come to Cantor and the theory of sets. I think there is no need to indicate here the fundamental importance of this development in mathematics. Nearly all modern analysis is based on it. Next to the concept of a function this was the decisive step towards collectivization. Many mathematicians contributed to the further introduction. The theory of measure, already studied by Cantor and continued by Jordan, Borel, Lebesgue must be mentioned. There were Arzelà, Fréchet, F. Riesz with function spaces, metrical spaces, etc.

Several questions can be connected with this point in the evolution. They concern necessity. To what extent was the mathematics of the foregoing period, the classical period, a condition for Cantor's work? It is clear that great mathematical erudition was necessary. There were preparatory studies, especially by Dedekind. Long before Cantor there were discussions on the actual infinite. Johann Bernoulli and Leibniz played a role here. Cantor refers for example to Riemann (see [13] and notes in Cantor's collected works). In the very beginning there were countable sets. Cantor introduced an essentially higher level, ideas of a fundamental new character. I believe that it is not so easy to consider Cantor's work as a more or less "natural" process, as a necessity in the evolution, as a development in some conscious causal relation with the past. Cantor did a step which in my view is scarcely connected with the past, a step that cannot easily be explained from the existing mathematics at that moment. Could a theory of sets - perhaps in a different form - already have been created in an earlier phase? And if Cantor had not developed his theory, would there have been at some epoch another mathematician to create a set theory? Evidently these are purely speculative questions and a reasonable answer seems to be difficult. But such questions are typical for the situation with respect to the *evolution* in mathematics, which I do not see as identical with the *history* of mathematics.

The arguments to support my opinion that the period beginning with Cantor must be seen as discontinuous in relation to the previous developments, can be found in Cantor's works, but especially in the applications to analysis. Set theory has led to results which are totally different from the classical results. They concern, for instance, properties of collections of the objects of classical analysis: sets of continuous respectively discontinuous functions, sets of functions in relation to Fourier series, measure-theoretical subjects, etc. There are questions about the characterization of sets of all objects of a certain kind. Earlier I introduced, in an attempt to characterize the differences, the notions of strong and weak existence [1c]. Such ideas could not be posed in the classical period. They are connected with the past, they were sometimes *prepared* by classical analysis, but they could not be foreseen and they were not *determined* by the previous theory. Such results are on an essentially new level. This can be illustrated with an elementary, very simple example. Working in a given set E of functions, a function space, determined by some specific conditions, theorems often begin with: "Let f....", without any further specification. This can be seen as a reasoning on an abstract, higher level. The non-mathematicians shall be inclined to ask: "Which f do you mean?". The mathematician, however, does not see any difficulty in such a statement. Perhaps just such abstract formulations make that mathematics is called difficult in the non-mathematical world.

The history of the penetration of Cantor's ideas in the first years after his publications is an interesting subject of further research which may support my idea that in mathematics not all problems are determined by a historical process. However, this is a subjective opinion. D. van Dalen informed me that in his opinion some "continuity" as regards to set theory can be defended. This is an interesting subject for further discussion.

(iv). The phenomenon of axiomatic methods in mathematics is a broad subject for discussions on questions concerning "natural" progress, on necessity, "discontinuous" developments. In previous publications [1c], [1d] I already made some remarks on the development of axiomatic methods and I observed that a line can be drawn from geometry in Antiquity to the modern widespread axiomatic methods in geometry, and also in algebra and analysis. But has it real meaning to draw such a line if we are, for instance, concerned with the explanation of the appearance of axiomatic methods outside geometry?

The history of axiomatic in geometry is very well known. So I will restrict myself to just a few remarks. After Euclides there were the struggles about the axiom of parallel lines in the 18th and 19th centuries. Saccheri already did some axiomatic work, but this work had not the level of the later solution of the problem by Bolyai and Lobatschewsky. By the work of these two it became clear that geometries different from euclidean geometry are possible. Can this insight in the old problem be seen as a "natural" consequence in the evolution? I think an essentially new idea was born, not as an obvious consequence of the previous development. Discussions on the question whether the "real space", physical space, was euclidean or not are connected with this new insight. This line continues to Hilbert's *Grundlagen der Geometrie* (1900), which is the starting point for further developments.

There is a difference between the axiomatic of Antiquity and modern axiomatics which in my view is essential. In Antiquity the objects which are at the base of elementary geometry - points, lines, planes - were introduced by means of definitions - only apparent definitions - such as: a point is what has no parts, a line is what has length, etc. Next postulates are formulated concerning these objects. They are to be interpreted as predicates which can be reduced no more. (see Dijksterhuis [14] and chapters in Enriques [15], [16]).

In his *Grundlagen der Geometrie* Hilbert reversed things. A system is constructed by means of axioms. They concern objects called points, lines, etc., which are not defined. The geometrical elements are introduced by the way of implicit definition, just to avoid the difficulties of explicit definitions. I quote from Hilbert [17] p. 2:

"Erklärung. Wir denken drei verschiedene Systeme von Dingen: die Dinge des ersten Systems nennen wir *Punkte* und bezeichnen sie mit $A,B,C,\ldots.$; die Dinge des zweiten Systems nennen wir *Gerade* und bezeichnen sie mit $a,b,c,\ldots.$;

die Dinge des dritten Systems nennen wir *Ebenen* und bezeichnen sie mit $\alpha,\beta,\gamma,....$; die Punkte heissen auch die *Elemente der linearen Geometrie,* die Punkte und Geraden heissen die *Elemente der ebenen Geometrie* und die Punkte, Geraden und Ebenen heissen die *Elemente der räumlichen Geometrie* oder *des Raumes"*. On this base a system of axioms is introduced and the usual properties are proved. The nature of points, etc. is thus realized in an implicit way.

It may be that Hilbert had the opinion that there was no need to give definitions of the elements because everybody knew what is meant. But the way Hilbert formulated his intentions gives me the idea that an essentially new method was opened, which returned later in axiomatic methods in algebra and analysis. There is some resemblance to the situation, considered before, with regard to theorems on functions, without explicit information. Non-mathematicians may ask perhaps how it is possible to formulate reasonings without precising the nature of the objects. However, this is common in mathematics and to some extent even characteristic in mathematics of our time.

Although it can perhaps not be denied that there is some evidence in the development - there were earlier attempts, for instance by Pasch -, I think it is not so easy to explain this axiomatic work as a "natural" consequence of past developments, as a sort of necessity, as a causal relation. (for the question of implicit definition see also [18]).

Up to now I considered the line of axiomatization in geometry. This is in some way an internal development because originally the axiomatic method came from geometry. However, the introduction of axiomatic methods in algebra and analysis, considering the various subjects of mathematics, can be seen as an external development and this seems to lead to more complicated questions when we want to give an explanation. Indeed, in classical algebra and analysis we find no motives for the introduction of axiomatic methods in these areas. There are intriguing questions here. By which way mathematicians came to introduce axiomatics here? To what extent was this a "natural" process? Has there been an influence of the development of axiomatics in geometry on the introduction of the theory of groups, vector spaces, analysis in axiomatic form? Or does it concern an independent development? To what extent was Cantor's work a necessary condition in this evolution? How did axiomatic become a method for itself, operating on undefined objects? I propose these questions to be a subject of research.

(v). There are good reasons to consider the foregoing remarks on axiomatics against the background of the fundamental discussions on the foundation of mathematics towards the end of the 19th and in the 20th centuries, which have led to various trends as formalism, intuïtionism, and, more recently, the development and perhaps some decline of Bourbakism. They concerned different opinions on the nature of mathematics and the problem how mathematics should be founded. The same questions as before can be posed again. How were these trends introduced and what was there place with respect to past developments? In particular I mention intuïtionism with its very special ideas on the way by which mathematics must be developed: mathematics as a mental activity, constructivity, logic and mathematics, the rejection of the principle of the excluded third, etc. Intuïtionism must be considered as a fundamental discontinuity. That intuïtionism is not accepted by every mathematician is another matter. I shall not discuss this; there is much literature and, furthermore, I must leave this to the experts in this area.

II.3. Case-studies

In the preceding section some great developments were considered which have influenced the whole of mathematics. Next I will consider some special cases. They do not touch the whole of mathematics but they are interesting enough to analyze their realization. The question of "natural" or "non-natural" progress will again serve me as guide. Case-study is perhaps a kind of game, but it is a game with deep backgrounds. It is a way of getting an image of the evolution of mathematics and it may be helpful to get insight in the nature of mathematics. It is a game with subjective conclusions; it is an activity which needs careful examination of the conditions under which certain developments could be realized. It is a way of travelling through history.

(i). In II.2, example (iv) I observed in Hilbert's work on the axiomatization of geometry an aspect of a conversion of ideas. This aspect can be observed at more places. First there is Riemann's definition of an integral. After many years of using integrals in the old traditional way as an area or as primitive function, i.e. the inverse operation of differentiation, Riemann broke with the past and went a new way. In a classical paper [19] he asked what the meaning should be of $\int_a^b f(x)dx$. Apparently an integral was no longer a given concept for him. Here we see the aspect of conversion: traditional properties were the starting point for him. How did Riemann come to this idea? There are no indications in his treatise. The idea was essentially new, and I wonder whether some "natural" consequence of preceding developments can be ssen here. In my opinion the later work of Lebesgue, however important it was, was less radical in this respect. On the other hand, in a later development in this line, how should from this point of view the definition of an integral as given by Perron be considered? He took as a starting point the classical relation between integration and differentiation. Apparently a new idea!

The idea of a conversion of the line of thought can also be observed on a larger scale. The development of the theory of groups for instance is an example. Beginning with properties of concrete groups, for instance transformation groups, the introduction of abstract groups is based on the same idea. Properties are taken here as definitions or as axioms. This is the way of axiomatization. Bourbaki's definition of an integral as linear functional has the same background: the linear structure is the point of departure. To some extent this can be seen as a consequence of the method of algebraization. Is it a "natural" development?

It can even be defended that there is an idea of conversion in the way by which non-euclidean geometry was introduced. I already mentioned Bolyai and Lobatschewsky. By their work there came an end to the attempts (e.g. Legendre, Saccheri) to obtain the geometric property, which was originally expressed in the form of an axiom, as a theorem that should be proved with the aid of the other axioms. They reversed things and constructed a geometry in which this axiom is no longer included in the system of axioms on which this geometry is founded [20]. This is perhaps our way of looking towards this development and we may be sure that in those days it was not seen as a conversion of ideas.

In my view the idea of conversion cannot be explained as a "natural" consequence. Sudden ideas about connections may perhaps be at the base, but there is no necessity.

(ii). Let us now consider the rise of projective geometry from these points of view. It is a subject that goes back to Monge and Poncelet [21], perhaps even further (Desargues). It is developed in several directions: in geometrical form and in a more algebraic form under the influence of algebraization. Monge and Poncelet came to projective geometry in connection with their criticism of the usual analytic geometry with coordinates. Descartes's methods were too much algebraical and not really geometrical. It was in some way a methodological criticism. Does it concern a "natural" process?

There was some earlier criticism of this kind (Leibniz). I am inclined to see here no aspects of necessity. Such ideas, observed as discontinuities, may come from sudden good ideas, but also from a gradually growing personal insight that some methods and theories are unsatisfactory and should be improved. Much has been written on the history of projective geometry, but as far as I know the subject was never considered from the point of view of the present essay.

(iii). The introduction of Banach spaces is another interesting subject. In the first place it is very important in modern mathematics and in the second place its realization is more easily to trace than the preceding examples. Banach spaces were introduced in the late twenties and early thirties by Banach. Helly, nearly forgotten, also played a role [22]. Several lines in history can be drawn to Banach's axiomatic theory: integral equations, the problem of moments, the theory of systems of infinitely many linear equations with infinitely many unknowns, the "fonctions de lignes" of Volterra ([23]. For the history see [3]). Banach [24], and also some others as Mazur and Steinhaus (see papers in *Fundamenta Mathematica*), gave applications of his theory on classical problems of the theory of real functions, problems on convergence and divergence of Fourier series, etc. In classical form many of these problems were treated in books published in the Collection-Borel, which was rather famous in those years. In some of these books one finds the concept of space in the concrete form such as function spaces, or spaces of sequences, i.e. in non-axiomatic form. So, although perhaps less obvious, a line can be drawn through books of this series. Why did the authors take their examples from this area? Is it because of some French traditions in Poland? No reason can be found in any of these results and concrete theories to explain the step towards the *abstract axiomatic* theory, where the axiomatic concept of a norm is of first importance.

In *algebra* and in geometrical context there were earlier developments towards axiomatic theories. There is the work of Grassmann. In 1888 Peano gave a nearly exact definition of vectorspaces in algebra. But these works were for a long time forgotten and only in the twenties vectorspaces found definitely their place [25]. I already treated this subject before [3].

I see Banach's work from 1932 as a first step in a new area which hardly can be explained as a "natural" consequence of existing theories; it is a step that is not connected with a form of necessity, a step not finding its base in the subject itself, although in some way prepared by the past [26].

(iv). There is another interesting example in the classical theory of conformal mappings. This subject was initiated by Riemann. I have the impression that it has no more the great interest it had before. The development went another direction. There is a classical theorem of Liouville which states that the generalization of the theory of conformal mappings to $\mathbb{R}^3$ only leads to some trivial classes of mappings. Was Liouville's theorem the result of a systematic research for generalizations of the classical theory in $\mathbb{R}^2$ to higher dimensional spaces? Because of this result conformal mappings was a domain that was restricted to $\mathbb{R}^2$. However, in recent times a more general theory was developed. The main condition of conformal mappings was weakened and this led to the concept of quasi-conformal mappings. On this base a non-trivial theory was developed in $\mathbb{R}^n$, and this theory is still a subject of research in our time. One may suppose that the result of Liouville has had some influence on the creation of this new theory. However, I do not think this was a "natural" development, an obvious generalization, a theory that could be expected. In the way by which quasi-conformal mappings are defined there is no reason to think on a "natural" evolution. A sudden good idea was necessary to find the way to a new theory and this idea was in some way connected with the previous results but was not *determined* by them [27].

The origin of the theory of conformal mappings is a geometrical property of complex analytic functions (invariance of the angle between intersecting curves). There is still another theory which finds its origin in analytic functions and deserves to be mentioned in this framework.

It is the theory of quasi-analytic functions of real variables. These are classes of functions, defined by taking another property of the analytic functions as definition (the property that two functions are identical when they are equal on an arbitrary small domain). This is something like a conversion. It is most remarkable that the theory of these functions is connected with several other domains, domains which at first sight have not much in common: continued fractions, divergent series, the problem of moments, partial differential equations. How can such connections be explained in the light of free creation? What has been the way by which these connections were discovered? I think that just such relations demonstrate that good definitions have been taken as the point of departure. Whether such connections should be considered as "natural" is a highly subjective matter. In any case, we have here an interesting example of the fascinating activity of looking for lines that can be drawn between various subjects. Such lines reveal structures. This example deserves specialistic historical research [28]. Another subject one can discuss in this area is Weierstrass's idea of analytic continuation. Should it be seen as a discontinuity, a discontinuity of a more secondary type?

(v). Now we return to the axiom of choice mentioned at the beginning. Is this axiom a "natural product" of the evolution [2]? In my opinion acts of choice which can be traced in reasonings on subjects of classical analysis where - for instance in the years before Cantor - sets explicitly did not play a role, cannot be given as examples in favour of a statement that it concerns here a "natural" development. I think there was no awareness among mathematicians that it concerned here a method that had, or deserved, special attention. The examples concern concrete situations and it is not easy to see them as predecessors of an abstract statement. Abstraction, such as appears in Zermelo's formulation, was not a feature of classical analysis. That in the works of Bettazzi aspects can be found tending to the axiomatic form is another matter; this was in a period in which the theory of sets existed as a discipline. But then the line is rather short.

Furthermore, if we want to see here a "natural" process in the evolution, how shall we explain the fundamental discussions about the question whether Zermelo's statement should be accepted or should be rejected? If the axiom was "natural" there would have been no reason for discussion. I think there is an essential discontinuity in Zermelo's work, a discontinuity with important consequences.

III. Mathematics: Chains of Phases

The evolution of mathematics has led to an immense building of theories and special results, sometimes closely connected with each other, sometimes at a greater distance. As mentioned in the introduction the aim of this essay is to get a global insight in the line of the evolution, trying to avoid microscopy in order to come to a macroscopic view. One of the purposes of section II was to prepare such an attempt.

Now I consider again the discontinuities described in II.2. They have led me to an attempt to describe the great line of the evolution as a chain, or several chains, consisting of phases, a succession of phases of increasing level, each characterized by characteristic properties or special methods. Avoiding microscopy in this way one can get insight in the sense and the essential aspects of the evolution and the way by which the evolution developed. The roots of this idea of phases can be found in my previous publications [1a], [1b]. I considered the trend towards algebraization, in a later stage followed by collectivization. The combination of these two trends has led to the modern building. This is a way of classification with respect to methodological aspects. It is a very rude classification, doing no right to the results in the course of the evolution. Still more global is a classification with respect to the influences which played a role. There is the classical period as an external phase, created under strong influences of physics. The modern period can be seen as a mainly internal phase [1c]. Such classes are too global; they give no insight in the essence of the subjects which concern mathematicians. In defining phases new ideas as well as new methods should be taken as a guide.

In my view the following large phases can be discerned in such a model.

As the *first phase* I see the old mathematics, directed towards geometry, that is the mathematics such as existed in Antiquity and in our western culture before Descartes.

A *second phase* begins with the arithmetization of Descartes, a process that radically changed the picture and influenced all further developments.

A *third phase* begins with the introduction of the concepts of variable and function. Any further development was unthinkable without these concepts. In this phase the creation of infinitesimal calculus finds its place.

A *fourth phase* begins with the introduction of the general concept of sets of mathematical objects and abstract sets. Totally new areas were opened then.

As a *fifth phase* an axiomatic phase must perhaps be mentioned, but it is not easy to distinguish it from the fourth.

Now the arguments to support this idea of phases. Evidently the phases should not be considered as separated stages in the evolution, as if in a new phase the old phases were forgotten. Each of the phases has been of influence on the rise of the next one. Each phase is in some way *prepared* by the preceding ones. But, as will be clear from the preceding pages, in my opinion they are not historically determined by preceding phases. A new phase develops from a discontinuity in mathematical thinking, created by one, or sometimes more, mathematicians. The origins are essentially to be found in the "freedom of creation".

These five phases are only intended as a first, rather global, classification. A common aspect of any of these phases is that in the transformation from a phase to the next one new ideas, new concepts, are added to the preceding phase. Sometimes these ideas did not come from the existing theories; sometimes it concerned mathematical elements which did not yet find a useful place in the theory. These new ideas were then incorporated in the methodological apparatus. This led to new fields of problems, sometimes connected with existing problems, but also to entirely new domains. They were, in my opinion, not always historically determined. Methodological aspects were important in these transformations such as arithmetization, algebraization and collectivization. In any of these phases special characteristic elements can be recognized, which cannot be found in preceding phases.

Let me try to give these general remarks a somewhat more concrete form. In passing from the first to the second phase numbers are brought in connection with the geometrical concepts, curves, etc. They took the place of the usual geometrical reasonings. This is the way of arithmetization, the development of a new apparatus.

The transformation from the second into the third phase needs scarcely explanation. In the preceding phase mathematics was, globally said, of a "statical" type: problems concerned fixed numbers, fixed figures. In the third phase numerical variables and numerical functions were introduced, first in connection with a physical reality, in a later stage as functions in a more abstract form, an abstract mental idea, thus coming from outside the existing theory. The idea of change, as related to numbers, came into the picture and mathematics of a "dynamical" type started.

In the fourth phase the introduction of sets of undefined elements, abstract sets, is a characteristic feature. The idea is to consider different objects in some mutual connection as a unity on higher level and to perform operations with such unities. This is the way towards function spaces, abstract spaces in analysis and many other areas of modern mathematics. New types of problems were introduced, for instance a new type of existence theorems. The idea of collectivization as a methodological apparatus in a mathematical form was new, coming from outside existing mathematics. A special mental activity was necessary [1b], [1c].

In the fifth phase there is the idea of building theories starting from premises by the methods of deductive reasonings. I already discussed axiomatics, perhaps to be seen as a way of 2000 years, but in the time of modern mathematics, our century, I think it must be considered as a new idea, an idea that did not find a place in the preceding phases. Nevertheless there are close relations between the fourth and the fifth phase.

However, too much of the existing mathematics has been left out in these global classes; some more details are desirable in order to get an impression of what is going on and to do justice to mathematics. More structure is possible and I think this is possible with avoidance of microscopy. Within each of these periods more lines can be drawn, leading to more structure.

A line can be seen from early projective geometry (Poncelet, Monge) to the developments in modern mathematics with regards to this subject. Aspects of algebraization also find a place within this line.

There is a large line from the early axiomatic theory of Banach spaces (Banach 1932) - perhaps beginning earlier with the introduction of infinite dimensional spaces - to developments in more modern times: there are the locally convex spaces, where the geometrical notion of a convex set is added to the theory, and some more spaces in analysis, leading far into functional analysis. Furthermore I observe a line connecting algebra and topology: topological algebras, valued fields.

I also mention the developments in potential theory, leading from the classical theory to the axiomatic theory. (Compare also the remark in II.3, example (iv) on a line coming from analytical functions).

In this way one can bring more structure in the picture of the evolution of mathematics. Mathematics appears as a kind of tree. The idea to see this evolution as an ordered chain - or chains - of phases, main phases or more secondary ones, connected by "discontinuities", leads to some fundamental questions when we try to understand this evolution.

What can be said about the situation towards the end of a phase, a situation that evidently may have lasted several years? This is the *problem of the motives* by which mathematicians are - and were - led in their work. Was there among mathematicians involved some consciousness of coming near to new ideas and concepts because the development of existing mathematics asked for it, apparently needed it? Was there, perhaps, some feeling of discomfort? Was there a feeling that something should happen - for instance in connection with unsolved problems - when looking at mathematics as a domain of "problem-solving"? Are there in mathematical literature features of something like a crisis towards the end of a phase and, if so, were mathematicians aware of it?

Some concrete questions may illustrate this. Considering Cantor's work, was there before Cantor some outlook about things to come? I think this is not so likely because with Cantor totally new areas were opened, areas with problems and results that in no way could be foreseen. Perhaps there was a somewhat different situation in the third phase; the phase of developing analysis. I am inclined to think that often there may have been some view on the direction of developments, on coming results. In some way that period was "constructive", rather "technical" ([29]; see II. 2, (ii)). In general it was - and still is - not unusual to formulate conjectures. It concerned conjectures inside a theory, an internal affair.

Riemann's famous hypothesis about the zeros of the Z-function is a classical example. Cantor also formulated some conjectures, for instance on the continuum hypothesis (see [13], p. 11), but this too was inside the theory he had created. To some extent there is *prediction* inside theories [30]. Should this be considered as a strong form of "problem solving"?

Describing the evolution as chains of phases may lead to a picture of mathematics as a system of lines in some causal relation, a strict causal system. From the foregoing it will be clear that in my opinion this is not the right point of view. Partially there is something of causality and a kind of necessity in the evolution may even be frequent in developing theories, but the really great progress comes from what I called "discontinuities". They are produced by "free creation". I emphasize that the idea to consider the evolution of mathematics in the form of chains of increasing level has the character of a *theoretical model*, well to distinguish from the level of the mere facts, that is the *descriptive level.*

An analogous idea to consider a system of growing "levels of thinking" as a theoretical contribution to better understanding and insight in didactical processes, a theory of learning mathematics, in particular with respect to secondary education, is found in a theory developed by P.M. van Hiele in his thesis from 1957 [31]. The thesis of D. van Hiele-Geldof [32] is in close connection with Van Hiele's thesis. Van Hiele observes in the didactical process the existence of "levels of thinking" which led him to introduce a "level-theory" (niveau-theorie). The theory is concerned with a theoretical analysis of the facts such as they present themselves to us in the field of didactics. On a higher level there is better understanding as is necessary for getting more insight. The didactical process goes along a way of increasing levels. His theory is a model for the process of learning. In his theory there is an essential gap between the level of the mere facts, the *descriptive level*, and the theory of levels. His thesis is that the theory cannot be *derived* from the descriptive levels and that the difference between description and theory is essential.

I also mention the contribution of Van Hiele in a book containing a series of essays on aspects of teaching mathematics, written by various qualified teachers [33]. Reflecting about his doctoral thesis, he refers to the insights of modern philosophers and especially to the influence and ideas of G. Mannoury (1867-1956), professor of philosophy of mathematics at Amsterdam University. In the framework of the theory of discontinuities in the evolution, which I developed before, it is of interest to quote a passage from this essay. Under the head "Theorie is een persoonlijke schepping" [Theory is a personal creation] Van Hiele writes: "Theorie kan, zeggen ze, niet uit de feiten worden afgeleid. De theorie is een eigen schepping. Dat betekent dat je niet hoeft te proberen het theoretische te bewijzen, want dat is onmogelijk" [Theory cannot, they say, be derived from the facts. The theory is a creation for its own. This means that it has no sense trying to prove the theoretical, because this is impossible].

The transition of a level towards the next higher one can thus be considered as a "discontinuity" in thinking, such as I developed before. Van Hiele gives many arguments to support his theory; it is not well possible to give a summary. I only remark that it is curious to observe that there is a parallel between a theory on the evolution of mathematics and the didactical process of learning. Theoretically they follow to some extent the same way.

With regard to the question to derive a theory from the facts there is the same situation in both cases. The representation of the evolution of mathematics in the form of phases, such as I developed before on the base of the mathematical facts, has no form of necessity. I gave a first great line and I pointed to smaller and more detailed lines just to do more right to the representation in the form of a model. However, it is evidently possible that other, and perhaps better, models of the evolution can be developed: the facts do not lead in a form of necessity to the chain-model, because personal interpretations play a role. The description and the theory are of a different level.

A main problem thus is: *How do mathematicians come to their new ideas and results?* Here I see *creativity* as a most important feature. In the next section we will discuss therefore various aspects of creativity.

IV. Mathematical Creativity

IV. 1. Mathematics: objects and aims

The reflections in the preceding pages lead to the question of the nature of mathematics. The diversity of theories and objects might lead to the conclusion that an answer - valid for all time and for all that is going on - to the question what mathematics in essence is, shall be difficult to give, perhaps cannot be given, if we try to do right to this diversity. Saying that mathematics consists of a system of propositions with proofs, without any specification, seems too simplistic. *Mathematics grows and creativity is a main tool.* So there is reason to discuss creativity and its aspects in various forms.

In former times it was sometimes stated - and I observed it even recently - that mathematics is the science in which the properties of space and number are studied [34]. It is a statement that found its base in a realistic standpoint with regard to mathematics, mathematics in relation with "physical reality". It relates mathematics to external elements. For instance, if we suppose to have some picture of the real numbers as a straight line, this is an external interpretation. Real numbers, introduced as equivalence classes, as a completion of the field of rational numbers, are "abstract" things, whatever we may understand by "abstract". In fact they are no less abstract than, for instance, the p-adic numbers, which are also a completion of the rational numbers, but with respect to another metric (see p. 6). And for the p-adic numbers there is no such realistic picture. There are fundamental mathematical differences between these fields, but they have nothing to do with abstractness. That there are differences between them with regard to the possibility of application in physics, has probably topological reasons and is a matter of physics. With respect to "space" I mention the evolution of the idea of space from the classical "realistic" picture as a 3-dimensional, later n-dimensional, euclidean space $\mathbb{R}^n$ to the idea of what we nowadays call a space in a general sense, in which the concept of the neighborhood of a point takes a central place and which is now far away from "reality" [35]. The statement "space and number" must be seen against the background of mathematics in the classical period, a period which I mentioned in the foregoing pages. This was a phase with highly external influences, a phase in which mechanics, physics and mathematics formed a certain unity, an "external phase". For modern mathematics, in which the tendency towards abstraction is an essential aspect, such a statement is entirely out of date.

There is another historical aspect in the points of view with respect to the essence of mathematics. It is treated extensively in a paper from Bos [36]. First he mentions Proclus in Antiquity. For Proclus the way of mathematical reasoning, the method to obtain results, was of primary importance, not the results of the reasonings. Bos observes an analogous standpoint with regard to the essence of mathematics in the ideas of Wolff (1679-1754). For both, methods in mathematics are primary because they bring us nearer to certainty and truth ([36], p. 122).

Boutroux has a similar standpoint in his most interesting book *L'idéal scientifique des mathématiciens* (Paris, 1920), a classic with regard to reflections like these. In the chapter "Le point de vue de l'analyse moderne, I - L'évolution de l'analyse mathématique au XIXe siècle" he wrote: "Composer, à partir d'éléments simples, des assemblages de plus en plus complexes et bâtir ainsi de toutes pièces, par sa propre industrie, l'édifice de la science, telle apparaît désormais la tâche du mathématicien. La faculté créatrice du savant se trouve à tel point exaltée, dans cette période nouvelle, que, de moyen qu'elle était, elle se transforme bientôt en but. Laissant aux praticiens le soin d'interpréter et d'utiliser ses théories, le mathématicien de l'école algébriste attache moins de prix aux théories construites et aux résultats acquis qu'à la méthode par laquelle il y parvient. Son but principal n'est pas de connaître des faits nouveaux, mais d'accroître sa puissance créatrice et ses ressources de constructeur en perfectionnant de plus en plus ses procédés" (p. 182).

This passage should be placed in the context of the underlying philosophy of that book. Boutroux presents the evolution of mathematics as a stream of mathematical thoughts and ideas, somewhat in phases, where an algebraic period precedes the period of analysis. The preceding chapter concerns "L'apogée et le déclin de la conception synthétiste", where one finds the section "Les limites de l'algèbre". In the pages which follow this quotation the author describes the reaction against this idea of "methods" as an essential aspect: "Il est bien évident tout d'abord que le mathématicien ne saurait construire dans le vide. Il importe que ses théories soient applicables à la géométrie et la physique. Or, les besoins de ses sciences obligent le savant à étudier des relations mathématiques qui ne se réduisent pas à des combinaisons algébriques".

Has this old idea to see "methods" as the essence something to do with the formal structure of algebra?

It is interesting to compare this with the situations in modern mathematics. On the way towards abstraction - an important feature of modern mathematics - methods were important: algebraization - to some extent classical -, collectivization, axiomatization. In some way they were at the origins of the modern developments. In this context one should mention the rise of Bourbakism where the structural aspects of mathematics and the axiomatic form as method are important, perhaps primary. However there seems to be some decline in this point of view [36].

Do all such standpoints lead to the conclusion that the methodological apparatus in connection with "doing" forms the essence of mathematics? Should mathematics fundamentally be considered as a "way of thinking"? I think such a conclusion does no right to the amount of theories and results in mathematics of all times: *mathematics is more.* However, the problem of the nature of mathematics remains.

It is remarkable that in discussions on fundamental concepts and ideas - for instance on the essence of mathematics in relation to mathematical methods - often arguments pro as well as contra with respect to individual points of view can be given. This leads to uncertainty. When we conclude that mathematics is "more" without specification what means "more", we can point to some other aspects.

There is the place and function of what is called "applied mathematics". I have in view here mathematics which is directed towards applications in other disciplines, like physics, mechanics, etc. Considering the phenomenon of such applications from a fundamental point of view - that is the *possibility,* disregarding the factual situations - one may ask what is of primary importance: the mathematical results for themselves or the applicability, the methodological apparatus which appears to be available. What is more important in the applied area: mathematical methods or specific results? What is primary in these circumstances of the evolution: theorems and results or the fact that some methods are recognized as being useful for a systematic apparatus? Perhaps the answer is that it is not well possible to separate these aspects in a strict way. Only by way of illustration of the idea, and not without much hesitation - being well aware of the fact that I am leaving my usual domain and raise a question that surely has been analyzed extensively - I wonder: when Einstein developed his general relativity theory, the mathematical apparatus appeared to be available, but what was the essential thing for him: the suitable apparatus or the specific results themselves?

Such applications have to do with mathematics in relation to *external* elements. In some way it concerns here the reverse of the classical development of the introduction of infinitesimal calculus, where preceding mechanical and physical backgrounds played a role. Here I point to the reverse: from mathematical methods to concrete external facts.

But, discussing the place of mathematical methods, I also want to consider some aspects of mathematics of an *internal* character.

A traditional aspect of mathematics is "to do mathematics", to cultivate, to practice mathematics. This has to do with technical capability, the practice of mathematical methods. For mathematicians who cultivate the methods this way, the mathematical apparatus is of fundamental importance and perhaps more in the foreground than results. So mathematics can be approached from at least two sides.

Methods come in the foreground when we concern the way by which we transfer mathematical knowledge. Transfer is related to "doing" mathematics; we have to show how to treat mathematical problems and problems of mathematical kind which present themselves in life. It can be a way towards new developments and applications. We have to cultivate a mathematical attitude. But how to stimulate this? Methods are of special importance here. Already at secondary level this methodological aspect is characteristic. The exercises are not important for themselves; what is important is the way to solve them. I agree that this side of doing mathematics is mainly educational: on this secondary level it concerns teaching and learning mathematics, and it has to do with "doing". But on a higher level it is also a more intrinsic aspect of mathematics: *mathematics as a game.* It has some relation to "problem-solving" at a high level and is to some extent in the neighborhood of "methods" as essence of mathematics. At high level we find this aspect in some journals in the section "Problems". These problems are exercises of high level. The composer knows the solution and it is the task for the readers to find a solution. I think for them it is not of first importance to know the result, but the satisfaction to be able to find a solution may be more in the foreground: it is mathematics as a game. Here one needs *inventivity;* it is an activity of methodological character. There may be ways of solving that differ essentially from those which were initiated originally. It is even possible that by some chance new and rather unexpected relations with other areas are found. In this respect there is some, although perhaps weak, connection with aspects of creatitvity. This "problem-solving" is part of our Mathematical Culture. It is indeed a very classical activity among mathematicians to pose problems to each other as a method of stimulation. Already in the time of Leibniz this was customary [37]. In the next section some more remarks on "problem-solving" and the aspect of invention in mathematics.

In the preceding pages I discussed the problem of the essence of mathematics in relation to the methodological apparatus in mathematics. Now I want to make some more general remarks on the nature of mathematics. This is a domain of the philosophers among the mathematicians, which is not very familiar for me; for more profound reflections I must therefore refer to literature [38].

Firstly there is the question of the objects of mathematics. This problem presents itself in particular when classical mathematics (the mathematics of the third phase) is compared with modern mathematics. Classical mathematics was in some way related to a *physical reality* and the objects had more or less connections with concepts of the physical world (there is perhaps a somewhat different situation with respect to concepts of algebra). From a modern point of view one can be inclined to say that there were already abstract concepts in that period, for instance the real numbers (as I discussed before), or the general concept of a function. But did the mathematicians of that period themselves look at such objects as in some way abstract things? In any case, in this respect the classical phase can scarcely be compared with the modern period with its abundance of abstract things: rings, fields, varieties, lattices, etc. What in essence are the objects of these areas of modern mathematics? I think we must say that in many respects mathematics as it is nowadays, has broken with ideas of a certain "physical reality". The old objects with their orientation towards "physical reality" are still present - one can easily indicate such areas - but next to them new objects were created which are abstractions from concepts of preceding phases. They were created by means of "free" human mental acitivity. The distance between the old objects and these abstractions may be large, but I think that in most cases in some way a road backwards can be traced. A careful analysis of the origins of fundamental objects would be interesting. I think we can say that in modern mathematics "physical reality" is replaced by *"abstract reality"*. This idea deserves further attention.

Next there is the question of the aims of the practising mathematicians. What are the general purposes in mathematics? Of course I have in view here the really general purposes. Evidently there are purposes in special domains, for example the problem of classification of finite groups. Then there still are - just as in the classical period - external purposes. But this is not what I mean. I want to compare the new situation again with the circumstances in the classical period. The abundance of abstract theories we have nowadays, may perhaps encourage the impression that after all the situation was more simple in the classical period; at least this might be a feeling of the older generations. One might think that it is nowadays more difficult to discover the way. And indeed, the domain of mathematics nowadays is much more extensive than before. But is this a reason for seeing a fundamental difference between the actual situation and earlier phases? If we might have the impression that formerly there was more order in the development, could this not be because we look backward at the older periods from our higher level, a point of view from which we have in some way an overall picture of the past? One would like to know how the mathematicians of the earlier periods looked at the developments of their time. Did they have some idea of an unlimited growth, such as some have with respect to modern developments?

In former phases the mathematicians explored by means of the methods of their time - geometrical, algebraical and analytical methods - the consequences of the concepts they had introduced. They explored an area with a certain "physical reality", sometimes near to the original concepts, sometimes at greater distance. Perhaps there was a special place for algebra.

This is not different in our time. The mathematicians explore by means of the adequate methods of our time - the analogical method, algebraization, collectivization, axiomatization, etc. - the new concepts they have introduced, concepts that are often of an abstract character. It is a domain with a certain *abstract reality.*

Previously I published a paper entitled *Where does the development of mathematics lead to?* [1d]. This question is connected with the problem posed before: "What is mathematics?" After some more reflection I am now inclined to say that this question does not make sense, because what we call mathematics exists in fact for two thousand years or even more as a human activity, presenting itself in various forms - often connected with each other, changing in the course of time -, that pass away but sometimes return in new forms. It has no much sense to ask why all this exists and whether or not there is a final goal. There is perhaps even no answer to the question what mathematics *is.* I am more inclined now to compare the existence of mathematics with the existence of other Human Activities such as the Art of Painting, Literature, Poetry, etc. I wonder whether there are analogies between the ways of pure mathematics and the Art of Painting. Here I think of the tendency from realism (classical mathematics) towards abstraction. I refer for instance to the nearly mathematical-geometrical work of the Dutch painter Mondriaan. Can we compare this with developments in Art? (I already made some remarks on "physical reality" and "abstract reality"). Although it is not my domain I venture to ask: has there ever been given a formal definition or a characterisation of the Art of Painting, embracing all the tendencies in this Art? Should we say that this Art consists of "games with colours", just as Poetry as "games with words", and then mathematics as "games with symbols"? And what about music? Nevertheless I am afraid these characterisations do not contribute to more insight into the nature of these subjects.

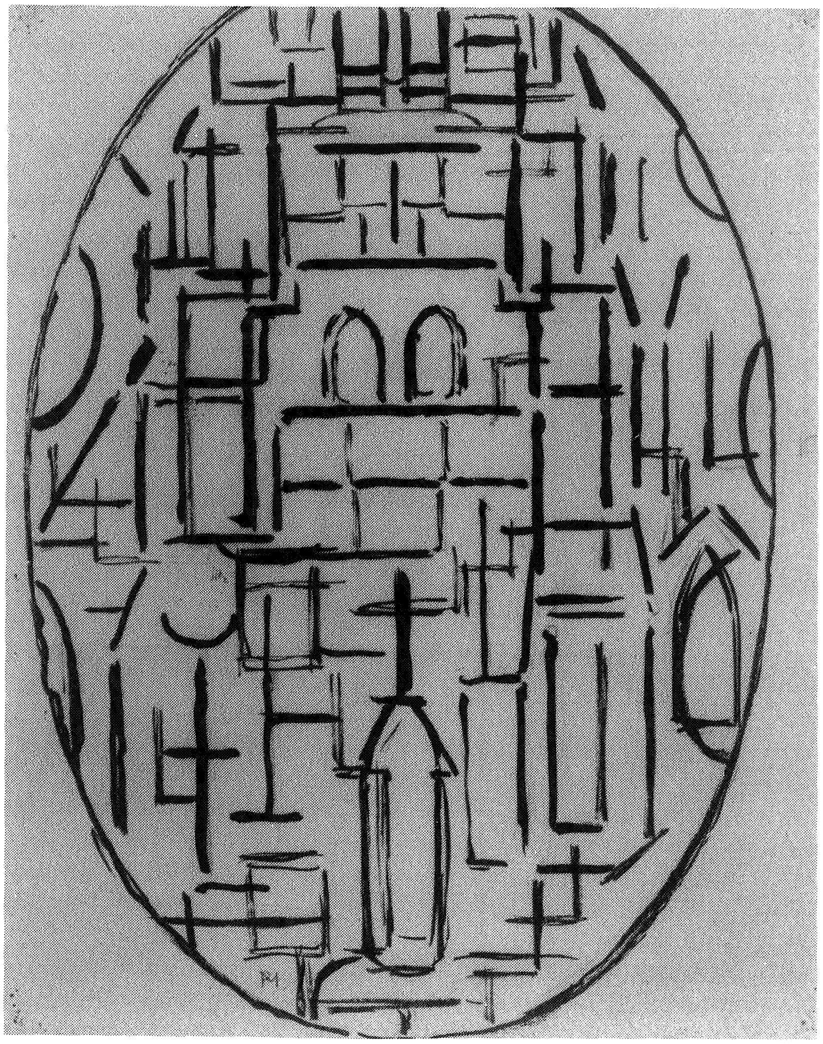
Perhaps there is another way to come nearer to the essence of mathematics. We can try to give an analysis of the developments of the methods, the ways of thinking. Progress is in close connection with creativity. In the next section we shall discuss the various aspects of creativity.

IV.2. Creativity and ways of thinking

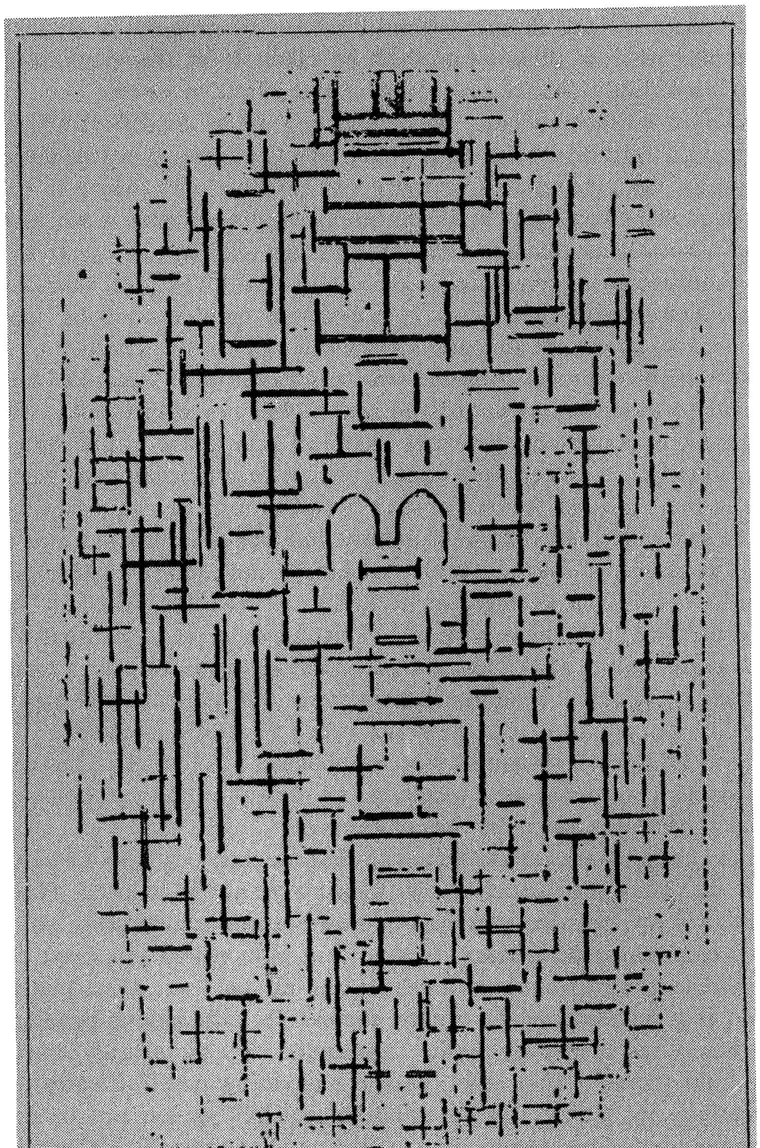
In direct attempts to come nearer to the essence, the nature, of mathematics, as I tried before, one meets serious difficulties. Problems arise also when one tries to follow the way of analyzing the methodology, the methods of progress and creativity, the ways of generating mathematics. One should think on apparatus such as imitation, analogy, generalization, discovery and invention, "doing", and in particular creativity and creation. What is creativity in mathematics and do all mathematicians mean the same when speaking about it? I think this is rather questionable. The best one can do is to analyze such concepts in their mutual relations and in particular to give examples of developments.

In his recent book *Wat is creativiteit?* (1987) S. Dresden is concerned with problems that arise in an analysis of creativity. Although science is not entirely excluded - there are references to H. Poincaré, J. Hadamard, M. Planck - the author mainly deals with the domain of the Arts: Painting, Literature, Poetry. The examples used as illustrations are mainly taken from these domains. However, the problematic is placed in a broad and general framework and so the mathematician shall read this very interesting and beautiful book with in his mind ideas about the progress of mathematics. Reading this book I was surprised to find several parallels with ideas and aspects one meets when studying the problematic of the progress of mathematics. I mention analogy, imitation, "problem-solving" they all find a place under the head of creativity. But is there a reason to be surprised? In both cases it concerns *general* structural insights with respect to progress in relation to creativity. Are the really *fundamental* elements of progress in mathematics different from those in other disciplines? Certainly, there are elements of mathematical thinking that cannot be replaced in other domains: for instance algebraization and collectivization, which seem to be strictly reserved for mathematics, although even these aspects can be seen in the light of ways of thinking. One then comes in the domain of

Piet Mondriaan, Church at Domburg, pendrawing, 1909, Collection Haags Gemeentemuseum.



Piet Mondriaan, Church at Domburg, Indian ink, 1914, Collection Haags Gemeentemuseum.



Piet Mondriaan, Church at Domburg, charcoal drawing, 1915. Photo: Rijksbureau voor Kunsthistorische Documentatie, Den Haag.

reflections about the origins of mathematical concepts and ideas to which we return later on.

In this section I shall discuss several elements of progress in mathematics. The examples used as illustration shall be taken from this domain. It may finally be interesting to make a comparison with Dresden's work.

I think most mathematicians, when reflecting about creativity, will have in mind the great historical creations, which brought forward mathematics. But this vision is too restricted. Creativity is also necessary when it concerns "doing" or perhaps "making" mathematics. Nevertheless there is not much risk in saying that mathematicians consider this last element of creativity as of another, more technical character, as of a somewhat lower level than the great creations which initiated new ways. It is closer to "problem-solving" and connected with *inventivity.* Real creation has aspects of another character.

"Doing" mathematics is in some way associated with technical capacity, which leads to more or less occasional results, but not with fundamental new developments, although for the latter technical knowledge is unavoidable. In my view creativity is placed in a contraposition to practising mathematics, to "doing". Is there a good reason for such a distinction? Letting aside that it is difficult to give objective criteria for quality and value, I think it concerns here opposite positions. "Doing" and "making" are more connected with mathematics as a "knowledge", as technics, a knowledge that is "certain" (compare the Dutch term "wiskunde"). This comes close to of what Dresden describes as the Latin "ars", "technè" in Greek. He gives a detailed analysis of these concepts, which I shall not repeat here. His observations are equally applicable to mathematics, a curious fact which I already mentioned. "Ars" is according to Dresden a system of rules established for "doing". Opposite to it is mathematics considered as a discipline of creativity and creations, a discipline that is steadily growing by acts of creativity.

Creativity is generally associated with introducing new theories, concepts and ideas that did not exist before, or at least were not introduced. This standpoint differs from the idea that non-mathematicians sometimes - not to say mostly - have about mathematics. It is not uncommon that they have the idea that mathematics is in some way finished, that all has been done, that there is no "new", that mathematicians occupy themselves with solving problems that present themselves in life or in the framework of what already exists, with studying "old things". They ask: "Is there anything new?" Clearly, there is some creativity in "doing", but I think "creativity" should not be confused with "inventivity". Can we say that mathematics is "made"? There is some difference between "doing" and "making". The first has to do with things that already exist, the second concerns that what not yet exists - in whatever sense - and should be made. But the difference is rather subtle and I refer again to Dresden for a detailed analysis with examples, of course taken from his field of interest. For the moment I only remark here that this leads to the question whether there is *freedom* in "making", in creativity. Some more remarks on this point will be made later on. Here I only observe that this leads to the following problem:

What are the origins of new areas and new ways? Where do they find their source?

These are questions related with the idea of "discontinuities", introduced before. It is no longer the question here what mathematics *is*, the problem of the *nature* of mathematics (which I let aside), but the question *how* general progress is realized. This is a subject which belongs perhaps more to the domain of psychology than to mathematics. It concerns an analysis of the ways of thinking, an element of creativity. It can be placed in the framework of the classical idea of "creatio ex nihilo", creation as an issue coming from the "Nothing" - whatever this may be -, creation finding its origins in the "Nothing". Dresden writes about this and refers to classical philosophy, to theology, and also to modern philosophical ideas. It is perhaps, from our point of view, a way of expressing that we do not know the real sources, that there is no strict causality. Later on some more remarks and examples.

Considering fundamental new areas and new concepts in relation to creativity, there are two other aspects which deserve attention. They have also to do with the concept of "new". They are (i) the process of "discovering", and (ii) the process of "invention".

A. METHODOLOGICAL ASPECTS

The elements of progress, mentioned above, should carefully be distinguished, with special emphasis on aspects of creativity. *Discovering,* such as I use this term here, has to do with establishing and presenting results which in fact already "exist" in the framework of a theory. They were not yet explicitly known or formulated and should therefore be "discovered" by practising mathematics. Perhaps this activity contains elements of creativity, but I think it is more based on a profound knowledge of the areas which it concerns. High intellectual capacity and inventivity are needed, and a feeling for perhaps rather unexpected relations. A form of creativity is needed, but not in the sense described before, where I introduced "creation" as the mental activity to do decisive steps opening the door into a fundamentally new domain. "Discovering" is based on some sense of "reality". Should the introduction of non-euclidean geometry by Bolyai and Lobatschewsky be considered as a "discovery"? I do not think so (compare p. 13). Let us consider another example. When Stieltjes established the remarkable relation between the problem of moments - a problem that originally had its sources in mechanics - and the theory of continued fractions, this can scarcely be considered as a fact of creation in the last sense. This is a result from a process of high inventive discovering: the connection "existed" as a matter of fact but should be discovered. Evidently it was a work of very high quality. Later on there appeared to be connections with the theory of quasi-analytic functions: again a process of discovering. It is easy to give more examples. For instance, the formulas for elliptic functions "existed" before they were derived, "discovered". This is connected with technical capability, with "mathematical knowledge". Should this be considered as "creation"? The introduction of the field of p-adic numbers is also a matter of discovering. I think this must be seen as the development of a theory, "making" mathematics. It is connected with inventivity, the capacity of "doing".

This brings me to consider the concept of "invention" as element of progress in mathematics. Is there "invention" in mathematics in a strict sense? What is the position of "invention" with respect to creation? If we want to give invention a place, I think it can be considered in a special sense as an act of "creativity". It can be seen as creativity with respect to certain more or less well defined goals and directed by ideas about them, stimulated by other developments, maybe in different areas. In this way "invention" can be considered as "directed-creation", creation directed towards goals. I think for example of the introduction of infinitesimal calculus (Leibniz, Newton) in relation to problems in mechanics and physics. Should we speak of "invention"? It does concern creativity, but is this "free-creativity"? Anyhow, I think it is difficult to see this development as a form of "creatio ex nihilo". The way was prepared by preceding theories. And, to consider a development in recent time: how do we consider the recent proof of Bieberbach's conjecture on univalent functions? Is it invention, discovery or creation? I think the formulation of the conjecture itself is an act of creativity. The proof is invention. But is it invention by hazard or directed?

Here appears the problem of the motives for certain developments. What can in general be said about *motives* for mathematicians to go certain ways? There may be external and internal motives; how is their relation to creativity? The motives for creations in modern developments seem mostly to be found in itself. I refer to my idea to see the modern period as an "internal phase", in opposition to the earlier "external phase". In this direction several questions can be posed. For instance, should Cantor's work be considered as a product of "creatio ex nihilo"? Or is creation in some way connected here even with chance? I will leave this last question open here.

Let us now consider some more special methodological aspects in their relation to creativity. They concern the methods of *analogy, imitation* and *generalization.*

Are there any aspects of creativity in the very common method of analogy? An answer depends on the standpoints with respect to "creation". If we connect creativity with strict originality of concepts, the answer should perhaps be negative. These three methods apply to what already in some way exists. They are not concerned with creatio ex nihilo; there are relations with the problematic of "existence".

Dresden introduced the concept of "imitation", which is not much used in mathematics. One can design imitation as nearly "the same" with respect to other objects, in some way similarity. As an example I mention the transformation of analytic geometry in $\mathbb{R}^2$ into analytic geometry in $\mathbb{R}^3$. It is a rather evident copy. So there are scarcely elements of creativity in it.

With respect to analogy in a general sense the situation has deeper facets. There are many examples tending towards a deeper insight. I point to the effects of algebraization and collectivization where analogical methods are used to unify separated areas and in fact "new" domains are created. One can think of the introduction of Lie groups and Lie algebras in modern form detached from differential equations. This is perhaps not to be seen as creatio ex nihilo, but it demonstrates the *power of analogy*. This leads to the following question: *Is the analogical method creative and what is the power of mathematical analogy?* I think this power must be found in the *Idea* which opens the door to the possibility of using analogical methods. Ideas may perhaps find some sources in imitation and similarity, as for instance with respect to the way from analytic geometry in $\mathbb{R}^n$ to Hilbert space. Such ideas may help to bring separated areas into contact with each other. In my view this is an aspect of creativity and more than analogy itself a constructive *method* of developing. Considered in this way analogy can lead to new areas. Sometimes such new areas shall be considered as *generalizations*.

It is now the place to make some general remarks about *processes of generalization*. Such processes are very common in the progress of mathematics. Every mathematician knows its various aspects: proving theorems under more general conditions than originally were posed; generalization of concepts, sometimes bringing them together in a more general theory; generalization of theories leading to abstract concepts and theories; etc. In a general way one can say that it concerns the realization of ever broader and more embracing theories. The question then rises if there are connections with "creativity". I think the creative aspect of processes of generalization must be found in the fact that ways are indicated which open the possibility of generalization in certain situations. Just as with the methods of analogy it is the *Idea* that is creative, more than the process itself. But does "generalization" lead to essentially and fundamentally new things? Anyhow it is no creatio ex nihilo.

There is then a reason for some critical notes. An intriguing question is if there is unlimited generalization, and, in case there should be no final goal, what are the consequences of such processes. Unlimited tendency towards generalizing theories has some features of the tendency towards *perfection* - a term used by Dresden - in the realization of ever more general frameworks. A general theory can surpass itself with respect to the results that are comprised. Paradoxically, "absolute generality, "perfection", may lead to theories which to some extent are sterile: theories in which no "specific properties" can be proved in their framework; there are no "special cases". With "specific" I mean something like: "under the conditions (restrictions) X a property P is valid for a certain class A of objects of the theory". In some way they can be called "exceptional cases" in the general situation. In *absolute,* ultimate, generality they find no place. In such a situation "invention", "discovering" and "creativity" have no longer a function and as a consequence the concern of mathematics, the essence, would change fundamentally: *progress is fundamental for mathematics.*

Perhaps "doing mathematics" would be the aspect that remains. To illustrate: if a certain property P is valid for *all* curves, whatever they may be, this may be interesting, but the subject of research is cut down. Such theories, if they anyhow should exist, are too "general", too "perfect", and seem to have no real function in the building. The utmost generality has no function. The paradox then is that, in order to get specific properties, restrictions on the theory will be necessary. This can be expressed in a paradoxical way: "if we want to have "all", we shall have "nothing". Such generality may thus lead to introduce other concepts such as conditions on effectivity or constructivity.

Dresden points to an analogical situation in the Art of painting: a perfect painting is in some way sterile, dead; it is too beautiful.

Let me give some other examples to illustrate this paradox. As a first case some consequences of the theorem of Hahn-Banach in functional analysis can be mentioned. By means of this theorem one proves the following results:

1. To *any* bounded real-valued function f, defined on an interval I, a real number, designed by $\int f$, can be associated verifying the usual properties of the ordinary Riemann-integral (finite addivity, etc.). This number is called the Banach-integral.
2. To any bounded sequence (a_n) of real numbers a real number can be associated, the Banach-limit, Lim a_n, such that the ordinary conditions are verified (additivity, etc.).

In this way *any* bounded function is integrable and the concept of "integrability" has no longer sense. We have a similar situation with regard to "summability" of series. Can we "do" anything with these concepts? They are too general, too "perfect". If we want to "do" mathematics with them, we have, paradoxically, to make restrictions on the "integral" or the "sum", returning to more "constructive" classical concepts (Riemann, Lebesgue). "Integrability" and "summability" exist as useful concepts just because of their restricted sense, their "exceptional" position; just this opens the possibility of *specific* properties.

The situation is even more paradoxical. The proof of the theorem of Hahn-Banach is based on Zorn's lemma (or the axiom of choice), and therefore also these results on integration and summation. To some extent these results can be called "empty": we cannot practice mathematics with them. They demonstrate the power of the method. In some sense the attention is shifted from the result towards the apparatus, the method. In cases of extreme generalization - and perhaps these cases can be seen as extreme - the *method* is dominating; in some way it has become the essence, the way to show its power. Is this "generalization" as a goal for itself, something like a process "from goal to method" such as L.E.J. Brouwer introduced in his thesis *Over de grondslagen der wiskunde* (1907)? See in Mannoury, *Handboek der analytische Signifika, II,* 96 (see p. 22). Here I point to my remarks on historical standpoints with regard to the essence of mathematics. The paradox here is that this same theorem of Hahn-Banach is applicated in many other branches, for instance in the rather "constructive" area of partial differential equations, despite its non-constructive base. One may ask why some applications are useful and others not (unless to show the power of a method). This demonstrates the limitations of the method of "generalization" in the process of the growth of mathematics.

I mention another example. The tendency towards abstraction is one of the features of modern mathematics. It may be seen as an attempt to reduce theories to their very essence, to most general theories that embrace concrete theories and transform them into abstract theories. In extreme situations there may be some danger in doing so: the way of abstraction can result in theories that are sterile in the sense that specific properties no longer can be proved and find no applications in other areas. The theory has become too general, perhaps too abstract to "do" mathematics. Paradoxically such developments can lead to the introduction of more and more detailed refinements of concepts and definitions, with no other goal than to reach the utmost generality. An example is the work of Maurice Fréchet presented in his book *Les espaces abstraits* (1928) in the area of topological spaces. This book is full of new and very detailed concepts and definitions, but contains little or no specific properties connected with these concepts. Should this work be considered as a "super-generalization", no longer allowing properties in these detailed situation? Fréchet announced the idea to write a second volume, treating general analysis in connection with these general abstract spaces. But it never appeared. Was this general theory after all not well possible? To come to a succesful theory one must paradoxically return to less detailed and less general concepts, to more traditional ones, for instance metric spaces or more usual, less exotic, topological spaces. Here is the same situation as we observed with respect to the Banach-integral. In this context two monographs of Appert must be mentioned: *Propriétés des espaces abstraits les plus généraux,* 1, 2 (1934), composed under the influence of Fréchet. Has there ever been a useful continuation of the ideas developed in these books?

Just as in section III with respect to the presentation of the evolution of mathematics in its totality in the form of chains of phases, the way towards *abstraction* can be presented in the form of a series of levels. The concept of abstractness is in close connection with the problematic of the *specific identity* of the objects with which mathematics is concerned. In processes of abstraction one makes abstraction from some adequate aspects of the specific identity of objects. It is a traditional method. The idea of a derivative of a function f is introduced in this way. One defines

$$f'(x)=\lim_{h\to 0}\frac{f(x+h)-f(x)}{h}$$

firstly without any further conditions and specification of f; questions on the existence come later on. No specific identification of f is given. In some way it is an abstract method and perhaps such methods make mathematics difficult for non-mathematicians. There is an analogous situation when studying curves in geometry respectively algebra by the process of algebraization. Abstraction is made from the pure geometric concepts and the evolution turned to study chapters of algebra without specific identity of the elements and then giving geometric interpretations. There is the same situation when placing classical versus modern algebra, for instance the formal theory of groups. In this way modern algebra can be seen under the head of processes of algebraization.

Let us now consider the general process of abstraction as a series of levels. Non-mathematicians will be inclined to say that all real mathematics is "abstract", just because of statements which often begin with "let f be ..." or "let x be ...", these objects to be taken from certain sets. The abstractness is then in the indetermination of f or x. But this sense of abstractness differs from what mathematicians understand by the term "abstract". Just for this reason it is desirable to specify the mathematical concept of abstractness and this can be done by considering the mathematical way towards abstractness. Therefore I introduce a series of corresponding levels, which begins with the concrete level.

1. Traditional classical analysis in its first stages. Investigations on concrete objects: solutions of concrete differential equations, computations of integrals, maxima, minima, etc.
2. Studies of objects that are defined in an implicit way by means of conditions which they satisfy but whose concrete specific identity is not known. Their existence is considered as evident, for instance as a consequence of physical arguments. In a later phase formal existence theorems were introduced. As an exmplae I mention the implicit study of differential equations, where concrete solutions are unknown.
3. Studies of theories whose objects are unknown; their existence is axiomatically posed. They even have no specific identity and in constructing a theory one does not need their identity. It concerns a different kind of "existence", in some way perhaps comparable with science fiction. Compare for instance the axiomatic geometry of Hilbert I mentioned before. It is the abstract phase of axiomatic theories. See for instance the axiomatic potential theory in comparison to classical potential theory.
4. Formal studies about objects which in principle have no specific identity. For abstract groups and rings; algebras, etc. Studies which are concerned with structures. It is no problem to give examples to illustrate.

For non-mathematicians there may even be some "mystery" in the fact that mathematical operations are possible - even fundamental for modern mathematics - working on such "unknown" objects. Operations like $\oplus$ or $\otimes$, the direct sum respectively the tensor product, are mysterious for laymen. The definition of the derivative for the non-specific symbol f, which I gave before, is perhaps already strange for them, abstract as it is. Properties with proofs and theories are based on such operators. What is the sense of operators working on "unknown" objects, on entities whose specific identity is unknown, or even cannot be given in a concrete way for reasons of principle, as a consequence of special methods of proving (Zorn's lemma)? What is their creative power? How to explain this? Mathematicians will answer that such operations are based on specified rules and that it concerns formalisms. What means mathematical "existence" for objects without specific identity? I pose in opposition to each other the traditional classical constructive-existence, and the pure-existence *as we use in mathematics* of our times. They are essentially of a different type, belonging to different levels with respect to their philosophical backgrounds. In previous publications I designed them by strong respectively weak existence [1f]. Banach's *Théorie des opérations linéaires* - a book I already knew in my first years - is still fascinating reading in this context. (Compare this with the theory on discontinuities in the evolution I developed before. See also some passages in the next section "Ways of thinking" (p. 43)).

This is the place to mention *Mathesis en Mystiek, een signifiese studie van kommunisties standpunt* by the philosopher Mannoury (Amsterdam 1924; second edition with an introduction by J.Ch. Boland (Utrecht 1978); for Mannoury see the remarks in section III). In this book he gave his vision on the "mystery" of mathematics. He developed his curious and original ideas with regard to the philosophical foundation of mathematics in the context of general philosophical points of view. He gave reasonings in a polemical or paradoxical, sometimes even enigmatical form. Just some examples from the chapter "Geloven en Zijn" [To believe and to be]: "Is er iets dat waarlijk is?" [Is there something that really is?]; "... en daarom voert de mathesis tot mystiek en de mystiek tot mathesis..." [... and therefore mathematics leads to mystery and mystery to mathematics...].

Such contemplations brought Mannoury to the development of a new discipline of philosophical kind, which he called Signifika. It is not easy to express in a few lines what is meant, but let me try. Signifika is in a general sense concerned with a critical analysis of the means of communication and their relation to the things, concepts, ideas, theories that are introduced. These "means" may be linguistic acts but Mannoury considered them in a broader sense, designing them as "taaldaden" (= acts in language). One should take into account that such expressions are not always clear and objective. I mention, for instance, the various meanings given to the mathematical notion of "existence" (see before). It is a science in which concepts are important (sets, functions, convergence, existence, etc.). I refer to the introduction by Boland in the second edition of 1978. It would take too much place to give details and I can only give some catchwords in this discipline: means of communication and their objectives, problems on exact formulation, symbols, the problem of causality in relation to lines of development, free will, questions on interpretation (see I.2), intuition, forms of negation and conjunction, equality and identity, formalization. Furthermore see the following books by Mannoury:

1. *Mathesis en Mystiek,* already mentioned before.
2. *Les fondements psycho-linguistiques.* Bussum 1947.

3a. *Handboek der analytische Signifika, I. Geschiedenis en Begripskritiek.* Bussum 1947.

3b. *Handboek der analytische Signifika, II. Hoofdbegrippen en Methoden der Signifika, Ontogenese en fylogenese van het verstandhoudingsapparaat.* Bussum 1948.

4. *Signifische Dialogen,* by L.E.J. Brouwer, Fred. van Eeden, Jac. van Ginneken S.J., G. Mannoury. Utrecht 1939.

The present essay deals with lines of development in connection with "discontinuities", aspects of creativity, freedom of creativity and further elements of the mathematical evolution. These problems are to some extent also treated in Signifika and therefore the ideas developed in the present essay can find a place in it. One should take into account that the elements of the mathematical evolution, on which my reflections are based, were already present in the fifties. My essay could have been written in those years but, as far as I know, no one has written something alike. One can add that since those years there appeared to be less interest in this field. For the reasons of the decline of this interest see Appendix no. 8.

With these remarks on Signifika and general philosophy I perhaps deviated too far from my proper subject. Let me therefore return to the more limited area of mathematics.

In the framework of abstractness there is another idea which is customary among mathematicians and therefore should find a place here. I mean the fact that some theories or notions are said to be *pathologic.* They are called pathologic when they seem to be against all common sense, in contrast to natural insight, abstract to such a degree that it is doubtful whether they are of any use. I mention for instance the curve of Peano and the so-called paradoxical decompositions of geometric figures. There is the theorem that a ball can decomposed in two disjoint parts such that each of these parts is congruent to the given ball, i.e. "making two balls from one ball". Such decompositions, however, play a role in measure theory (the paradox of Hausdorff). But letting aside the term "pathologic", which was introduced in a more modern period, the idea is much older. Such notions must then be considered in the historical context. There is the example of functions whose definition is in adequate way given by more than one analytic expression, formerly considered as not fashionable or even not allowed and not being a "real function". Continuous functions without a derivative were in the first years considered as abnormal; nevertheless they had to be accepted, in contrast to natural insight. Furthermore there are the examples of functions whose Fourier series is everywhere divergent. In those years this was considered as "pathologic", although one did not use this denomination. The line goes into modern times. I mention for instance Köthe, who calls pathologic any topological linear space whose dual space (i.e. the space of continuous linear functionals) only consists of the element *0* ([39], p. 160).

All such results are the product of creative thinking. One may ask what is the sense of such creations. I think they have sense in the line of the historical development. Such things should be seen as stations on the way of the evolution. It is an area that can be studied under the head "Creativity and the problematic of the identity of mathematical objects".

It seems that all these methods have some restricted meaning when it concerns the question whether or not results obtained by them deserve a definite place in the mathematical building, although it is difficult to formulate concrete restrictions. Applications of these methods may sometimes have features of a "game", but a "game" very different from the "game" of "problem-solving", mentioned before. Here is meant a "game" which finds its goal in itself, born in freedom and perhaps in some way connected with aspects of hazard, because the future will show what remains and what shall be forgotten or proved to be useless. Is the situation in mathematics in this respect essentially different from the Arts? In Dresden's book one actually find similar developments.

One may be inclined to summarize these methodological aspects under the heading of what is commonly called "research" and "doing research" as a special professional scientific activity. However, I have some difficulties in using this terminology when it concerns the really fundamental progress in mathematics. I miss in it the high importance of creation and in particular *free* creation. In "research" I feel some kind of restriction, the idea that there are some concrete results one wants to reach, efforts that may have success but on the other hand may fail - in which case one should try what can be reached. There is in some way a connection with directed creation (see p. 36). Research is also related with formulating conjectures, such as I considered in section III. Conjectures may be a way towards progress. But it is a method which has its limitations. I already mentioned (p. 22) the conjecture of Riemann concerning the zero's of the Riemann-Z-function. Evidently it has no sense to form a group of qualified mathematicians charged with the task to solve this problem. A similar case is the problem of Fermat. I do not believe so much in research-projects for fundamental progress in mathematics. Fundamental progress affords highly individual work. Compare for instance the solution of the continuum-hypothesis by P.J. Cohen in 1963 (see Van Dalen [38]). More recent is the solution of the Bieberbach-conjecture in 1984 by L. de Branges. Essential progress is based on renewing ways of thinking as a creative process. It shall be the subject of the next section.

B. Ways of thinking

In the preceding pages I mentioned the problem of the origins of new areas and theories. This is a problem that can be considered under the heading of "ways of thinking". Indeed, an analysis of ways of thinking can lead to a better understanding of the course of mathematics.

In this context I pointed to the classical idea of "creatio ex nihilo", however without giving there a discussion of the meaning of this idea. I stated that "nihilo" is perhaps a way of expressing that we do not know an answer to the question of the origins. Indeed, "nihilo" has apparently not much sense when it concerns concrete developments. But it may be different when it concerns *ideas* and *ways of thinking,* at least in case we try to give a suitable interpretation. All the more it is then perhaps a structural element of thinking which can also be traced in other areas than mathematics, thus placing mathematics in general Culture.

Creatio ex nihilo then should concern developments apparently established without strict causal relations or influences, spontaneously arising ideas or concepts, as issued from the "Nothing". It is perhaps only a way of speaking to hide the elusive character of this creatio ex nihilo, which works unexpectedly and in different directions. Dresden deals with it in a general philosophical context. He points to Antiquity, to philosophy, to theology and in modern periods to Heidegger and Sartre. I shall not repeat his beautiful reflections. I only remark that the supposition that the "Nothing" has some sense - whatever this may be - leads to an internal paradox: indeed, "Nothing" should not be nihilo. In his book mentioned before Mannoury makes some interesting and rather confusing remarks on this. The mathematical concept of an empty set may be a matter of analogous paradoxical reasoning.

But I restrict myself to mathematics, putting the questions: 1. What should creatio ex nihilo mean in mathematics? and 2. Are there (or have there been) in mathematics developments which can be (or should be) considered as examples, as products, of creatio ex nihilo? Such information, such examples, can be helpful to make us more clear the mystery behind great creations (see my remarks on discontinuities). If creatio ex nihilo is taken in the most strict sense, the answer must evidently be that in mathematics there is no creatio ex nihilo, at least not when it concerns *concrete mathematical theories* with definitions, theorems, etc., concrete theories in the sense of non-philosophical theories, which however may still be "abstract" in the mathematical meaning of this word. With respect to *ideas,* being at the base of theories, we can consider this from another point of view.

The strict "Nothing" cannot be the base of developments in mathematics. Technical mathematical knowledge and also some abstract concepts are - and have been - indispensable in any concrete development, abstract things perhaps less in the older periods than in modern times. This concerns developments in mathematics dealing with "making" and "doing", mathematics in the classical sense of "ars", "technè" (compare Dresden). In section II I stated already that developments, even developments that cannot be explained by causal relations, what I called "discontinuities", are nevertheless prepared in some way by previous developments. They do not grow in vacuum. There are reasonable elements in the course of mathematics, although there is not always a strict causality. The past does not determine the future.

Let me give an example. Consider the so called pure-existence theorems in mathematics, i.e. the principally non-constructive theorems (for instance connected with the axiom of choice or Zorn's lemma). Should we say that it concerns here creatio ex nihilo in the sense that the objects stated in the theorem are brought to "mathematical" existence, by means of the proof, given by means of the methods of proving that are accepted - perhaps the methods that the mathematicians want to accept? Do they thus come from the "Nothing"? Or should they be considered as coming from a reservoir of mathematical ideas, whatever this may be? Do they already "exist" before the proof was given? This "Nothing" is anyhow different from "Construction".

On one hand the answer on this question depends on the way one looks at the nature of mathematics, i.e. the philosophical position. On the other hand it can be seen as a question of interpretation of "nihilo": should it be concerned as a question of ideas or in a concrete sense? But one thing is sure: in the strict "Nothing", or at the base of the nihilo, no mathematics can be practised nor developed. Mathematics is always concerned with research on, or development of *something,* whatever this may be. So the problem is again: what do we understand by creativity in mathematics, or, formulated otherwise, what do mathematicians call creativity? Or perhaps: what is mathematical creativity?

But even if the initiation of new *ideas* on new developments should be considered as having something to do with creatio ex nihilo, the problem remains: where do they come from? When we consider the "Nothing" with respect to mathematics, should we interpret this as "free from causal relations" with past developments? Even this seems to be improbable: mathematics - and surely modern mathematics - is complex to such a degree that useful new ideas cannot be supposed to arise without any previous knowledge. Even in this more abstract sense technical capacity is inevitable. Should we perhaps think of *spontaneously arising ideas,* suddenly arising ideas or concepts coming from critical thinking on the already existing parts of mathematics? In this sense it should be considered as *free creation* where the mathematician who is occupied in a domain, for instance comparing it with other domains, is free in formulating his ideas and giving them form in a theory based on them. On the other hand he is free to abandon them when this theory seems unlikely. This is a form of freedom which does not imply "without any causal relation". It concerns an act of willing. Mathematicians have freedom to create new directions, although there is some continuity in the developments. But freedom is not identical with chaos. In some situations new directions may arise from dissatisfaction with the existing theories when one tries to find improvement. Is there a reservoir of ideas? I do not think so, and this even illustrates the freedom. For a mathematician it is a matter of decision whether or not he thinks it useful to develop a theory around an idea. Interpreted in the right way such ideas can be seen as in some way results of creatio ex nihilo, because the causal connections are difficult to trace.

One may conclude that creativity is a process which is difficult to characterize, so it becomes more mysterious what we shall understand by creatio ex nihilo. Nevertheless, I think that many mathematicians - if not most of them - when asked for information about the origin of a newly introduced idea, will answer that they do not know this exactly, and that the idea might have come spontaneously in their mind when reading and thinking. That is about the same as saying that it came from "Nothing". "Spontaneous" and "from Nothing" have some common aspects. I once asked Brelot, the master of potentialtheory and the initiator of axiomatic potential theory, for information about the origins of the axiomatic theory - clearly far away from differential equations - but I got no real answer, only some vague remarks. Evidently he did not remember any more his arguments to go this way. His idea to detach potential theory from the theory of the differential equations of Laplace and to form an axiomatic theory was highly original (there were some imitations by Tautz and Doob).

Obviously there is no kind of necessity in such new ideas. May we suppose that a concept which was not formulated in a certain period - although some mathematician has had it perhaps in his mind but did not introduce or publish it because of a supposed lack of usefulness - would certainly appear in a future period under other circumstances? Evidently this is an academic question, but I think there is no necessity. But how is this with regard to the really fundamental creations? Is there, notwithstanding freedom, some kind of continuity in the course of mathematics?

Let us consider a concrete example to illustrate how difficult it is to trace the origins of ideas. This example concerns part of my own work from 1942, continued in 1952. I do not mention it because of its importance - there was not much follow up, although the number-theorist Popken told me that he saw possibilities of further applications - but only because I remember the way that brought me to introduce the transformation in question.

This work was concerned with the field Q_p of p-adic numbers. I already mentioned it before as a completion of the field of rational numbers with respect to the p-adic metric. This field can be considered in some way in contraposition to the field of real numbers. I was struck by a theorem in quite another area: the theory of probability. In his treatise *Traité du calcul des probabilités et ses applications,* tome II, fasc. 1: "Applications à l'arithmétique" (Paris, 1926) E. Borel proved a certain theorem which especially draw my attention. He defined a classification of the real numbers in two classes, defining what he called "nombres normaux" and "nombres exceptionnels" (i.e. numbers that are not normal). This is a definition in terms of the frequency of the digits of the decimal expansion of the number. He proved that the set of exceptional numbers has measure zero. In view of the well known representation of the elements of Q_p as a series

$$\Sigma a_i p^i$$

I wondered if such a theorem could also be proved in Q_p. Therefore a measure analogous to Lebesgue measure in $\mathbb{R}$ was needed in Q_p. Turkstra just had defined such a measure in his thesis (V.U. Amsterdam, 1938) *Metrische bijdragen tot de theorie der diophantische approximaties in het lichaam der P-adische getallen.* But I had the idea that this could be done in a more easy way. The idea consisted in defining a map, $Q_p \to \mathbb{R}$ thus leading to a measure by means of this map. I succeeded and could prove the theorem of Borel for Q_p. This map was defined as the map from Q_p into $\mathbb{R}$ generated by the simple transformation $p \mapsto p^{-1}$. I also had the hope that in this way I could get a certain realistic picture of Q_p in some analogy to that of $\mathbb{R}$ in the straight line. But this hope appeared to be false: for topological reasons the map thus defined cannot be bicontinuous. Nevertheless I studied this map and some applications. The introduction of this map rested on a *spontaneous idea,* an idea that came suddenly in my mind. Should we say that it came from the "Nothing" because a strict origin is difficult to give or to trace, since such ideas come unconsciously? There was no *direct* origin. On the other hand, in connection with previous developments I introduced even developments in quite another area.

I think this example demonstrates the intricate way of processes of creation.

Creation goes its own way and is difficult to define. Having read Dresden, I think it is not different in other domains of our Culture.

In Borel's theorem on normal and exceptional numbers we have a rather paradoxical situation. Not a normal, nor an exceptional number is known in an explicit way. What is π for instance? So Borel proved a theorem on objects, not knowing whether there exist any. But what is then the sense of such a classification? Shall we say that his theorem originates in the "Nothing"? (For more about normal numbers see [40]).

Evidently creativity can go too far. Not all what can be defined as a consequence of creativity has mathematical sense, not only because of a lack of usefulness, but also for deeper reasons which lay in the foundations of mathematics.

There is another way of thinking that can be traced in many developments in mathematics. All the more, it seems to be important in many other areas in a general sense, but I shall take my examples from mathematics.

In a previous paper on theoretical aspects of the historical developments in mathematics [1g] I pointed to the importance of tracing ways of thinking which can be seen at the base of historical developments. I mentioned the idea of *conversion* - where properties, proved in a certain theory, are in a later phase taken as a point of departure - and the dialectic way of thinking, unifying different areas. It is not easy to trace ways of thinking which are applicable in trying to understand developments in a general sense, and to place them in a broader framework. One should have some overall picture and I think there is no mathematician anymore who has such a broad view. Nevertheless I tried to do so and was led to the aspects mentioned above.

There is still another way of thinking which can be traced in a general way and has contributed to the progress. During long periods in the course of the development of mathematics one observes that there was much traditional thinking, thinking along customary roads, being conformistic about some theories or concepts, and not until these traditional patterns were broken, new or renewed areas were formed. Such decisive facts may have happened in the form of discontinuities or some spontaneously arising ideas. It is the force of non-conformistic thinking, of deviation from customary ways, which appears as a source of creativity, even of important creations. In this non-conformistic thinking one recognizes a conversion, but the deviation from conformistic is certainly broader. Dresden points to it in his reflections of the alpha-culture, but we find it also in mathematics. Before making some more fundamental remarks about the origins of non-conformistic thinking, I will illustrate this phenomenon by some examples. Let us first consider the history of the concept of an integral.

From the very beginning the concept of an integral was for centuries associated with computation of areas and volumes, with limits of sums (Riemann, Lebesgue, Stieltjes, etc.), with the computation of complicate integrals in closed form or numerical way, with application to other domains, but - except elementary facts, for instance the relation between differentiation and integration

- with little or no theory of the integral. Of course this computational aspect is still present in our days, because it is necessary in applications in nearly any domain of analysis, however not - as in earlier times - as a subject for itself. And furthermore nowadays computers play a role. The situation changed in the first years of our century. Next to traditional, conformistic thinking about the integral there came quite another way of thinking about this concept: non-conformistic thinking. The theoretical point of view came on the foreground. This needed a fundamental change in attitude of mind. This new idea is far away from limits of sums. From this new point of view the concept of an integral is defined as a linear map from a certain vectorspace into $\mathbb{R}$: the integral is introduced as a linear form. The computational facet is no longer the departure, it concerns a more abstract approach. This is the result of a deviation from customary thinking and is in some contraposition to the "realistic" concept of an integral. It was Banach who defined in his book *Théorie des opérations linéaires* (Warszawa, 1932) for the first time this concept, a "universal integral". But what about the origins? It must be seen in the framework of the initiation of functional analysis, in which the Polish mathematicians did important work. So, when the book *Adventures of a mathematician* by S.M. Ulam, belonging to that circle, appeared, I had the hope to find some information in it. But there hardly is any. Many years later this way was also followed by Bourbaki, where one finds a real theory of the integral. Mathematicians do not write about their sources!

This result of Banach should be placed in a much broader framework. Banach's book was the first on the new area of functional analysis, and his universal integral was a rather elementary result in this field. This book can hardly be over-estimated. One finds there numerous results coming from non-conformistic thinking: linear operators, different types of convergence, weak and strong convergence, linear analysis, etc. At the base of this work is the abstract concept of a vectorspace, for itself a result of non-customary thinking about the concept of "space". The idea of vectorspaces differs from the classical way of looking at "space", coordinate-space, coordinate-geometry, because now there came a direct method of defining linear spaces with emphasis on the addition of points: the addition of vectors as a direct and axiomatics process.

Bourbaki's introduction of an integral finds nowadays application in many areas of analysis, for instance in non-archimedean analysis, i.e. analysis over a non-archimedean valued field. The introduction of an integral along the way of Riemann appeared not to be applicable. However, Bourbaki's method succeeded. But this was near to imitation.

To some extent the integral of Banach and Bourbaki can be seen as a result of conversion - I mentioned it before - but this is certainly not the case when we trace back the algebraic aspects of space. This way has been long and we have to go back to Descartes. Before Descartes there was properly speaking no real "algebra". When, for example, a designed a line segment, a^2 was a square, not a rectilinear segment. According to Descartes a^2 was also a rectilinear segment. Thus, algebra and geometry were connected. To come to this standpoint a strong change in attitude was necessary: Descartes broke with classical standpoints. However, it is quite another matter whether the mathematicians involved in such developments felt it as such. But we can look at it in this way and this may be helpful for us to understand.

In a more general framework the standpoint of Descartes can be seen as a first result of a process of algebraization. Such processes are not always to be considered as results finding their ground in conversion, but surely in non-conformistic thinking.

Looking through the history of mathematics it is not difficult to give more examples of such developments. I mention the process of the development of the concept of function. In its first years a function was an analytic expression in some connection with computation: maxima, minima, zero's, etc. In the hands of Dirichlet this became, as seen from quite another standpoint, as an entity, a map. This was a standpoint different from all classical ideas. Only Dirichlet's original way of thinking could lead to this result. We cannot understand this long process by means of the more restricted idea of conversion, nor by arguments of causality.

There is another example in the theory of differential equations. At the beginning one sees the process of finding explicit solutions, in some way processes that were tending to "doing", to computation. After a long period of this "doing", the attitude gradually changed. The emphasis turned to problems of solvability and to pure existence theorems, and for this a fundamental new attitude was necessary, more like the standpoint of modern mathematics. This is also a point of view in other areas of mathematics. All the more, the problem of explicit solutions changed in problems concerning integration by means of computers. We can express this in another way. "Doing" or "making" mathematics turned to creativity of quite another, sometimes more theoretical character. There is a shift from capability towards getting deeper insight, detaching from the classical connections with physics and mechanics, leading towards a more autonomous position.

C. Necessity and freedom

Necessity and freedom seem to be contradictory. Do there exist developments in the course of mathematics which were in some sense introduced by sheer necessity? I think there are, at least when we interprete necessity and freedom in a suitable way. This concerns the necessity-problematic.

There are developments in mathematics which started as a consequence of a creative attitude contrary to customary points of view, but nevertheless had to be accepted as a matter of fact in a form of necessity. This again should be considered as a form of breaking with customary patterns of thinking and, seen in this way, they are an act of freedom. It is an aspect that is not limited to mathematics. Dresden points to Planck and the developments in quantum mechanics. Nevertheless there was some freedom for the initiators to go such ways.

There are well known examples in mathematics of such situations. They are in the field of giving *counterexamples,* which is in some way a methodological aspect, perhaps even an aspect of the mathematical culture. They can to some extent be considered as *directed* creativity, directed in the sense of being in strong connection with existing theories. There is some causality a posteriori in it, but on the other hand there is also freedom. I mention the historical developments on the relations between continuity and differentiability of real-valued functions, and the examples of continuous nowhere differentiable functions, which necessarily had to be accepted in contrast to customary thinking. One can also mention the example of Fourier series, where the old idea that for any real-valued function its Fourier series converges to this function, appeared to be false by means of a counterexample [1f].

The really great progress exists thanks to creativity and in particular free creativity. From this point of view, the emphasis should perhaps more be laid on the fact that mathematics exists as a permanent process of growing, than on the question what mathematics *is.* If at any time this process would come to an end, the essence of mathematics would change, leaving only "doing", "practising" mathematics just as always has been done; in the foregoing I made some remarks about it. Perhaps it might become a science of applications to other domains. A "dead" science? From the current point of view mathematics exists by the grace of creativity and its results. Creativity is a rather confusing concept and I could not give a formal definition. We discussed: creation, invention, discovering, the power of analogy, imitation, the process of generalization, directed creation, creation in relation to processes guided by what can be called "the hazard" in some contrast to directed creation. Indeed, paradoxically, creativity seems to have limitations with respect to its results, although it seems difficult to formulate these limits explicitly. At most we can observe them when mathematicians are confronted with theories which appear to be not satisfying and therefore must be eliminated. I meant "the hazard" in this sense: not an arbitrary "trying without goal" but a critical judgment of results to which acts of creativity have led, something like "hazard a posteriori", with respect to acceptance of theories.

What do we understand by "freedom" in creativity? I think we have to understand by "free": ways not determined by strict causality. Free means that the mathematician is free to choose his way, taking into account the history and what has been done. Are there reasons to make choices? And on which grounds choices should be made? History shows that there are choices. Were they taken on objective or subjective grounds? Have choices to do with insights in the nature of mathematics, with ideas on what mathematics is or ought to be? There are examples of various types, taken on different grounds such as methodological criticism, lack of elegance, definite rejection of theories for reasons of principle, etc. The best way to illustrate this is by mentioning some examples from history. Already Leibniz criticized the algebraical way showed by Descartes to be of a too automatical character. However, after all he did not reject the methods of Descartes.

In a much later period Monge and Poncelet had a similar sort of criticism:

analytical geometry was unsufficiently geometrical. They did not want to eliminate analytical geometry, but their ideas led to new areas in geometry: projective geometry.

Another example. Steiner had objections to analysis and so he became the man of synthetic geometry.

I think in these cases the criticism is connected with the idea that some mathematicians did not make the right choice, or at least that other choices should be considered because they lead to "better" theories.

Here we can mention the discussions in the early years of Cantor's theory of sets with respect to questions whether or not this theory should be incorporated. I think here is not so much a question of choice; it is more a matter of principle.

There are also examples from more recent periods of mathematics, like the introduction of intuïtionism, created in connection with criticism on the foundation of the existing theory. This is again a matter of principle. Still more recent is the introduction of what is called constructive mathematics, in which for instance the axiom of choice is rejected.

On smaller and very secondary scale there are also acts of choice. In the early years of my research in non-archimedean analysis I introduced for reasons of systematization semi-non-archimedean and totally-non-archimedean spaces. Soon I rejected these semi-non-archimedean spaces, continuing with totally-non-archimedean spaces, then simply called non-archimedean spaces, because I had the idea that their definition would not lead to good results. Why? What is a "good" result? And who decides? I think the mathematical community in its totality decides, in the framework of future developments, what is useful and what has not much sense. On the other hand I studied, as an extension of non-archimedean valued fields, non-archimedean metric *spaces* in a general sense and even non-archimedean topologies. There are connections with topological spaces of dimension zero. In some way this process can be seen as a natural process (see section II).

In his fundamental work *Über unendliche lineare Punktmannigfaltigkeiten (Abhandlungen mathematischen und philosophischen Inhalts,* p. 139-246) Cantor wrote about the question of "freedom". From a paragraph where he is concerned with a philosophical discussion about the realistic versus the idealistic standpoint about the number system I quote the following passages - there are some more, but they are too lengthy -:

"Die Mathematik ist in ihrer Entwicklung völlig frei und nur an die selbstredende Rücksicht gebunden, dass ihre Begriffe sowohl in sich widerspruchslos sind, als auch in festen durch Definitionen geordneten Beziehungen zu den vorher gebildeten, bereits vorhandenen und bewährten Begriffen stehen".

"Es ist, wie ich glaube, nicht nötig, in diesen Grundsätzen irgendeine Gefahr für die Wissenschaft zu befürchten, wie dies von vielen geschieht; einerseits sind die bezeichneten Bedingungen, unter welchen die Freiheit der Zahlenbildung allein geübt werden kann, derartige, dass sie der Willkür einen äusserst geringen Spielraum lassen; dann aber trägt auch jeder mathematische Begriff das nötige Korrektiv in sich selbst einher; ist er unfruchtbar oder unzweckmässig, so zeigt er es sehr bald durch seine Unbrauchbarkeit und er wird alsdann wegen mangelnden Erfolg fallen gelassen. Dagegen scheint mir aber jede überflüssige Einengung der mathematischen Forschungstriebes eine viel grössere Gefahr mit sich zu bringen und eine um so grössere, als dafür aus dem Wesen der Wissenschaft wirklich keinerlei Rechtfertigung gezogen werden kann; denn das *Wesen* der *Mathematik* liegt gerade in ihrer *Freiheit* (l.c. p. 182).

Cantor prefers to speak of "freie Mathematik" instead of "reine Mathematik".

The quotations illustrate the confusing, perhaps even paradoxical, situation.

Apparently in practice there are limitations to the freedom of creation, although these limitations sometimes seem to be of subjective character. How can such limitations be explained? Do such criticisms and rejections find their origin in mathematics itself, or are they results of personal points of view? Are they connected with the nature of mathematics? And what are the opinions of those who take a platonic point of view? When it concerns applications of mathematical theories in other domains of science there are perhaps exterior criteria, but when it concerns the internal evolution the facts are very different. In that case the situation is quite different from, for example, the situation in theoretical physics with its experimental criteria. These facts emphasize the special place of mathematics. A parallel can be observed in all these examples. Although there is freedom of creation, it seems that mathematicians had, and still have, some idea, be it a subjective idea, of what mathematics is or ought to be.

I think the majority of mathematicians nowadays take a pragmatic standpoint. Just as Hilbert said that nobody could drive mathematicians out of the paradise created by Cantor, I think the results that can be obtained by certain methods are the essential ground to accept theories. This implies, however, that sometimes points of view with respect to classical concepts must be changed. I mention for instance the evolution of the classical concept of existence in mathematics. Being constructive in former periods, most mathematicians nowadays take a much broader standpoint. In previous publications I discussed this evolution, introducing the ideas of strong respectively weak existence [1c]. This is an aspect of the evolution of "problem-solving" [41].

Another question connected with the idea of freedom of creation. In the history of mathematics are well known examples of the fact that sometimes new ideas and new concepts were, in nearly the same period, introduced independently by different scientists. There were perhaps some differences in the way of introduction, but the development went into the same direction. One can mention Leibniz, Newton and infinitesimal calculus, a discontinuity in the evolution. Poincaré and Stieltjes introduced the idea of asymptotic series from different sides. Banach and Wiener introduced normed spaces about the same time (although Wiener's name is nearly forgotten in this connection), a discontinuity. How can we explain such parallel ideas? Is it a phenomenon that is included in the nature of the special area, a kind of platonism? My personal standpoint is to see this fact as a consequence of the structure of the mental considerations of the mathematicians. I believe they result from their specific way of thinking, from the nature of the mental processes which apparently agree among mathematicians. It is the way of thinking that makes mathematicians to what they are.

In particular in the quotations of Cantor, I find a reason to discuss the role, the function, of giving definitions as a way to introduce new, or renewed, concepts in order to stimulate progress. This problematic, which is connected with giving strong definitions along the rules of logic, belongs more to the domain of mathematical logic and the foundations of mathematics than to the subject of this essay. My remarks will be related to problems on creativity. Problems then are:

(i). Are there aspects of creativity in giving strong definitions to introduce new, or to renew, concepts?

(ii). Does everything that is defined - evidently under the condition that the definition does not contain internal contradictions - exist?

My intention is to demonstrate the sense of these problems by means of some examples in connection with the preceding pages. They illustrate confusing, even to some extent paradoxical situations. It is, however, a subject which deserves further historical studies.

In mathematics several types of definitions were developed in the various periods of history, going back to Antiquity. The nominal definition is classic and the most simple one. When we considered Hilbert's *Grundlagen der Geometrie,* we mentioned implicit definitions. Objects are characterized by means of typical properties, sometimes otherwise proved; they can then be considered as a reversed situation. It concerns a kind of definition of descriptive type. Implicit or descriptive definitions are connected with discussions on constructivity, placed opposite to non-constructive definitions. In constructive definitions one formulates the operations to be executed, in order to obtain the object one wants to define - in opposition to descriptive definitions. But the opinions differed as to what should be understood by "constructivity", in particular when it concerns matters of the transfinite hierarchy. In [12] I pointed to discussions about this problem between Baire, E. Borel and Lebesgue in the first years of the 20th century. Borel used the expression "nommer un objet" to design exact definitions. Peano made extensive studies on this subject. He introduced the idea of "definition by abstraction". An example is to define the equality of the areas of two figures by the fact that they can be decomposed in congruent parts (compare elementary geometry). In this context the work of Frege must be mentioned. Russell rejected this type of definition. See [42], [43].

As to the relation between definitions, existence and creativity the best we can do is to show by means of some examples that here too there are different opinions. In Kennedy [43] one reads under the heading "The existence of the thing defined is not necessary": "In mathematics definitions of non-existent things are numerous". He gives the following example (there are some more): Euclid gives a name to the largest prime number and then proves that it does not exist, a paradoxical situation. This standpoint is in contrast with the attitude of Van Dalen, commenting a passage of Frege ("Jeder aus den definirten Namen rechtmässig gebildete Name muss eine Bedeutung haben"): "Met andere woorden men kan wel definiëren wat men wil, maar men wordt niet ontslagen van de verplichting aan te tonen dat het gedefiniëerde bestaat" [38].

Before I mentioned the introduction of a type of integral defined for *any* bounded real-valued function as an example of perfectness. It is called a *universal integral,* just because it is defined for *any f.* It concerns a non-constructive result. But it is not difficult to give a descriptive definition. One formulates conditions on additivity - finitely additive whether complete - and invariance with respect to certain groups of transformations (for instance translations). It is known as the *measure problem,* formulated for measures in $\mathbb{R}^n$, which is equivalent. The problem is connected with the existence of such a measure for *every* bounded set: a universal measure. In some cases a *universal* measure is proved to exist; in other cases it does not exist. These results are proved with transfinite methods. Further studies led to the field of topology and concerned also questions of principle. I wonder whether one can "do" mathematics with them. However, the underlying idea is very interesting and based on a creative attitude. See [1f].

Another example concerns the so-called Denjoy integral. One may say that the difficulties with regard to differentiation and integration as opposite operations found their final solution in this integral. In the classic book of Saks on the theory of the integral [44] one finds the way of introducing it: (i) "Descriptive definition of the Denjoy integrals", and (ii) "Constructive definition of the Denjoy integrals". Constructivity must be understood here in a special sense because transfinite methods are used ("Denjoy constructed a transfinite hierarchy of methods of integration"; [44], p. V). Does this concern a means "to do"? It may be explained to some extent by considering constructivity in the sense of recursive definitions, operations which could be performed if we could go on for an infinitely long time. Saks compares the Denjoy integral with the classification of real-valued functions given by Baire. Should this classification be considered then as constructive? I think it is not so easy to give a descriptive definition here. Compare, from the other side, the descriptive definition of an integral according to Bourbaki.

A further example is the introduction of normal and exceptional numbers by Borel, as mentioned before. But what is the sense of Borel's theorem, taking into account that we do not know whether the objects with which this theorem is concerned exist or not. Compare a paper by Gale [40], where normal numbers are considered with regard to the problematic of the foundation. See also the introduction of the concept of a distribution by L. Schwartz. It is non-constructive, nevertheless very important for "doing" mathematics.

As a conclusion from these examples I think we may say that giving definitions just to introduce new or generalized concepts as a purpose for itself is hardly to be seen as a creative activity. I think, for instance, on the inventivity in introducing complicated concepts in general topology and the theory of real-valued functions in the years round the turn of the century. I made some remarks about this in [1f]. But the *ideas* that precede definitions, ideas which grow in mind, may be of a very creative character, whether or not they appear worthwhile to be followed. And it is quite another matter whether ideas are followed by formal definitions. The ways of thinking are important. Do such ideas come from the "Nothing", creatio ex nihilo? Comparing with Dresden I think the situations in mathematics do not much differ from some aspects of the alpha-culture.

The idea of free creation leads to speculations about the future. Earlier I introduced the idea of looking at the evolution in the form of phases. Can a new phase be expected at a still higher level, after the theory of sets and axiomatization? Should the theory of categories be seen as such a higher level?

Freedom of creation also leads to speculations about the past. If there exist concepts which cannot be explained by means of some causal arguments - that means if there is free mental activity, creatio ex nihilo - is it conceivable that in the course of history of mankind "another mathematics", a "different mathematics", could have been developed, started by scientists thinking in another way and developing creations leading in a different direction? It is difficult to define what should be meant by "another mathematics". Perhaps one should think of a kind of "mathematics" with totally different concepts, unknown for us - such as the concepts of set theory were unknown before Cantor - but apparently on the basis of the natural numbers. It is clear that hardly any answer can be given. I only pose the question to emphasize the problems which present themselves when one enters the area of philosophical reflections about the nature of mathematics and the idea of "free creation", which is connected with it. However, to pose this question is less absurd than it may seem to be at first sight. There is even some literature about it. In a paper *Mathématiques vides et mathématiques significatives,* included in a series of articles by several mathematicians [45], Dieudonné, referring to the work of Gödel and Cohen, writes about various mathematics ("autant de mathématiques que vous voulez") and later on he points to mathematics according to the mind of mathematicians ("suivant le genre") [46]. It is not quite clear what Dieudonné means by various mathematics. Should it be understood in the sense that there are subjective elements in mathematics, i.e. that mathematics is not unique?

Wilder [38] speaks about mathematics in relation to other cultures than ours (Chinese, Maya, Arabic).

In a book review [47] I read that "there is now a whole spectrum of "experimental mathematics" which poses some very serious questions for mathematicians". What had happened if mathematics had been developed as an experimental science? A very recent note by Keith Devlin should be mentioned here: "Mathematics without theorems" [48]. The author discusses the idea of the mathematical enterprise as the study of structures, being a particular aspect of the world in the form of abstract representations (or models), analyzing these models in a rigorous logical fashion. It concerns "model-building" as an initial stage, preceding our "theorem-proving" aspect, thus opening the possibility of "different models". Does Devlin mean "system-building", preceding "operations of proving", to be applied to them?

In another recent paper, "What do we do when we do mathematics?" Ernst Snapper raises explicitly the question of the unicity of mathematics: "I do not believe that mathematics is a unique doctrine, but that there are several kinds of mathematics, depending on how one *chooses* to create classes and chooses to prove theorems" [49].

In the same context there is an editorial note in *Computers and Mathematics* entitled "Computers and the nature of mathematics" [50]. The author gives as his opinion that "mathematics is the science of patterns". I have some difficulty with it. Can "doing" and problem-solving find a place under this heading? I am inclined to see strict logical proving as a characteristic aspect. Compare also the opinion of MacLane about the question what algebra is [1f].

The main question in all this is: *our mathematics, such as we developed in two thousands of years in connection with "freedom of construction", is it unique?* This is a theme that deserves further study.

A last remark. It might be expected that in this section some notes about the position of "free creation" against the background of the foundations of mathematics - in particular on the paradoxes and contradictions in relation to axiomatic systems - should have found a place. However, this subject was let aside because I have insufficient competence in that area. I only want to state that in my opinion problems on the foundations should well be distinguished from the problem what mathematics *is* and that this last problem should precede all discussion on the foundations.

V. Conclusions

I will resume and try to come to conclusions. As seen from a very general standpoint the reflections in the preceding pages can be resumed as a study on the question "What is mathematics"? But then there are two directions. If the accent is on "what", it concerns a matter of *quality of mathematics,* about which I gave some examples. However, if the accent is given to "is", we have another interpretation and I think this is a deeper one. Reading the question in this way, it concerns the *ontology of mathematics.* This is a very difficult philosophical problem, on which there exists no common opinion.

To come to an answer - if possible anyhow - I tried to give an analysis of the origins and sources of mathematical theories and theorems, in connection with causality in the course of mathematics. I came to design creativity as a most important aspect. In particular free creativity was discussed.

At the end, reading Dresden's book on creativity once again, it is most remarkable to observe that most of the facets he discussed in his framework - the so called alpha-culture - are in adapted form equally applicable to a study about the progress of mathematics. One gets an impression of unity, a kind of unity of alpha-culture and beta-culture. However, there is also a very fundamental difference. The fundamental aspect of proving in mathematics, according to strong rules of logic, does not find a place in Dresden's work. There is perhaps some kind of proving in the alpha-culture, but this is more in the sense of plausible inference, anyhow not according to strong logical reasonings. May we conclude that strong logical conclusions are restricted to mathematics and thus are characteristic for this science? I mean logical in the sense of the logical system that is accepted: intuïtionism, formal systems, etc. In mathematics acts of creativity need to be closed by strong logical proving. In other words: is this what we mean when we speak of the *nature* of mathematics - which is different from discussing what mathematics in existential sense *is?* I think it is not the right point of view to see mathematics as a system that "is", that exists as a domain that is clearly fixed and demarcated, and that should be explored. In the preceding pages I gave special attention to mathematical creativity and its working. Mathematics has a strong dynamical character under the working of creativity. Mathematics is permanently involved in processes of generating new ideas and concepts, new theories: mathematics *is* not, mathematics is *generated.* Mathêmatics is connected with ways of thinking, specific mathematical-thinking. The capacity of "doing" is not the primary characterization. When this capacity is diminishing in the course of time, nevertheless this way of *thinking* remains. Just this makes it difficult to express the various aspects of the nature of mathematics in the form of a general definition, valid for all what is going on and for all time, a method which in principle determines the direction to go in future. In view of the difficulties to come to more insight in the nature of mathematics one could, in stead of starting from the mathematical objects themselves, try to approach the problematic from the side of the mathematicians who are involved in the investigations, thus avoiding problems on existence. Shall we *consider* as "mathematics" any system in which the activity of strong logical proving and coming to conclusions is the most essential criterion (even if there are different opinions among mathematicians on what is accepted as a "strong proof")? I return to this idea in the Appendix (see also [51]).

This finally leads to the question *Wat is creativiteit?*, i.e. the title of Dresden's book. In the last pages he tried to find an answer to this question, but he remarked himself that he did not succeed in a satisfactory way. He could only pose counter-questions. I quote: creativity "bevindt zich tussen zijn en niet-zijn, en verwezenlijkt zich alleen maar voorlopig in een werk" [creativity is something between to be or not to be and is only provisionally actualized in a work]. I think this is also true for mathematics.

Appendix

Themes for Further Special Studies

In this Appendix I add to the considerations in the preceding chapters some special themes for further historical-philosophical studies. They are close to the preceding reflections.

1. *The philosophy of the "why?" and "quod erat demonstrandum".*

A. In the preceding chapters one shall miss more detailed remarks and reflections on the concepts of "proving" and "a proof", notwithstanding the fact that these concepts are of a high importance - perhaps even the most important aspect of mathematics. They should be taken into account when determining the place and function of mathematics among Human Sciences. It is the old "quod erat demonstrandum". I already made some remarks on this in [1h]. I prefer to incorporate this subject in an Appendix.

First of all: my intention with these remarks is that they may be of some use for formulating themes for further studies. There are more questions than answers. A possible reaction is that much has been written about this subject and that there is not much new in these reflections. But I observe that in a most general sense "proving" is not restricted to mathematics, although for mathematics with its strict formal lines and conclusions there is a special situation. I hope that reflections on "proofs" in such a broad sense, considering "proving" from a multi-disciplinary standpoint, may contribute to determine the position of mathematics. A future study in this field must be based on diverse specialistic knowledge and therefore perhaps needs cooperation. In mathematics "proving" is to some extent the closure of reasonings and the process of creation. On the other hand, proving theorems is not, by exclusion of any other method, the only way towards mathematical truth. There exist other ways. In the preceding chapters I pointed to "making" and "doing" mathematics, where technical methods to find formulas, propositions and applications, etc. play a role. There it concerned the question of capability, creation in another sense.

When I consider "proving" from a multidisciplinary standpoint, I mention for instance "proving" in the juridical sciences, and aspects of "proving" in some disciplines of the so-called Alpha-Culture. In such non-mathematical sciences one finds reasonings where methods are used which also appear in mathematics: implications, the reverse of reasonings, etc. But they not lead to the strict formal logical conclusions we have in mathematics. There is some analogy with mathematics in *form*, but not in *intrinsical* value. Seen in this way

- giving "proofs", asking for certainty and for truth, adapted to special cases and disciplines - the activity of "proving" must perhaps be considered as a special phenomenon of Human Culture. Is "proving", in whatever form, a fundamental element of progress in human civilization? Proving is connected with "not-believing-without-any-more". From such a standpoint "proving" belongs to the area of the problematic of the "why?", the problem of asking for motivations, for reasons of conclusions. The problem then arises of the origins and the development of the "why?". How far does this go back in history? The question then is not how we prove and which methods are used or accepted, but why we prove. Here it concerns the genesis of the problematic of the "why?". This should not be confused with the modern mathematical "proof-theory". I am not concerned with the proofs themselves and their theory, but with the philosophical backgrounds of "proving".

It seems apparent that the problematic of the "why?" and the activity of giving "proofs" are in a broad sense connected with each other and these connections have several sides. A comparative study of this domain is not only of interest from a philosophical and psychological point of view but also in a broader sense. I have conclusions in mind which can be drawn with respect to the place and the essence of mathematics. Let me give some examples.

Considering this problematic from a didactical-psychological point of view I refer to a passage due to Freudenthal: "The highest pedagogical virtue is patience. One day the child will ask why, and there is no use to start systematic geometry before that day has come. Even more: it can really do harm. For we have agreed upon teaching geometry as a means to make children feel the strength of the human spirit - that is of their own spirit - and we should not deprive them of the right to make discoveries of their own. The clue of geometry is the word "why". Only joy-killers will deliver the clue previously" [52]. In this context it is worth noting that when in my country one asks a child "Waarom doe je dat?" ("Why do you do that?"), it is not unusual that the answer is "daarom" (to some extent equivalent with "therefore"). The play with words is thus "wherefore - therefore". We are here in the field of psychology: the child does not yet understand that "daarom" is no motivation. So the parents' reaction is "daarom is geen reden" (therefore is no reason).

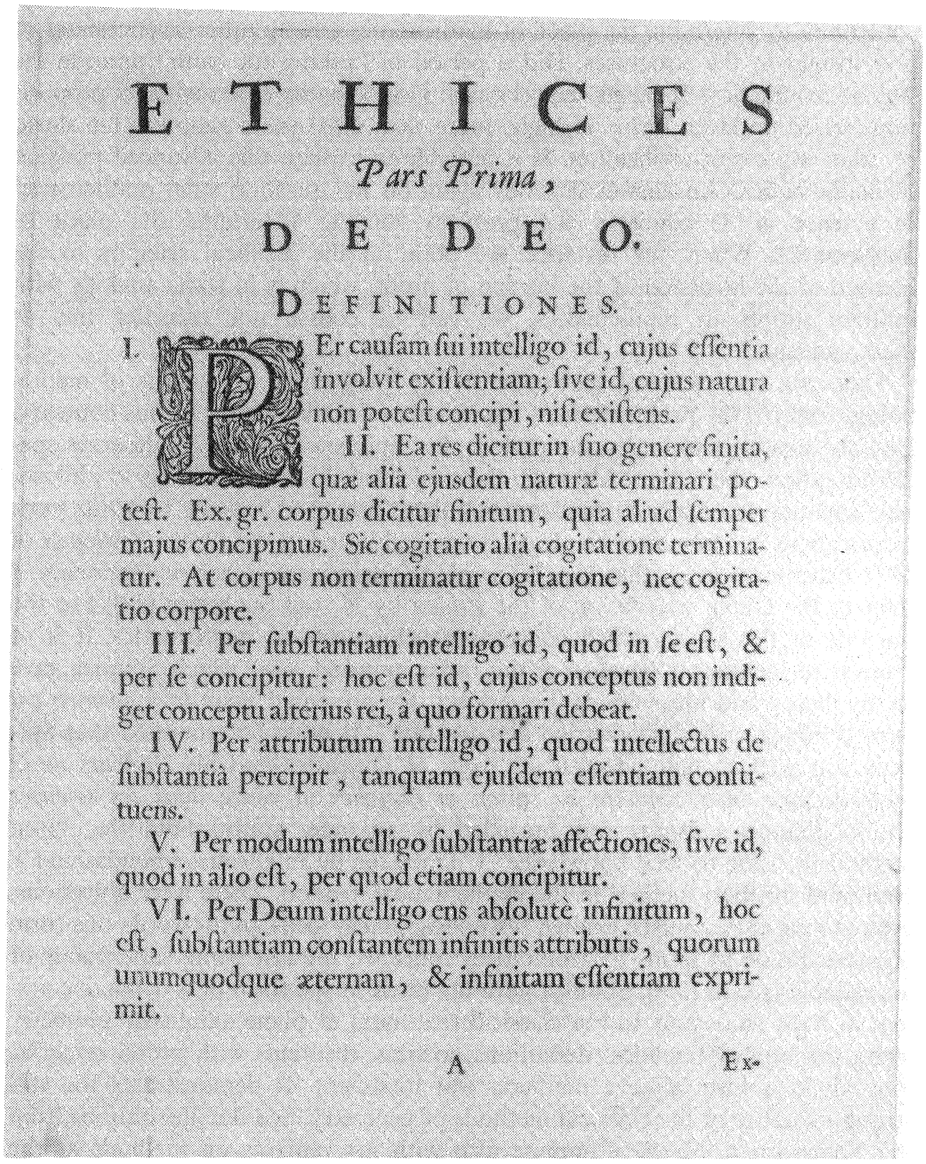
In the course of their development children have to learn what it means to give answers in reasoned-out forms, what is essentially different from saying that this is an innate aspect. This leads into the domain of psychology.

It is not my competence to analyze processes which are considered and called "proofs" in non-mathematical domains. I can only add some more remarks to what is said in the preceding pages. Considered from the mathematical point of view it does not concern here strong logical proofs and conclusions but reasonings leading to some form of credibility, to judgments based on implications, etc. There are reasonings based on methods of exclusion of situations by means of specialized criteria, but not in strong logical form. There are methods of "proving" the origin of old manuscripts or the identity of painters (problems of falseness, respectively authenticity). I also point to the witch-trials. Such reasonings can go far back in history.

But we return to mathematics. It is a matter of fact that the old Greek mathematicians had at their disposal methods of proving that were based on forms of logic, even axiomatic methods. It would be interesting to know something more about the arguments that led them to introduce and follow this way, that is again the question of the "why?". I think such knowledge could contribute to determine the place of mathematics among other sciences and to give insight in the structures. Did a period of "asking for truth" precede the way of axiomatics? It seems that classical Greek mathematics with its propositions based on strict forms of logic, more than 2000 years ago, was far ahead of other aspects of civilization. Is it possible to explain this advanced position of mathematics? An answer depends again on the question what mathematics in essence *is*. It concerns the problem here to determine the place of mathematics. When, for instance, we point in the juridical sciences to the method of the inversion of the burden of proof, which is in some analogy with indirect proofs in mathematics, we can of course not consider this as mathematics.

There are two elements that come in the foreground: (i) aspects of methodology and (ii) the problematic of the essence of mathematics. In this context a classical treatise written by the philosopher Spinoza (1632-1677) deserves special attention: the *Ethica*. Although the subject of this book belongs to philosophy and not especially to mathematics there are good reasons to write some words about it. Only after his death it was published in the *Opera Posthuma* of 1677 because he knew that his philosophical ideas were rather revolutionary. I refer to the Dutch translation of the *Ethica* by N. van Suchtelen [53]. For the purpose of this essay, which concerns developments in mathematics, it is of interest to mention it because of the rather unusual *form* which Spinoza gave to his theory and ideas about this subject. He presented them in a *geometrical form*: "Ethica ordine geometrico demonstrata". It must be remarked that Spinoza was well acquainted with the work of Descartes and his *Discours de la méthode pour bien conduire sa raison et chercher la vérité dans les sciences* (1637). Spinoza's *Ethica* was preceded by an older study about the "best method to come to true knowledge" ("Tractatus de intellectus emendatione"). He found this best method in the mathematical method. There were objections, even among cartesianists, against his philosophical ideas and towards this form of presentation in terms of geometrical concepts, the last being superfluous or unreadable (see in [53]). Spinoza gave his ideas in the form of a deductive system in *form* analogous to the classical treatment of plane axiomatic geometry along the way of Euclides: definitions, axioms, theorems with proof, remarks, thus all in a form alike a mathematical treatment. It demonstrates the still strong influence of the classical methods of geometry. See the illustrations from the *Ethica* on p. 62-64. Compare also with my remarks on methods as the essence of mathematics in the forgoing pages. In philosophical context Mannoury made remarks on the *Ethica* in his *Handboek der analytische Signifika*, I, p. 100, 147 mentioned before.

But does it really concern a subject that is appropriate to mathematization? Can an analogy in *form of composition* be an argument to say that it thus



ETHICES

Pars Prima,

DE DEO.

DEFINITIONES.

I. PEr cauſam ſui intelligo id, cujus eſſentia involvit exiſtentiam; ſive id, cujus natura non poteſt concipi, niſi exiſtens.

II. Ea res dicitur in ſuo genere finita, quæ aliâ ejuſdem naturæ terminari poteſt. Ex. gr. corpus dicitur finitum, quia aliud ſemper majus concipimus. Sic cogitatio aliâ cogitatione terminatur. At corpus non terminatur cogitatione, nec cogitatio corpore.

III. Per ſubſtantiam intelligo id, quod in ſe eſt, & per ſe concipitur: hoc eſt id, cujus conceptus non indiget conceptu alterius rei, à quo formari debeat.

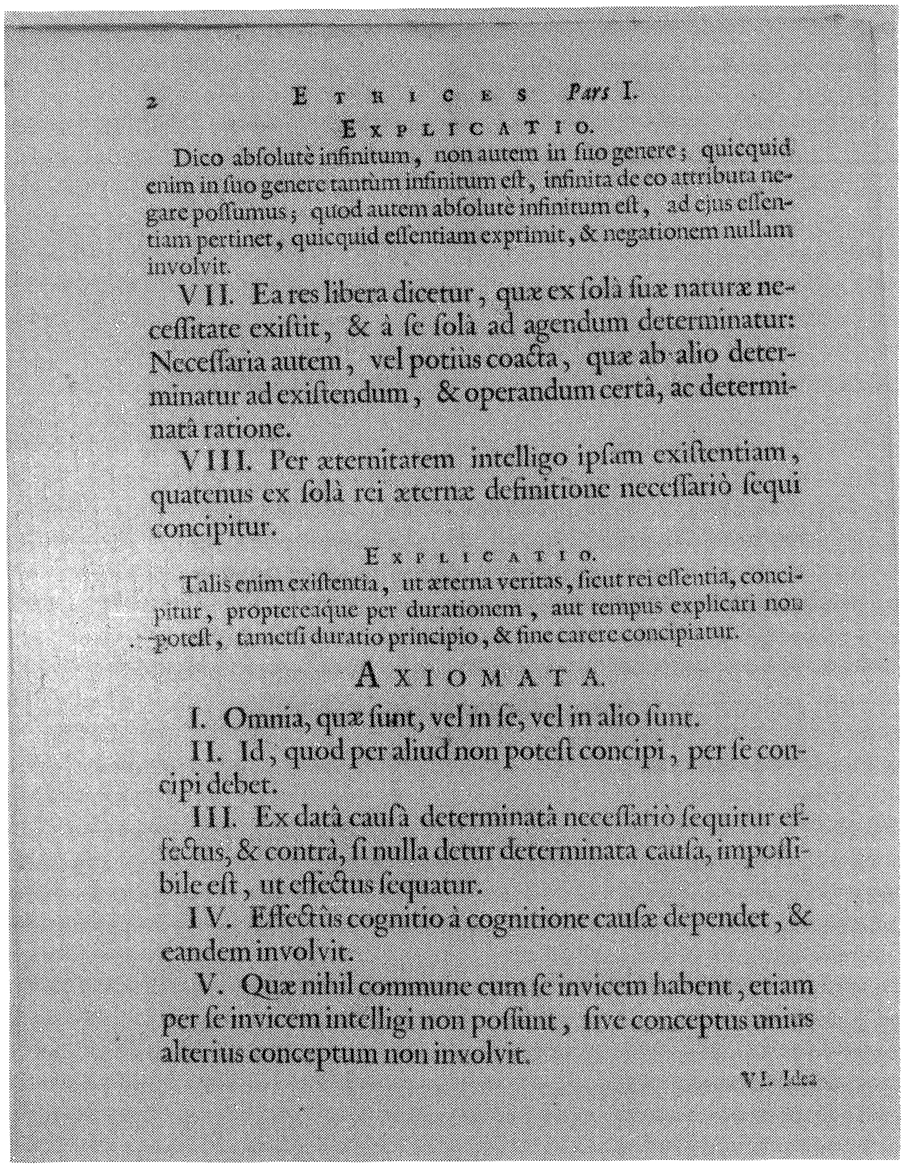
IV. Per attributum intelligo id, quod intellectus de ſubſtantiâ percipit, tanquam ejuſdem eſſentiam conſtituens.

V. Per modum intelligo ſubſtantiæ affectiones, ſive id, quod in alio eſt, per quod etiam concipitur.

VI. Per Deum intelligo ens abſolutè infinitum, hoc eſt, ſubſtantiam conſtantem infinitis attributis, quorum unumquodque æternam, & infinitam eſſentiam exprimit.

A Ex-

B.d.S. [Benedictus de Spinoza], Opera Posthuma. *[Amsterdam] 1677. Photo: Koninklijke Bibliotheek, Den Haag.*



2 ETHICES *Pars* I.

EXPLICATIO.

Dico abſolutè infinitum, non autem in ſuo genere; quicquid enim in ſuo genere tantùm infinitum eſt, infinita de eo attributa negare poſſumus; quod autem abſolutè infinitum eſt, ad ejus eſſentiam pertinet, quicquid eſſentiam exprimit, & negationem nullam involvit.

VII. Ea res libera dicetur, quæ ex ſolâ ſuæ naturæ neceſſitate exiſtit, & à ſe ſolâ ad agendum determinatur: Neceſſaria autem, vel potiùs coacta, quæ ab alio determinatur ad exiſtendum, & operandum certâ, ac determinatâ ratione.

VIII. Per æternitatem intelligo ipſam exiſtentiam, quatenus ex ſolâ rei æternæ definitione neceſſariò ſequi concipitur.

EXPLICATIO.

Talis enim exiſtentia, ut æterna veritas, ſicut rei eſſentia, concipitur, proptereaque per durationem, aut tempus explicari non poteſt, tametſi duratio principio, & fine carere concipiatur.

AXIOMATA.

I. Omnia, quæ ſunt, vel in ſe, vel in alio ſunt.

II. Id, quod per aliud non poteſt concipi, per ſe concipi debet.

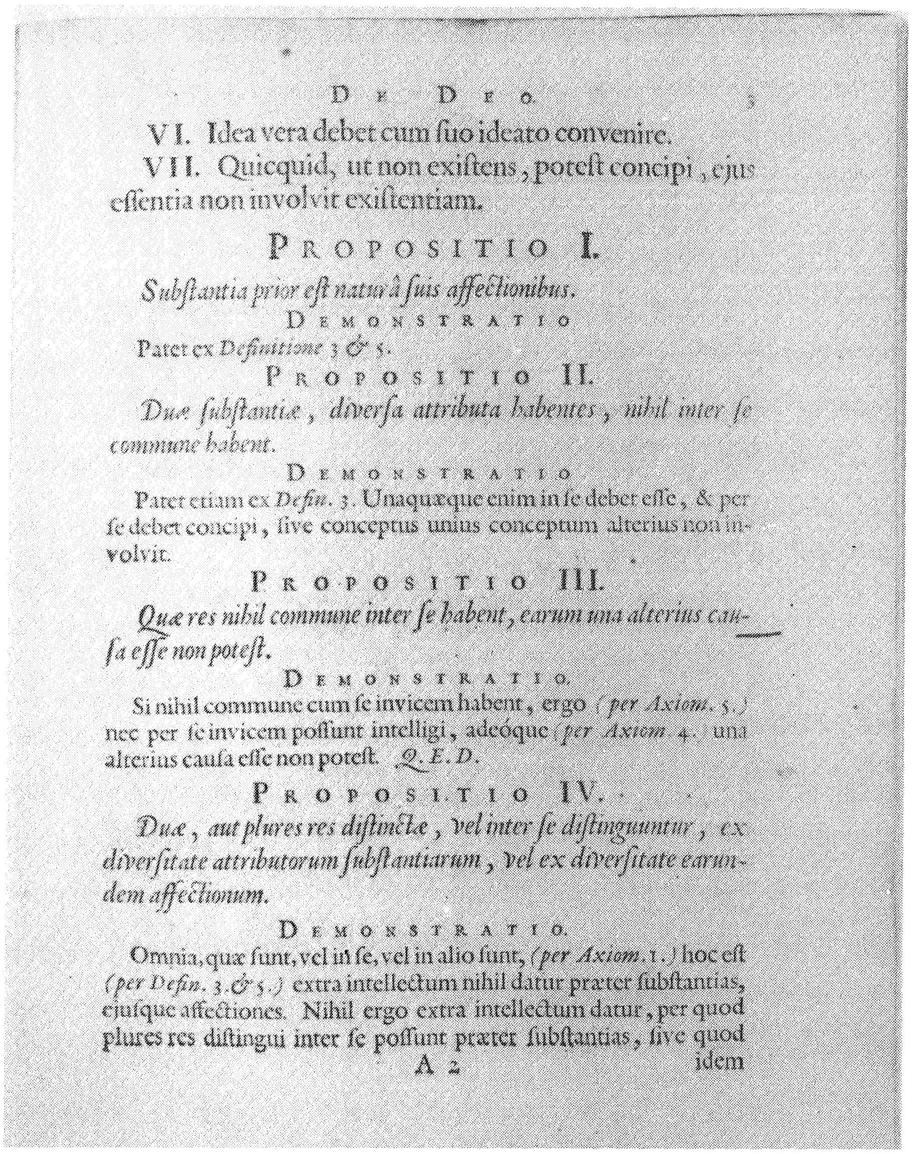
III. Ex datâ cauſâ determinatâ neceſſariò ſequitur effectus, & contrà, ſi nulla detur determinata cauſa, impoſſibile eſt, ut effectus ſequatur.

IV. Effectûs cognitio à cognitione cauſæ dependet, & eandem involvit.

V. Quæ nihil commune cum ſe invicem habent, etiam per ſe invicem intelligi non poſſunt, ſive conceptus unius alterius conceptum non involvit.

VI. Idea

B.d.S. [Benedictus de Spinoza], Opera Posthuma. *[Amsterdam] 1677. Photo: Koninklijke Bibliotheek, Den Haag.*



DE DEO. 3

VI. Idea vera debet cum ſuo ideato convenire.

VII. Quicquid, ut non exiſtens, poteſt concipi, ejus eſſentia non involvit exiſtentiam.

PROPOSITIO I.

Subſtantia prior eſt naturâ ſuis affectionibus.

DEMONSTRATIO

Patet ex *Definitione* 3 & 5.

PROPOSITIO II.

Duæ ſubſtantiæ, diverſa attributa habentes, nihil inter ſe commune habent.

DEMONSTRATIO

Patet etiam ex *Defin.* 3. Unaquæque enim in ſe debet eſſe, & per ſe debet concipi, ſive conceptus unius conceptum alterius non involvit.

PROPOSITIO III.

Quæ res nihil commune inter ſe habent, earum una alterius cauſa eſſe non poteſt.

DEMONSTRATIO.

Si nihil commune cum ſe invicem habent, ergo (*per Axiom.* 5.) nec per ſe invicem poſſunt intelligi, adeóque (*per Axiom.* 4.) una alterius cauſa eſſe non poteſt. *Q. E. D.*

PROPOSITIO IV.

Duæ, aut plures res diſtinctæ, vel inter ſe diſtinguuntur, ex diverſitate attributorum ſubſtantiarum, vel ex diverſitate earundem affectionum.

DEMONSTRATIO.

Omnia, quæ ſunt, vel in ſe, vel in alio ſunt, (*per Axiom.* 1.) hoc eſt (*per Defin.* 3. & 5.) extra intellectum nihil datur præter ſubſtantias, ejuſque affectiones. Nihil ergo extra intellectum datur, per quod plures res diſtingui inter ſe poſſunt præter ſubſtantias, ſive quod

A 2 idem

B.d.S. [Benedictus de Spinoza], Opera Posthuma. *[Amsterdam] 1677. Photo: Koninklijke Bibliotheek, Den Haag.*

concerns mathematics? The answer is evidently negative. Analogy in *form* does for itself not imply strong logical reasonings, based on strong definitions and axioms, which is fundamental for mathematics. This example suggests me to follow another way when it concerns the problem of the essence of mathematics.

Taking into account these analogies in *form* of composition between various scientific disciplines and mathematics - in some cases more than in other ones - the problem presents itself to give an analysis of this phenomenon. Now, there is a fundamental difference, which can be found in the concept of "proving" as a means to build theories. In mathematics "proving" is essentially different from arguments in the weak form of credibility, based as it is on strong logical reasonings, reasonings in a formal form, i.e. formalization.

The philosophical problem of the nature, the essence, of mathematics is related to questions about "proving" and there is reason therefore to study this aspect somewhat nearer. In section IV, p. 29 I already made some remarks on relations between mathematics and disciplines of the Alpha-Culture and I pointed to some analogies.

The question of the *nature of mathematics*, the question what mathematics in existential sense *is*, is subject of several writings, but a more or less definite answer appears to be a difficult problem and, moreover, the opinions do not coincide. The preceding remarks on "proving" in a broad sense lead me to propose an approach of this problematic which perhaps is unusual. In view of the fact that giving "proofs", in whatever form, exists as a matter of fact in several disciplines, one can, in a sort of conversion of ideas, pose the problem to determine in a comparative study the place of mathematics in the totality of these disciplines. When the problem of the nature of mathematics is difficult to answer, one can at least try to find a demarcation of mathematics and the relative position with respect to other disciplines. It concerns then reflections *about* mathematics. Perhaps aspects of "proving" can be used as a criterion to mark this position. Is the place of mathematics in the totality of "proving-sciences" determined by the criterion of strong formal logical proving and coming to conclusions? Is this the deciding point for being mathematics, or, in a weaker form, for being considered as mathematics or perhaps called mathematics, accepting the subjectivity of this standpoint? This would be a criterion avoiding the difficulties in formulating standpoints with respect to the nature of the mathematical objects, a difficult domain, mostly studied with rather vague results.

I am well aware of the fact that such a thesis does not concern the problem what mathematics is. But I consider this as another problem, as it is also another matter which methods of proving are allowed, provided that they are given along strict logical lines. This leads into the area of the standpoints *about* mathematics: Hilbert with formalism, Brouwer with intuïtionism, etc. This concerns the matter of the foundation of mathematics and in my view the problem of the nature of mathematics as a philosophical problem should precede questions of foundations. Before we can speak about foundations there must be clearness about *what* it concerns. From the preceding chapters it will be clear that in this respect I think in the first place of human creativity and not in some way on origins of modern mathematics in "nature", whatever this may be. The problematic sketched above is in close connection with Dresden's general reflections in *Wat is creativiteit.* Compare [54].

I propose the developments around the "Why?" and "proving" to be the subject of a multi-disciplinary study, a theme for a general dispute [55]. The essence should be "The philosophy of the "Why?". Besides mathematics and logic there should be a place for a study on the origins, the form and function of the "Why?" and "proofs" in, for instance, (1) philosophy, (2) antiquity, (3) psychology, (4) some alpha-sciences, (5) juridical sciences, (6) experimental sciences.

B. The origin of the *idea* and the evolution of mathematical proof and the signification of proving in the course of times. What is the genesis of the concept of proof in mathematics as a phenomenon? How rose the need of proving and what was considered as a proof? Which were the means of proving in the course of history? Here the ideas of what is accepted as a proof in the various periods should be taken into account. A critical view on what we call mathematical rigour should find a place, avoiding to impose modern standpoints on the past. The way towards intuïtionism and constructivism should be discussed. And with respect to recent developments the role of computers can be a point of discussion. See also my remarks on experimental mathematics. See also in [54], [16].

2. *Problems on axiomatization*. The history of methods of axiomatization from the geometrical origins in Antiquity to the penetration in algebra and analysis.

3. *History of "problem-solving"*. Here the various standpoints play a role which were taken by mathematicians in the historical periods with respect to the means by which problems were or ought to be solved. Under which circumstances was a problem considered as being solved? In previous publications I already mentioned that only half a century ago the solution of a problem in elementary geometry (a construction of some figure) should consist of four elements: an analysis, the construction, a proof that the construction satisfies the conditions, a discussion of the conditions under which there is solution. Which are the origins of these conditions? Well known is the classical condition on the use of ruler and compasses. Why just these? How was the development of other methods? Connections with classical unsolved problems and the role of approximations should have attention. Many centuries later geometry with only the compasses was developed. In algebra the developments on the solution of algebraic equations in terms of radicals is another subject. This finds its continuation in the next subject.

4. *Constructivity and constructions*. The role of constructions in the periods of history and the evolution towards non-constructive theories. In particular I have in view here the developments in analysis. In the classical, constructive period mainly explicit solutions, for instance of differential equations, were accepted. Existence was constructive. Which was the way towards the

acceptation of non-constructive results and theories? What has been the role and place of approximations? See [56] (p. 128) and also my publication on strong respectively weak existence in analysis [1f]. Finally there is the way back to constructive theories and the rejection of non-constructive theories by some mathematicians in recent times [57].

5. *External and internal influences.* Earlier [1c] [1f] I designed the classical period as a period with strong external influences, whereas modern mathematics is a mainly internal period, with strong autonomous developments. The transformation from the external into the internal period should be a subject for further studies. Compare my idea of describing the evolution as a chain of phases.

6. *Sets and algebra.* Mutual relations in the development of the theory of sets and modern algebra. See [1e] p. 340.

7. *Modern algebra and analysis.* The penetration of concepts of modern algebra in analysis.

8. *Rise and decline of mathematical subjects.* The last subject, which I shall explain here in a somewhat more elaborate way, is not so much concerned with the evolution of concepts; it concerns the rise and decline of subjects in mathematics.

It is a common phenomenon in the history of mathematics that subjects, which in their proper period seemed to be important, passed into the background or even nearly disappeared.

Only half a century ago, for example, the theory of continued fractions found a place on the university programmes beside the theory of infinite series. Now they have lost their place. Continued fractions have never been of equal importance as the theory of series. However, there are indications that continued fractions return in a somewhat different form in connection with algorithms.

The strict theory of real functions, important in the early years of the theory of sets and in the first decennia of our century, has not so much interest any more (see [1a], p. 57). Which are the reasons for the decline?

One can also mention the rise and decline of descriptive geometry. Classical analytic geometry with coordinates has lost the important place it had half a century ago in the programmes of the universities. It is mostly replaced by linear algebra. Has there been any influence of this old form of geometry on the creation of the new form of geometry?

Classical differential geometry in the sense of Gauss is replaced by a subject with a very different and much more abstract character (theory of varieties, etc.). In a review of a book on Riemannian geometry [58] one can find the following passage:

"One has only to compare this material to that in the collected works of Cartan and Lie and in Darboux' *Théorie des surfaces* to realize how much of our heritage has been jumped overboard. Perhaps this is due to our overemphasis on maintaining our status in the eyes of our big brothers, the topologists".

There are analogous aspects in the development of algebraic geometry as treated in the classical way by means of coordinate systems. It is still fully alive, but in a quite different form. Methods of modern algebra and modern algebraical concepts have taken the place of the old methods. Algebraic geometry has become an abstract theory to such an extent that one may ask whether it concerns algebra or geometry [59]. This development is an aspect of the general tendency towards abstraction. But it is curious to learn from algebraic-geometers that in recent years the more concrete algebraic geometry seems to return to some extent. There seems to be a more general tendency to a revival of more concrete results. What can be said about the reasons of such a development?

In the rise, decline and sometimes return of subjects we seem to have a general phenomenon in the evolution of mathematics, whose general aspects deserve to be studied. Are there something like waves in the evolution? What can be said about the motives of this phenomenon in a general way? Do the reasons differ from case to case? Perhaps some subjects fade away because new and important results in such an area are no longer obtained in new, and in general higher phases. Perhaps they return sometimes because just in the light of new phases new light can be thrown on the subject, or because the matter appears to be useful in a new phase [60].

The interesting question rises whether there are analogous situations in other sciences. In physics there are empirical reasons for the rise and decline of subjects. I think in mathematics this is mainly due to internal influences.

In this respect D. van Dalen posed an interesting question. Mathematicians are used to speak of a beautiful theory or proof. His problem is whether a subject can survive because it is beautiful or elegant, even if it is perhaps of no much use. I do not know an example of such a phenomenon. But there is then the question: what is "beauty" in mathematics? Such a qualification seems to be of a subjective character; the opinions of specialists may differ. And furthermore, how can there be a connection between formal, emotionless, strict logical, "cold" truth, where there is only true or false, and judgments on quality? They seem to be of a quite different character.

With this question we are back again at the problem of the nature of mathematics, a question which was stated on several places in the preceding part. Here just some more remarks.

Oxtoby writes in a review [61]: "In mathematics a real classic seldom dies, nor does it just fade away. It is more likely to be enhanced by a later one than superseded by it". The journal from which I quote this passage, *The mathematical intelligencer,* regularly contains contributions about this subject.

In 1950 Wilder wrote about the problem what mathematics constitutes in a paper entitled "The cultural basis of mathematics". I quote the following passage ([62], see also [38]):

"Let us look for a few minutes at the history of mathematics. I confess I know very little about it, since I am not a historian. I should think, however, that in writing a history of mathematics the historian would be constantly faced with the question of *what sort of material to include.* In order to make a clearer case, let us suppose that a hypothetical person, A, sets out to write a *complete* history, desiring to include all available material on the "history of mathematics". Obviously, he will have to accept some material and reject other material. It seems clear that his criterion for choice must be based on knowledge of what constitutes mathematics. If by this we mean a *definition* of mathematics, of course his task is hopeless. Many definitions have been given, but non has been chosen; judging by their number, it used to be accepted of every self-respecting mathematician that he would leave a definition of mathematics to posterity. Consequently our hypothetical mathematician A will be guided, I imagine, by what is *called* "mathematics" in his culture, both in existing (previously written) histories and in works *called* "mathematical", as well as by what sort of things people who are *called* "mathematicians" publish. He will, then, recognize what we have already stated, that mathematics is a certain part of his culture, and will be guided thereby".

Wilder illustrates this by giving several examples, for instance Chinese-Japanese mathematics, Greek mathematics, all kinds of mathematics in other cultures than ours. I already mentioned this aspect in my reflections on freedom of creation (see p. 50). Interesting reading in this context is Wilder's paper *The nature of mathematical proof* [63].

Notes

1.a. A.F. MONNA, *L'algébrisation de la mathématique; réflexions historiques.* Communications of the Mathematical Institute, Rijksuniversiteit Utrecht, 6 - 1977.

1.b. A.F. MONNA, *Evolution de problèmes d'existence en analyse; essais historiques.* Communications of the Mathematical Institute, Rijksuniversiteit Utrecht, 9 - 1979.

1.c. A.F. MONNA, *Evolutions en mathématique.* Communications of the Mathematical Institute, Rijksuniversiteit Utrecht, 14 - 1981.

1.d. A.F. MONNA, "Where does the development of mathematics lead to?". *Nieuw archief voor wiskunde*, series 4, 1 (1983) 33-56.

1.e. A.F. MONNA, "Algebraic and set theoretic aspects of the evolution of mathematics". *Proceedings of the Koninklijke Nederlandse Akademie van Wetenschappen, series A, Mathematical sciences*, 86 (1983) 329-341.

1.f. A.F. MONNA, *Methods, concepts and ideas in mathematics: aspects of an evolution.* CWI Tract 23. Amsterdam 1986.

1.g. A.F. MONNA, "Towards theoretical history of mathematics. An essay about mathematics". *Nieuw archief voor wiskunde*, series 4, 6 (1988) 211-225.

1.h. A.F. MONNA, *Op de grens van twee werelden. Ervaringen met wiskunde in een overgangstijdperk.* Center for Mathematics and Computer Science. Amsterdam 1989.

2. J. CASSINET, *L'axiome du choix avant l'article de E. Zermelo de 1904.* Séminaire de Philosophie et Mathématique, Ecole Normale Supérieure. Paris 1982.

3. A.F. MONNA, *Functional analysis in historical perspective.* Utrecht/New York 1973.

4. A.F. MONNA, *Analyse non-archimédienne.* Berlin, etc. 1970.

5. In unpublished notes D. van Dalen made some remarks of philosophical and historical character on the relations between the discrete, for instance the natural numbers, and the continuous, lines, circles, etc., and the attempts to lay a bridge over the gap between these areas.

6. YU. I. MANIN, *Mathematics and physics.* Boston/Basel 1981.

7. What is then the deeper reason for the fundamental role that the number-continuum plays in all mathematics?

8. One finds something of this idea of freedom in the following passage from a paper by E. Borel, where he considers by way of example the theory of algebraic functions of two variables: "On voit bien nettement quand on considère ces parties de la science encore en formation, à quel point la

logique pure y est impuissante. Ce qu'il faut, c'est une idée heureuse, c'est l'introduction de telle notion qui permettra de grouper les faits connus et ensuite d'en découvrir de nouveaux... L'invention proprement dite, l'invention vraiment féconde consiste, en mathématiques comme dans les autres sciences, dans la découverte d'un point de vue nouveau pour classer et interpréter les faits". E. BOREL, "Logique et intuition en mathématique". *Revue de métaphysique et de morale* 15 (1907) 273-283. Also in: *Oeuvres de Emile Borel*, 4 (Paris 1972) 2089. I agree with Borel that a happy idea is needed, but I do not agree with the last sentence. Just through new and good ideas, resulting from freedom, new creations, not yet existing facts, come to life. It concerns not only classification of known facts. In [1b] I gave examples from analysis which came to life after the happy ideas of Cantor.

9. W. HEISENBERG, *Fysica in perspectief.* Utrecht 1974.
10. L. YOUNG, *Mathematicians and their times: history and mathematics of history.* Amsterdam, etc. 1981.
11. H.J.M. BOS, "On the representation of curves in Descartes' Géométrie". *Archive for history of exact sciences* 24 (1981) 295-338.
12. A.F. MONNA, "The concept of function in the 19th and 20th centuries, in particular with regard to the discussions between Baire, Borel and Lebesgue". *Archive for history of exact sciences* 9 (1972) 57-84.
13. D. VAN DALEN and A.F. MONNA, *Sets and integration. An outline of the development.* Groningen 1972.
14. E.J. DIJKSTERHUIS, *De elementen van Euclides*, I, II. Groningen 1929.
15. F. ENRIQUES, *Fragen der Elementargeometrie*, I, II. Leipzig 1907-1911.
16. O. BECKER, *Grundlagen der Mathematik in geschichtlicher Entwicklung.* 2. Auflage Freiburg 1963.
17. D. HILBERT, *Grundlagen der Geometrie.* Leipzig 1899, cited after the edition Leipzig/Berlin 1923.
18. With regard to the method of implicit definition I refer to H. HANKEL: *Theorie der complexen Zahlensysteme insbesondere der gemeinen imaginären Zahlen und der Hamilton'schen Quaternionen nebst ihrer geometrischen Darstellung* (Leipzig 1867). In this book he introduced a formal method for extending the system of natural numbers to the entire numbers and rational numbers, called the "Principle of permanency of the formal laws". This is a kind of implicit definition. The method is much disputed - even not accepted - but, considered in the historical context, I think it was a new, original and interesting *idea.* See A.F. MONNA, "Hermann Hankel". *Nieuw archief voor wiskunde,* series 3, 21 (1973) 64-87.
19. B. RIEMANN, *Über die Darstellbarkeit einer Function durch eine trigonometrische Reihe.* Habilitationsschrift Göttingen. See: *Gesammelte mathematische Werke und wissenschaftlicher Nachlass.* Leipzig 1876. See [13].
20. H.J.E. BETH, *Inleiding in de niet-euclidische meetkunde op historischen grondslag.* Groningen 1929.
    WOLFGANG AND JOHANN BOLYAI, *Geometrische Untersuchungen. Erster*

Teil: Leben und Schriften der beiden Bolyai; Zweiter Teil: Stücke aus den Schriften der beiden Bolyai. Urkunden zur Geschichte der nichteuklidischen Geometrie, 2. Leipzig, etc. 1913.

21. J.-V. PONCELET, *Traité des propriétés projectives des figures*, I, II. Paris 1865-1866.
22. See: H. HOCHSTADT, "Eduard Helly, father of the Hahn-Banach theorem". *The mathematical intelligencer* 2, nr. 3 (1980) 123; A.F. MONNA, "Hahn-Banach-Helly". *The mathematical intelligencer* 2, nr. 4 (1980) 158.
23. A.F. MONNA, "Volterra et les fonctions de lignes; un centenaire". *Nieuw archief voor wiskunde*, series 3, 30 (1982) 247-257.
24. S. BANACH, *Théorie des opérations linéaires.* Warszawa 1932.
25. It is interesting to quote a most curious passage from the book *Paul Ehrenfest* by M.J. KLEIN (Amsterdam 1970). In a course of 1914 on theoretical mechanics Ehrenfest discussed the analogy between integral equation and systems of linear algebraic equations. Klein then writes: "Ehrenfest had learned this method from Hilbert, but it was far from common knowledge that time; "Hilbert space" had not yet become fashionable" (p. 208).
26. The history of algebraic vectorspaces is difficult to explain. Grassmann's treatises were very difficult reading and therefore it is not so strange that they were long forgotten. But Peano's work was certainly not difficult. What is the reason that vectorspaces found their place not earlier than in the twenties, when abstract algebra had long been introduced? What is the reason that this result of the Italian school did not get the interest it deserved, while, on the other hand, the results on algebraic geometry soon found their way? It is remarkable that the work of Bettazzi, preceding the axiom of choice in the Italian school (see [2]), also got little interest. See a paper by BOTTAZINI in: *Social history of nineteenth century mathematics*, H. Mehrtens, H. Bos, I. Schneider eds. Boston 1981. See also the review by J. DIESTEL of Banach's book in *The mathematical intelligencer* 4, nr. 1 (1982) 45 and A.F. MONNA, "Letter to the editor". *The mathematical intelligencer* 5, nr. 1 (1983) 6.
27. J. VÄISÄLÄ, *Lectures on n-dimensional quasiconformal mappings.* Berlin, etc. 1973.
28. T. CARLEMAN, *Les fonctions quasi-analytiques.* Paris 1926.
    J.A. SHOHAT and J.D. TAMARKIN, *The problem of moments.* New York 1943.
    J. HADAMARD, *Le problème de Cauchie et les équations aux dérivées partielles linéaires hyperboliques: leçons professées à l'université Yale.* Paris 1932.
    A.F. MONNA, "Problème des moments et fonctions quasi-analytiques". *Nieuw archief voor wiskunde*, series 3, 17 (1969) 189-199.
    For a functional-analytic approach see: A.F. MONNA, "Classes quasi-analytiques". *Nieuw archief voor wiskunde*, series 3, 20 (1972) 138-142; "Note supplémentaire". *Nieuw archief voor wiskunde*, series 3, 21 (1973) 43.

29. The following passages from F. KLEIN *(Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert.* Reprint Berlin 1979) may be of interest with respect to these problems. Treating the history of the theory of elliptic functions, he writes (p. 106): "Das nun folgende Jahr 1828 ist eine Epoche angestrengtester Konkurrenz von Abel und Jacobi um den Ausbau der Theorie der elliptischen Funktionen". And about Abel: "Abel bewältigt mit grösster Genialität die allgemeinsten Probleme; die mathematische Idee ist das bei ihm wirksame Element". In the years around 1880 Klein and Poincaré both worked on the theory of automorphic functions. Klein writes (p. 380): "In der Tat gelang es mir wieder, Poincaré um ein kleines zuvorzukommen". Apparently both had some idea about the kind of theorems that should be proved and the way to follow. For me this is a remarkable psychological fact. I believe any active mathematician, working on a theory, knows the fact that sometimes he has the conviction that a certain theorem must be true, although he cannot find the proof.
30. In 1983 I read that J.R. Strooker was writing a book *Homological conjectures.* See: L. VAN DEN DRIES, *Reducing to prime characteristic by means of Artin approximation and constructible properties, and applied to Hochster algebras.* Communications of the Mathematical Institute, Rijksuniversiteit Utrecht, 16 - 1983. I quote from the Preface by J.R. STROOKER: "I felt that there must be some "meta" reason why this should work".
31. P.M. VAN HIELE, *De problematiek van het inzicht, gedemonstreerd aan het inzicht van schoolkinderen in meetkundeleerstof.* Thesis Utrecht, supervised by H. Freudenthal. Amsterdam, etc. 1957.
32. D. VAN HIELE-GELDOF, *De didaktiek van de meetkunde in de eerste klas van het V.H.M.O.* Thesis Utrecht, supervised by M.J. Langeveld. Amsterdam, etc. 1957.
33. (F. Goffree, ed.), *"Ik was wiskundeleraar".* Stichting voor de Leerplanontwikkeling. Enschede 1985. See the contribution by P.M. VAN HIELE, p. 101-136, especially p. 107.
34. Malebranche, an adept of Descartes, stated this in the following words: "L'objet des mathématiques pures, c'est la grandeur en général, qui comprend: 1. les nombres nombrants avec leurs propriétés, 2. l'étendue intelligible avec toutes les lignes et les figures qu'on y peut découvrir". See BRUNSCHVICQ [56], p. 131.
35. Historically interesting are chapters in H. POINCARÉ, *Dernières pensées* (Paris 1913), in particular the chapter "Pourquoi l'espace a trois dimensions". Even non-mathematicians have written reflections about aspects of the dimension of space (see MAURICE MAETERLINCK, *La vie de l'espace* (Paris 1928)). They are all of a philosophical-mystical character, but there are references to great mathematicians, geometers as well as philosophers.
36. H.J.M. BOS, "Elementen van de wiskunde - ze zijn niet meer wat ze vroeger waren". *Nieuw archief voor wiskunde*, series 4, 1 (1983) 101-132.
37. See the thesis of S.B. ENGELSMAN, *Families of curves and the origins of*

partial differentiation. Utrecht 1982; Amsterdam 1984. I wonder whether there are activities of this kind in any other science.

38. See for instance: R.L. WILDER, *Mathematics as a cultural system.* Oxford, etc. 1981. In this book mathematics is placed in the framework of culture. I do not find an answer here on my questions. All the more the author writes: "The question of what constitutes mathematics has never been satisfactorily settled" (p. 77). On another place Wilder gave as his opinion that it is hopeless to look for a definition of what mathematics in essence constitutes, embracing all what has been performed (Appendix, p. 69). Saunders MacLane posed the problem to give a characterization of algebra and tried several times to find an answer, but none of his attempts were successful [1f].

    I refer to *Mathesis en Mystiek* by G. MANNOURY I mentioned before. The title already can be seen as a measure for the uncertainty about the nature of mathematics.

    See: D. VAN DALEN, *Filosofische grondslagen van de wiskunde.* Assen, etc. 1978.

    My impression is that what is written about this theme is mostly rather vague: "unity", "culture", etc. See for instance: "An interview with Michael Atiyah". *The mathematical intelligencer* 6, nr. 1 (1984) 9-19. Historically interesting are the contributions by several mathematicians-philosophers in *L'oeuvre de Louis Couturat (1868-1914) - ... de Leibniz à Russell.* Ecole Normale Supérieure. Paris 1983. The papers are of a philosophical character.
39. G. KÖTHE, *Topologische lineare Räume.* Berlin, etc. 1961.
40. D. GALE, "The truth and nothing but the truth". *The mathematical intelligencer* 11, nr. 3 (1989) 62-67.
41. In [16], p. 94 BECKER mentions that already in Antiquity the notions of abstract existence respectively effective existence are found.
42. H.C. KENNEDY, *Peano, life and works of Giuseppe Peano.* Dordrecht 1980.
43. *Selected works of Giuseppe Peano*, translated and edited by H.C. Kennedy. London 1973.
44. S. SAKS, *Theory of the integral.* Warszawa 1937.
45. *Penser les mathématiques.* Séminaire de philosophie et mathématiques de l'Ecole Normale Supérieure. Paris 1982.
46. Dieudonné refers to the results of Cohen concerning the continuum hypothesis, concluding that there are as many mathematics as one wants, dependent on the place ascribed to the continuum in the sequence of the alephs.
47. *Bulletin of the American Mathematical Society*, new series 10 (1984) 136.
48. *Notices of the American Mathematical Society* 35 (1988) 1480-1482.
49. *The mathematical intelligencer* 10, nr. 4 (1988) 53-58.
50. *Notices of the American Mathematical Society* 36 (1989) 674.
51. a. *Actes du Colloque sur "Les mathématiques et la réalite" (17-18 mai 1974). Centre Universitaire de Luxembourg*, published in: *Dialectica, revue internationale de philosophie de la connaissance* 29 (1975) 3-84.

b. *Langage et pensée mathématiques. Actes du Colloque international organisé au Centre Universitaire de Luxembourg (9, 10, 11 juin 1976).*
c. D. RUELLE, "Is our mathematics natural? The case of equilibrium statistical mechanics". *Bulletin of the American Mathematical Society*, new series 19 (1988) 259-268.

52. H. FREUDENTHAL, "Initiation into geometry". *Report of a conference on mathematical education in South Asia, held at the Tata Insitute of Fundamental Research, Bombay on 22-28 February 1956. The Mathematics Student* 24 (1956) 83-97.
53. BENEDICTUS DE SPINOZA, *Ethica, voorafgegaan door het vertoog over de zuivering des verstands.* Uit het Latijn vertaald, ingeleid en toegelicht door Dr. Nico van Suchtelen. Derde, herziene druk, Amsterdam/Antwerpen 1952. On his mathematical method see also the Preface by LODEWIJK MEYER in SPINOZA'S *Ren. des Cartes principiorum philosophiae pars I & II. More geometrico demonstratae.* Amsterdam 1663 (*The collected works of Spinoza*, E. Curley, ed. (Princeton 1985) 224-230). For Descartes' *Discours de la méthode* and his *Géometrie* I refer to the thesis of J.A. VAN MAANEN, *Facets of seventeenth century mathematics in the Netherlands.* Utrecht 1987.
54. See also on this subject: W.M. PRIESTLEY, "Mathematics and Poetry: How wide the gap?". *The mathematical intelligencer* 12, nr. 1 (1990) 14-19 (with many references) and the reactions in nr. 3, 4-6.
55. *Bewijzen in de wiskunde*, P.W.H. LEMMENS, ed. CWI Tract 24. Amsterdam 1989. This book contains: 1. A.F. MONNA, "Bewijzen in de wiskunde, historische beelden"; 2. D. VAN DALEN, "Bewijzen, waarom en hoe"; 3. N.G. DE BRUIJN, "Machinale verificatie van redeneringen. Een beschrijving van het Automath project".
56. P. BOUTROUX, *L'idéal scientifique des mathématiciens dans l'antiquité et dans les temps modernes.* Paris 1920. This is a very interesting book which contains many stimulating ideas for further study in the domain of this essay. See: L. BRUNSCHVICQ, *Les étapes de la philosophie mathématique.* Paris 1912.
57. H.J.M. BOS, *Arguments on motivation in the rise and decline of a mathematical theory; the "construction of equations", 1637-ca. 1750.* Preprint no. 246, Department of Mathematics, University Utrecht, 1982.
58. *Bulletin of the American Mathematical Society* 82 (1976) 836.
59. With respect to the penetration of algebraical concepts in geometry I mention: W. SZMIELEW, *From affine to Euclidean geometry; an axiomatic approach.* Dordrecht 1983. In this book one finds a combination of algebraical and geometrical methods. Algebraical equivalents of geometrical results and geometrical interpretation of algebraical concepts are given.
60. See the essay of BOS [57], where an example of the decline of a theory is treated. An example in invariant theory is treated in a paper by KAREN V.H. PARSHALL, "The one-hundredth anniversary of the death of invariant theory". *The mathematical intelligencer* 12, nr. 4 (1990) 10-16.
61. *The mathematical intelligencer* 3, nr. 3 (1981) 140.

62. R.L. WILDER, "The cultural basis of mathematics". *Proceedings of the International Congress of Mathematicians* 1 (1950) 258-271.
63. R.L. WILDER, "The nature of mathematical proof". *American mathematical monthly* 51 (1944) 309-323.

Index

CWI TRACTS

1 D.H.J. Epema. *Surfaces with canonical hyperplane sections.* 1984.

2 J.J. Dijkstra. *Fake topological Hilbert spaces and characterizations of dimension in terms of negligibility.* 1984.

3 A.J. van der Schaft. *System theoretic descriptions of physical systems.* 1984.

4 J. Koene. *Minimal cost flow in processing networks, a primal approach.* 1984.

5 B. Hoogenboom. *Intertwining functions on compact Lie groups.* 1984.

6 A.P.W. Böhm. *Dataflow computation.* 1984.

7 A. Blokhuis. *Few-distance sets.* 1984.

8 M.H. van Hoorn. *Algorithms and approximations for queueing systems.* 1984.

9 C.P.J. Koymans. *Models of the lambda calculus.* 1984.

10 C.G. van der Laan, N.M. Temme. *Calculation of special functions: the gamma function, the exponential integrals and error-like functions.* 1984.

11 N.M. van Dijk. *Controlled Markov processes; time-discretization.* 1984.

12 W.H. Hundsdorfer. *The numerical solution of nonlinear stiff initial value problems: an analysis of one step methods.* 1985.

13 D. Grune. *On the design of ALEPH.* 1985.

14 J.G.F. Thiemann. *Analytic spaces and dynamic programming: a measure theoretic approach.* 1985.

15 F.J. van der Linden. *Euclidean rings with two infinite primes.* 1985.

16 R.J.P. Groothuizen. *Mixed elliptic-hyperbolic partial differential operators: a case-study in Fourier integral operators.* 1985.

17 H.M.M. ten Eikelder. *Symmetries for dynamical and Hamiltonian systems.* 1985.

18 A.D.M. Kester. *Some large deviation results in statistics.* 1985.

19 T.M.V. Janssen. *Foundations and applications of Montague grammar, part 1: Philosophy, framework, computer science.* 1986.

20 B.F. Schriever. *Order dependence.* 1986.

21 D.P. van der Vecht. *Inequalities for stopped Brownian motion.* 1986.

22 J.C.S.P. van der Woude. *Topological dynamix.* 1986.

23 A.F. Monna. *Methods, concepts and ideas in mathematics: aspects of an evolution.* 1986.

24 J.C.M. Baeten. *Filters and ultrafilters over definable subsets of admissible ordinals.* 1986.

25 A.W.J. Kolen. *Tree network and planar rectilinear location theory.* 1986.

26 A.H. Veen. *The misconstrued semicolon: Reconciling imperative languages and dataflow machines.* 1986.

27 A.J.M. van Engelen. *Homogeneous zero-dimensional absolute Borel sets.* 1986.

28 T.M.V. Janssen. *Foundations and applications of Montague grammar, part 2: Applications to natural language.* 1986.

29 H.L. Trentelman. *Almost invariant subspaces and high gain feedback.* 1986.

30 A.G. de Kok. *Production-inventory control models: approximations and algorithms.* 1987.

31 E.E.M. van Berkum. *Optimal paired comparison designs for factorial experiments.* 1987.

32 J.H.J. Einmahl. *Multivariate empirical processes.* 1987.

33 O.J. Vrieze. *Stochastic games with finite state and action spaces.* 1987.

34 P.H.M. Kersten. *Infinitesimal symmetries: a computational approach.* 1987.

35 M.L. Eaton. *Lectures on topics in probability inequalities.* 1987.

36 A.H.P. van der Burgh, R.M.M. Mattheij (eds.). *Proceedings of the first international conference on industrial and applied mathematics (ICIAM 87).* 1987.

37 L. Stougie. *Design and analysis of algorithms for stochastic integer programming.* 1987.

38 J.B.G. Frenk. *On Banach algebras, renewal measures and regenerative processes.* 1987.

39 H.J.M. Peters, O.J. Vrieze (eds.). *Surveys in game theory and related topics.* 1987.

40 J.L. Geluk, L. de Haan. *Regular variation, extensions and Tauberian theorems.* 1987.

41 Sape J. Mullender (ed.). *The Amoeba distributed operating system: Selected papers 1984-1987.* 1987.

42 P.R.J. Asveld, A. Nijholt (eds.). *Essays on concepts, formalisms, and tools.* 1987.

43 H.L. Bodlaender. *Distributed computing: structure and complexity.* 1987.

44 A.W. van der Vaart. *Statistical estimation in large parameter spaces.* 1988.

45 S.A. van de Geer. *Regression analysis and empirical processes.* 1988.

46 S.P. Spekreijse. *Multigrid solution of the steady Euler equations.* 1988.

47 J.B. Dijkstra. *Analysis of means in some non-standard situations.* 1988.

48 F.C. Drost. *Asymptotics for generalized chi-square goodness-of-fit tests.* 1988.

49 F.W. Wubs. *Numerical solution of the shallow-water equations.* 1988.

50 F. de Kerf. *Asymptotic analysis of a class of perturbed Korteweg-de Vries initial value problems.* 1988.

51 P.J.M. van Laarhoven. *Theoretical and computational aspects of simulated annealing.* 1988.

52 P.M. van Loon. *Continuous decoupling transformations for linear boundary value problems.* 1988.

53 K.C.P. Machielsen. *Numerical solution of optimal control problems with state constraints by sequential quadratic programming in function space.* 1988.

54 L.C.R.J. Willenborg. *Computational aspects of survey data processing.* 1988.

55 G.J. van der Steen. *A program generator for recognition, parsing and transduction with syntactic patterns.* 1988.

56 J.C. Ebergen. *Translating programs into delay-insensitive circuits.* 1989.

57 S.M. Verduyn Lunel. *Exponential type calculus for linear delay equations.* 1989.

58 M.C.M. de Gunst. *A random model for plant cell population growth.* 1989.

59 D. van Dulst. *Characterizations of Banach spaces not containing* l^1. 1989.

60 H.E. de Swart. *Vacillation and predictability properties of low-order atmospheric spectral models.* 1989.

61 P. de Jong. *Central limit theorems for generalized multilinear forms.* 1989.

62 V.J. de Jong. *A specification system for statistical software.* 1989.

63 B. Hanzon. *Identifiability, recursive identification and spaces of linear dynamical systems, part I.* 1989.

64 B. Hanzon. *Identifiability, recursive identification and spaces of linear dynamical systems, part II.* 1989.

65 B.M.M. de Weger. *Algorithms for diophantine equations.* 1989.

66 A. Jung. *Cartesian closed categories of domains.* 1989.

67 J.W. Polderman. *Adaptive control & identification: Conflict or conflux?.* 1989.

68 H.J. Woerdeman. *Matrix and operator extensions.* 1989.

69 B.G. Hansen. *Monotonicity properties of infinitely divisible distributions.* 1989.

70 J.K. Lenstra, H.C. Tijms, A. Volgenant (eds.). *Twenty-five years of operations research in the Netherlands: Papers dedicated to Gijs de Leve.* 1990.

71 P.J.C. Spreij. *Counting process systems. Identification and stochastic realization.* 1990.

72 J.F. Kaashoek. *Modeling one dimensional pattern formation by anti-diffusion.* 1990.

73 A.M.H. Gerards. *Graphs and polyhedra. Binary spaces and cutting planes.* 1990.

74 B. Koren. *Multigrid and defect correction for the steady Navier-Stokes equations. Application to aerodynamics.* 1991.

75 M.W.P. Savelsbergh. *Computer aided routing.* 1992.

76 O.E. Flippo. *Stability, duality and decomposition in general mathematical programming.* 1991.

77 A.J. van Es. *Aspects of nonparametric density estimation.* 1991.

78 G.A.P. Kindervater. *Exercises in parallel combinatorial computing.* 1992.

79 J.J. Lodder. *Towards a symmetrical theory of generalized functions.* 1991.

80 S.A. Smulders. *Control of freeway traffic flow.* 1992.

81 P.H.M. America, J.J.M.M. Rutten. *A parallel object-oriented language: design and semantic foundations.* 1992.

82 F. Thuijsman. *Optimality and equilibria in stochastic games.* 1992.

83 R.J. Kooman. *Convergence properties of recurrence sequences.* 1992.

84 A.M. Cohen (ed.). *Computational aspects of Lie group representations and related topics. Proceedings of the 1990 Computational Algebra Seminar at CWI, Amsterdam.* 1991.

85 V. de Valk. *One-dependent processes.* 1992.

86 J.A. Baars, J.A.M. de Groot. *On topological and linear equivalence of certain function spaces.* 1992.

87 A.F. Monna. *The way of mathematics and mathematicians.* 1992.

MATHEMATICAL CENTRE TRACTS

1 T. van der Walt. *Fixed and almost fixed points.* 1963.

2 A.R. Bloemena. *Sampling from a graph.* 1964.

3 G. de Leve. *Generalized Markovian decision processes, part I: model and method.* 1964.

4 G. de Leve. *Generalized Markovian decision processes, part II: probabilistic background.* 1964.

5 G. de Leve, H.C. Tijms, P.J. Weeda. *Generalized Markovian decision processes, applications.* 1970.

6 M.A. Maurice. *Compact ordered spaces.* 1964.

7 W.R. van Zwet. *Convex transformations of random variables.* 1964.

8 J.A. Zonneveld. *Automatic numerical integration.* 1964.

9 P.C. Baayen. *Universal morphisms.* 1964.

10 E.M. de Jager. *Applications of distributions in mathematical physics.* 1964.

11 A.B. Paalman-de Miranda. *Topological semigroups.* 1964.

12 J.A.Th.M. van Berckel, H. Brandt Corstius, R.J. Mokken, A. van Wijngaarden. *Formal properties of newspaper Dutch.* 1965.

13 H.A. Lauwerier. *Asymptotic expansions.* 1966, out of print; replaced by MCT 54.

14 H.A. Lauwerier. *Calculus of variations in mathematical physics.* 1966.

15 R. Doornbos. *Slippage tests.* 1966.

16 J.W. de Bakker. *Formal definition of programming languages with an application to the definition of ALGOL 60.* 1967.

17 R.P. van de Riet. *Formula manipulation in ALGOL 60, part 1.* 1968.

18 R.P. van de Riet. *Formula manipulation in ALGOL 60, part 2.* 1968.

19 J. van der Slot. *Some properties related to compactness.* 1968.

20 P.J. van der Houwen. *Finite difference methods for solving partial differential equations.* 1968.

21 E. Wattel. *The compactness operator in set theory and topology.* 1968.

22 T.J. Dekker. *ALGOL 60 procedures in numerical algebra, part 1.* 1968.

23 T.J. Dekker, W. Hoffmann. *ALGOL 60 procedures in numerical algebra, part 2.* 1968.

24 J.W. de Bakker. *Recursive procedures.* 1971.

25 E.R. Paërl. *Representations of the Lorentz group and projective geometry.* 1969.

26 European Meeting 1968. *Selected statistical papers, part I.* 1968.

27 European Meeting 1968. *Selected statistical papers, part II.* 1968.

28 J. Oosterhoff. *Combination of one-sided statistical tests.* 1969.

29 J. Verhoeff. *Error detecting decimal codes.* 1969.

30 H. Brandt Corstius. *Exercises in computational linguistics.* 1970.

31 W. Molenaar. *Approximations to the Poisson, binomial and hypergeometric distribution functions.* 1970.

32 L. de Haan. *On regular variation and its application to the weak convergence of sample extremes.* 1970.

33 F.W. Steutel. *Preservation of infinite divisibility under mixing and related topics.* 1970.

34 I. Juhász, A. Verbeek, N.S. Kroonenberg. *Cardinal functions in topology.* 1971.

35 M.H. van Emden. *An analysis of complexity.* 1971.

36 J. Grasman. *On the birth of boundary layers.* 1971.

37 J.W. de Bakker, G.A. Blaauw, A.J.W. Duijvestijn, E.W. Dijkstra, P.J. van der Houwen, G.A.M. Kamsteeg-Kemper, F.E.J. Kruseman Aretz, W.L. van der Poel, J.P. Schaap-Kruseman, M.V. Wilkes, G. Zoutendijk. *MC-25 Informatica Symposium.* 1971.

38 W.A. Verloren van Themaat. *Automatic analysis of Dutch compound words.* 1972.

39 H. Bavinck. *Jacobi series and approximation.* 1972.

40 H.C. Tijms. *Analysis of (s,S) inventory models.* 1972.

41 A. Verbeek. *Superextensions of topological spaces.* 1972.

42 W. Vervaat. *Success epochs in Bernoulli trials (with applications in number theory).* 1972.

43 F.H. Ruymgaart. *Asymptotic theory of rank tests for independence.* 1973.

44 H. Bart. *Meromorphic operator valued functions.* 1973.

45 A.A. Balkema. *Monotone transformations and limit laws.* 1973.

46 R.P. van de Riet. *ABC ALGOL, a portable language for formula manipulation systems, part 1: the language.* 1973.

47 R.P. van de Riet. *ABC ALGOL, a portable language for formula manipulation systems, part 2: the compiler.* 1973.

48 F.E.J. Kruseman Aretz, P.J.W. ten Hagen, H.L. Oudshoorn. *An ALGOL 60 compiler in ALGOL 60, text of the MC-compiler for the EL-X8.* 1973.

49 H. Kok. *Connected orderable spaces.* 1974.

50 A. van Wijngaarden, B.J. Mailloux, J.E.L. Peck, C.H.A. Koster, M. Sintzoff, C.H. Lindsey, L.G.L.T. Meertens, R.G. Fisker (eds.). *Revised report on the algorithmic language ALGOL 68.* 1976.

51 A. Hordijk. *Dynamic programming and Markov potential theory.* 1974.

52 P.C. Baayen (ed.). *Topological structures.* 1974.

53 M.J. Faber. *Metrizability in generalized ordered spaces.* 1974.

54 H.A. Lauwerier. *Asymptotic analysis, part 1.* 1974.

55 M. Hall, Jr., J.H. van Lint (eds.). *Combinatorics, part 1: theory of designs, finite geometry and coding theory.* 1974.

56 M. Hall, Jr., J.H. van Lint (eds.). *Combinatorics, part 2: graph theory, foundations, partitions and combinatorial geometry.* 1974.

57 M. Hall, Jr., J.H. van Lint (eds.). *Combinatorics, part 3: combinatorial group theory.* 1974.

58 W. Albers. *Asymptotic expansions and the deficiency concept in statistics.* 1975.

59 J.L. Mijnheer. *Sample path properties of stable processes.* 1975.

60 F. Göbel. *Queueing models involving buffers.* 1975.

63 J.W. de Bakker (ed.). *Foundations of computer science.* 1975.

64 W.J. de Schipper. *Symmetric closed categories.* 1975.

65 J. de Vries. *Topological transformation groups, 1: a categorical approach.* 1975.

66 H.G.J. Pijls. *Logically convex algebras in spectral theory and eigenfunction expansions.* 1976.

68 P.P.N. de Groen. *Singularly perturbed differential operators of second order.* 1976.

69 J.K. Lenstra. *Sequencing by enumerative methods.* 1977.

70 W.P. de Roever, Jr. *Recursive program schemes: semantics and proof theory.* 1976.

71 J.A.E.E. van Nunen. *Contracting Markov decision processes.* 1976.

72 J.K.M. Jansen. *Simple periodic and non-periodic Lamé functions and their applications in the theory of conical waveguides.* 1977.

73 D.M.R. Leivant. *Absoluteness of intuitionistic logic.* 1979.

74 H.J.J. te Riele. *A theoretical and computational study of generalized aliquot sequences.* 1976.

75 A.E. Brouwer. *Treelike spaces and related connected topological spaces.* 1977.

76 M. Rem. *Associons and the closure statement.* 1976.

77 W.C.M. Kallenberg. *Asymptotic optimality of likelihood ratio tests in exponential families.* 1978.

78 E. de Jonge, A.C.M. van Rooij. *Introduction to Riesz spaces.* 1977.

79 M.C.A. van Zuijlen. *Emperical distributions and rank statistics.* 1977.

80 P.W. Hemker. *A numerical study of stiff two-point boundary problems.* 1977.

81 K.R. Apt, J.W. de Bakker (eds.). *Foundations of computer science II, part 1.* 1976.

82 K.R. Apt, J.W. de Bakker (eds.). *Foundations of computer science II, part 2.* 1976.

83 L.S. van Benthem Jutting. *Checking Landau's "Grundlagen" in the AUTOMATH system.* 1979.

84 H.L.L. Busard. *The translation of the elements of Euclid from the Arabic into Latin by Hermann of Carinthia (?), books vii-xii.* 1977.

85 J. van Mill. *Supercompactness and Wallman spaces.* 1977.

86 S.G. van der Meulen, M. Veldhorst. *Torrix I, a programming system for operations on vectors and matrices over arbitrary fields and of variable size.* 1978.

88 A. Schrijver. *Matroids and linking systems.* 1977.

89 J.W. de Roever. *Complex Fourier transformation and analytic functionals with unbounded carriers.* 1978.

90 L.P.J. Groenewegen. *Characterization of optimal strategies in dynamic games.* 1981.

91 J.M. Geysel. *Transcendence in fields of positive characteristic.* 1979.

92 P.J. Weeda. *Finite generalized Markov programming.* 1979.

93 H.C. Tijms, J. Wessels (eds.). *Markov decision theory.* 1977.

94 A. Bijlsma. *Simultaneous approximations in transcendental number theory.* 1978.

95 K.M. van Hee. *Bayesian control of Markov chains.* 1978.

96 P.M.B. Vitányi. *Lindenmayer systems: structure, languages, and growth functions.* 1980.

97 A. Federgruen. *Markovian control problems; functional equations and algorithms.* 1984.

98 R. Geel. *Singular perturbations of hyperbolic type.* 1978.

99 J.K. Lenstra, A.H.G. Rinnooy Kan, P. van Emde Boas (eds.). *Interfaces between computer science and operations research.* 1978.

100 P.C. Baayen, D. van Dulst, J. Oosterhoff (eds.). *Proceedings bicentennial congress of the Wiskundig Genootschap, part 1.* 1979.

101 P.C. Baayen, D. van Dulst, J. Oosterhoff (eds.). *Proceedings bicentennial congress of the Wiskundig Genootschap, part 2.* 1979.

102 D. van Dulst. *Reflexive and superreflexive Banach spaces.* 1978.

103 K. van Harn. *Classifying infinitely divisible distributions by functional equations.* 1978.

104 J.M. van Wouwe. *Go-spaces and generalizations of metrizability.* 1979.

105 R. Helmers. *Edgeworth expansions for linear combinations of order statistics.* 1982.

106 A. Schrijver (ed.). *Packing and covering in combinatorics.* 1979.

107 C. den Heijer. *The numerical solution of nonlinear operator equations by imbedding methods.* 1979.

108 J.W. de Bakker, J. van Leeuwen (eds.). *Foundations of computer science III, part 1.* 1979.

109 J.W. de Bakker, J. van Leeuwen (eds.). *Foundations of computer science III, part 2.* 1979.

110 J.C. van Vliet. *ALGOL 68 transput, part I: historical review and discussion of the implementation model.* 1979.

111 J.C. van Vliet. *ALGOL 68 transput, part II: an implementation model.* 1979.

112 H.C.P. Berbee. *Random walks with stationary increments and renewal theory.* 1979.

113 T.A.B. Snijders. *Asymptotic optimality theory for testing problems with restricted alternatives.* 1979.

114 A.J.E.M. Janssen. *Application of the Wigner distribution to harmonic analysis of generalized stochastic processes.* 1979.

115 P.C. Baayen, J. van Mill (eds.). *Topological structures II, part 1.* 1979.

116 P.C. Baayen, J. van Mill (eds.). *Topological structures II, part 2.* 1979.

117 P.J.M. Kallenberg. *Branching processes with continuous state space.* 1979.

118 P. Groeneboom. *Large deviations and asymptotic efficiencies.* 1980.

119 F.J. Peters. *Sparse matrices and substructures, with a novel implementation of finite element algorithms.* 1980.

120 W.P.M. de Ruyter. *On the asymptotic analysis of large-scale ocean circulation.* 1980.

121 W.H. Haemers. *Eigenvalue techniques in design and graph theory.* 1980.

122 J.C.P. Bus. *Numerical solution of systems of nonlinear equations.* 1980.

123 I. Yuhász. *Cardinal functions in topology - ten years later.* 1980.

124 R.D. Gill. *Censoring and stochastic integrals.* 1980.

125 R. Eising. *2-D systems, an algebraic approach.* 1980.

126 G. van der Hoek. *Reduction methods in nonlinear programming.* 1980.

127 J.W. Klop. *Combinatory reduction systems.* 1980.

128 A.J.J. Talman. *Variable dimension fixed point algorithms and triangulations.* 1980.

129 G. van der Laan. *Simplicial fixed point algorithms.* 1980.

130 P.J.W. ten Hagen, T. Hagen, P. Klint, H. Noot, H.J. Sint, A.H. Veen. *ILP: intermediate language for pictures.* 1980.

131 R.J.R. Back. *Correctness preserving program refinements: proof theory and applications.* 1980.

132 H.M. Mulder. *The interval function of a graph.* 1980.

133 C.A.J. Klaassen. *Statistical performance of location estimators.* 1981.

134 J.C. van Vliet, H. Wupper (eds.). *Proceedings international conference on ALGOL 68.* 1981.

135 J.A.G. Groenendijk, T.M.V. Janssen, M.J.B. Stokhof (eds.). *Formal methods in the study of language, part I.* 1981.

136 J.A.G. Groenendijk, T.M.V. Janssen, M.J.B. Stokhof (eds.). *Formal methods in the study of language, part II.* 1981.

137 J. Telgen. *Redundancy and linear programs.* 1981.

138 H.A. Lauwerier. *Mathematical models of epidemics.* 1981.

139 J. van der Wal. *Stochastic dynamic programming, successive approximations and nearly optimal strategies for Markov decision processes and Markov games.* 1981.

140 J.H. van Geldrop. *A mathematical theory of pure exchange economies without the no-critical-point hypothesis.* 1981.

141 G.E. Welters. *Abel-Jacobi isogenies for certain types of Fano threefolds.* 1981.

142 H.R. Bennett, D.J. Lutzer (eds.). *Topology and order structures, part 1.* 1981.

143 J.M. Schumacher. *Dynamic feedback in finite- and infinite-dimensional linear systems.* 1981.

144 P. Eijgenraam. *The solution of initial value problems using interval arithmetic; formulation and analysis of an algorithm.* 1981.

145 A.J. Brentjes. *Multi-dimensional continued fraction algorithms.* 1981.

146 C.V.M. van der Mee. *Semigroup and factorization methods in transport theory.* 1981.

147 H.H. Tigelaar. *Identification and informative sample size.* 1982.

148 L.C.M. Kallenberg. *Linear programming and finite Markovian control problems.* 1983.

149 C.B. Huijsmans, M.A. Kaashoek, W.A.J. Luxemburg, W.K. Vietsch (eds.). *From A to Z, proceedings of a symposium in honour of A.C. Zaanen.* 1982.

150 M. Veldhorst. *An analysis of sparse matrix storage schemes.* 1982.

151 R.J.M.M. Does. *Higher order asymptotics for simple linear rank statistics.* 1982.

152 G.F. van der Hoeven. *Projections of lawless sequences.* 1982.

153 J.P.C. Blanc. *Application of the theory of boundary value problems in the analysis of a queueing model with paired services.* 1982.

154 H.W. Lenstra, Jr., R. Tijdeman (eds.). *Computational methods in number theory, part I.* 1982.

155 H.W. Lenstra, Jr., R. Tijdeman (eds.). *Computational methods in number theory, part II.* 1982.

156 P.M.G. Apers. *Query processing and data allocation in distributed database systems.* 1983.

157 H.A.W.M. Kneppers. *The covariant classification of two-dimensional smooth commutative formal groups over an algebraically closed field of positive characteristic.* 1983.

158 J.W. de Bakker, J. van Leeuwen (eds.). *Foundations of computer science IV, distributed systems, part 1.* 1983.

159 J.W. de Bakker, J. van Leeuwen (eds.). *Foundations of computer science IV, distributed systems, part 2.* 1983.

160 A. Rezus. *Abstract AUTOMATH.* 1983.

161 G.F. Helminck. *Eisenstein series on the metaplectic group, an algebraic approach.* 1983.

162 J.J. Dik. *Tests for preference.* 1983.

163 H. Schippers. *Multiple grid methods for equations of the second kind with applications in fluid mechanics.* 1983.

164 F.A. van der Duyn Schouten. *Markov decision processes with continuous time parameter.* 1983.

165 P.C.T. van der Hoeven. *On point processes.* 1983.

166 H.B.M. Jonkers. *Abstraction, specification and implementation techniques, with an application to garbage collection.* 1983.

167 W.H.M. Zijm. *Nonnegative matrices in dynamic programming.* 1983.

168 J.H. Evertse. *Upper bounds for the numbers of solutions of diophantine equations.* 1983.

169 H.R. Bennett, D.J. Lutzer (eds.). *Topology and order structures, part 2.* 1983.