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The CWI is a research institute of the Stichting Mathematisch Centrum, which was founded on February 11, 1946, as a nonprofit institution aiming at the promotion of mathematics, computer science, and their applications. It is sponsored by the Dutch Government through the Netherlands Organization for the Advancement of Pure Research (Z.W.O.).

CWI Tract 23

Methods, concepts and ideas in mathematics: aspects of an evolution

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Centrum voor Wiskunde en Informatica
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1980 Mathematics Subject Classification: 01-02.

ISBN 90 6196 299 4

PREFACE

In this book we are concerned with aspects of the general evolution of mathematics and thus it is connected with the history. However, it is not the most common way of studying the history of mathematics. A study of the general progress, the evolution of mathematics as a whole, should well be distinguished from the history of mathematics as the science in which historical facts, developments of more or less special areas and their mutual relations are studied. Reflections on the general process of the evolution can be considered as a study of the Ideas that are behind the historical facts. It concerns the study of the underlying theoretical and structural aspects of the history of mathematics and in some way this domain can be seen as a "theory" of the history. For these reasons we prefer here the term "evolution" rather than "history". This standpoint can perhaps best be explained by saying that we look at the developments in mathematics from a point outside, observing the great lines of what has been going on inside in the course of the centuries. Not the factual developments themselves come on the first place, but the conclusions that can be drawn from them with respect to the general ideas on which the facts are based. We study trends in methods, the evolution of concepts, causes and reasons of the progress. It is one of the aims to find structures in the way of mathematics, to discover characteristic aspects in the various periods of history. In particular we consider the way of the development from what is called "classical mathematics" towards "modern mathematics". In this framework we will study the growing influence of algebra and algebraic methods, an aspect that is called the "algebraization", we will treat the evolution of the fundamental concepts of existence and existence theorems and finally we shall make some more general remarks about the evolution, for instance with regards to external influences on the developments. Some philosophical coloured aspects find a place in this framework.

It will be clear that reflections of this kind must be based on historical facts. But it should be kept in mind that when we consider factual developments in a somewhat greater detail, they are intended to illustrate structures and aspects of the evolution. Ultimately studies like these should lead to standpoints on the place and function of mathematics among sciences.

Acknowledgements

I want to express my thanks to my colleague A.W. Grootendorst from Delft for stimulating me to write this book. He has read the manuscript with a critical eye and gave me several valuable comments. I owe the composition of the Index of Mathematicians to him. I am grateful to him for his assistance in reading the proofsheets. Clearly any errors and shortcomings are my responsibility.

On my request my colleague D. van Dalen was so kind to write the section on the algebraization of logic; I am grateful for it.

Lastly my thanks go to Joke Stalpers and Jacqueline Vermey for the accurate way in which they typed the manuscript.

Utrecht/De Bilt, October 1985. A.F. Monna

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PART I THE ALGEBRAIZATION OF MATHEMATICS

INTRODUCTION

It is undisputable that the notions and the methods of modern algebra are of a continually growing importance in modern mathematics. One meets this phenomenon in nearly every part of mathematics. Group structures, vector spaces, homomorphisms,... seem to penetrate everywhere to such a degree that one can speak of an algebraization of mathematics. Several questions can then be posed. What are the reasons of this development? Is it only a mathematical fashion? If it should only be a mode a change in this phenomenon, as in any fashion, could be expected. Perhaps some waves can be observed in the application of parts of algebra. But aspects of algebraization, can be traced back to the 17th century. Such a long development can not be explained as a fashion, which is always a temporary phenomenon. Mathematics has always profited from the application of algebraic methods. Is it in some way inherent to the progress of mathematics?

These are questions which belong to the domain of philosophy of mathematics and a definite answer can scarcely be expected. We shall give examples of the phenomenon of algebraization in history. They may be useful to find the roots of this development and they may aid to understand better the way of mathematics. There may be subjective conclusions and points of view. But it is difficult to avoid them in studies of this kind.

CHAPTER 1 PRELIMINARY REMARKS ON ALGEBRAIZATION

At the international congress of mathematicians at Paris in 1900 Volterra called the 19th century the century of the theory of functions. Is it possible to give some characterization of the 20th century? In our century the great influence of modern algebra is apparent. Although there are earlier indications this trend is in particular evident since the twenties of our century; these are the years in which the explosive development of what was called " modern algebra" began its course. Clearly, this is not the only characteristic of modern mathematics. There is, for instance, also the influence of topology. Were these independent trends? Algebraization is found in geometry, in analysis and there is also something like "algebraization of algebra". One has only to have a view in modern books and papers to observe the difference with works of the "classical" period. In this chapter we shall give some first examples and there are some general remarks on the phenomenon.

In 1938 Carathéodory published a paper "Entwurf für eine Algebraisierung des Integralbegriffes" 1). This paper was followed by a book of the same author in 1956 "Mass und Integral und ihre Algebraisierung". In this theory Boolean algebras are important.

Numerous are the papers on function algebras. This is perhaps not an example of strict algebraization, but in any case methods and results of modern algebra are fundamental in this area. There are also many papers in which results of real or complex analysis are generalized to more general fields (local fields, algebraically closed fields,....).
We mention an article by S. Vasilach "Algebraic method for solving linear differential equations whose coefficients are functions of one variable" 2); one finds there references to some papers of the same author indicating the application of algebraic methods.

A conclusion in a paper of Akeman 3) is curious. The author studies a generalization of the theorem of Stone-Weierstrass for certain non-commutative algebras. There is no need to precise his result, but it is the author's conclusion which is remarkable : "This is a satisfying result because of its high algebraic content". Why is this satisfying?

References to algebraic methods are frequent in modern literature.

In a review of a book on Clifford algebras one reads : "The construction of Clifford algebras by purely algebraic means can be seen in...". 4). Perhaps this is not so curious because it concerns Clifford algebras. But in the next example there is more reason for reflection. We quote some passages from a paper of Varadarajan on the work of Harish-Chandra 5) : "Harish-Chandra's initial effort was to develop algebraic methods for the study of infinite dimensional representations of an arbitrary semisimple Lie group. He was able to obtain a close link between purely algebraic representations of the Lie algebra and the topologically significant representations of the Lie group". And : "The algebraic approach has acquired new life...". It concerns here an area related to Fourier analysis. This are only examples but merely the fact that passages like these are written, that it seems to be worthwile to make such remarks, is an indication that apparently algebra - i.e. "modern algebra"- plays a especially important role. A more careful analysis of the intrinsic meaning of such statements is scarcely necessary for such a conclusion. They illustrate that behind modern mathematics there is a world of mathematical concepts -algebraic concepts- quite different from the concepts of classical mathematics. Do they come from algebraization (and set-theoretical considerations)?

In connection with these examples there are some questions. Is it an aim of greatest value in mathematics to present the results in an algebraic form, an aim that is worthwhile to strive for? And if this would be the case, why? Is algebra -provided one can define what it is- more easy, more simple, than the traditional classical analysis? Or are the methods of algebra a better means for a good understanding of the results?

With respect to the question whether algebra should perhaps have a more easy structure, one can in defense of such a statement remark that analysis as far as concerns existence theorems consists of more complicated results than algebra (for any.... there exist... such that ...). One could observe that the theory of algebraic equations is more simple than the theory of transcendental equations, for instance the equation of Kepler $y - a \sin y = x$, $a \in \mathbb{C}$, $a \neq 0$ 6). It is difficult to compare them and to give an answer. What means "easy"?

In the following chapters we shall treat in some detail and in a more systematic way aspects of the history of the process of algebraization. This may be a contribution to a better understanding of the reasons and the real significance of the penetration of algebra and algebraic methods in mathematics. This shall be an attempt to give in some way a coherent description of the way of algebraization. But a description of the developments which we resume under this name is difficult. The phenomenon has various and sometimes opposite aspects and it is difficult to analyze them and to discover the mutual relations. We mention some of the aspects which present themselves and will be a point of discussion in historical context.

1. Algebra itself has several aspects which should be distinguished: the algebraic notation, arithmetic and arithmetization; algebra as a subdiscipline of mathematics. One should carefully distinguish between what is commonly called classical algebra and the study of sets provided with structures, called "algebras", for instance algebras of continuous functions.

2. Modern theories can lead to the idea of considering algebra mainly as a theory of structures. However there are also algorithmic aspects.

3. The role of topology in algebra (for instance the real numbers).

4. The relation between "classical" and "modern algebra"

5. The aspect of algebraic theories in relation to axiomatic theories.

6. Questions around "descriptive definitions" and "constructive definitions", a point of many discussions in the first years of our century (Borel, Lebesgue). In some way descriptive definitions seem to be connected with algebraization. How is the connection with axiomatic methods?

7. There is a historical controverse between "pathological" and "normal". Can methods of algebraization not be applied to pathological theories? The theory of real functions is a domain that was developed in the beginning of our century and then was a point of many discussions with respect to its pathological character.

It is difficult to discuss these aspects in an abstract sense. Therefore examples of algebraization shall be treated in the following chapters. Before proceeding in this way a remark must be made with regards to the significance of the phenomenon. When we speak of algebraization this suggests that it concerns a systematic aim of mathematicians or at leat of some of them. Certainly there seem to have been mathematicians who in parts of their work had this aim. See the preceeding remarks on Carathéodory. And perhaps there were some in older centuries (Descartes). But if we consider history from this point of view some caution is necessary. It is very well possible that from our point of view we consider certain older developments as an algebraization of the subject. But are we sure that the mathematicians who were concerned with it had also this aim? It should not be forgotten that algebraization -provided it exists- is a notion of our years. Interpretation of works of the past according to our standards, our ideas and norms, may be dangerous. The remarks we shall make in the following pages may thus be controversial because they concern a matter where opinions may differ.

CHAPTER 2 WHAT IS ALGEBRA?

Before treating algebraizations, some remarks must precede on the question what algebra is.

As concerns the history one knows that is was a development of many centuries which has lead to algebra of our years. Until the middle of the 19th century algebra consisted mainly of a theory of equations. The ordinary arithmetic of the mathematicians of the middle ages, where calculations were made with numbers, gradually grow out to a theory where letters were used. In this development the mathematicians of the East played an important role. First, theories were interpreted by means of geometrical considerations. The difficulties which were connected with it were overcome by Viète, Descartes and their successors. For literature on this point see [Novy, 1973].

Several mathematicians have written about the question what algebra of our century is. It seems that there is no common opinion. We mention the opinion of some authors.

In a lecture "Sur la relation de l'algèbre àl'analyse mathématique", given at the international congress at Rome in 1908, P. Boutroux made an interesting comparative study of algebra and analysis. Considering the history of mathematical analysis (Leibniz, Newton, Euler and their successors) Boutroux comes to the conclusion that the analyst tries to translate his results -correspondances between variables -into the language of algebra. His opinion is as follows : l'algèbre, ou science du calcul, n'est, en somme, qu'un instrument. Comme le physicien fait de la physique avec les mathématiques, l'analyste fait de l'analyse avec l'algèbre".

One is inclined to say that Boutroux gives here a characterization of algebra from the point of view of mathematical analysis. However, in his book from 1920 "L'idéal scientifique des mathématiciens" he returns to the problem in a more general framework. This book contains a study of the history of mathematics, in particular algebra and analysis, from the hellenistic period on. In the chapters "La conception synthétiste" and "Apogée, déclin de la conception synthétiste" he comes after a detailed analysis of the developments to the conclusion that pure algebra, that is disregarding its applications, consists of a method. He says that algebra "est l'art de combiner des signes litéraux (représentant des grandeurs) au moyen d'opérations connues". In the beginning there were only the operations of arithmetic (addition, multiplication etc.), but in the course of the development one studied extensions of the domain of the operations, leading to the theory of groups.

This was the opinion of the author as concerns algebra of the 18th and 19th centuries. But it seems that this is also his opinion with respect to algebra of the first years of our century (on this point the author is not very clear). Thus, in this opinion algebra would be a somewhat mechanical method, a process resembling the idea of the middle ages when algebra was the "méthode par excellence", the "ars magna" (Raymond Lulle).

It is here the place to remark that the denomination analysis for that part of mathematics which we design by that name has historical reasons. In the early years a problem was studied as follows. One designed the various quantities that appeared in the problem by letters a,b,..., x,... . The relations between these letters led to equations and thus to values of x,... . It was an algebraic method, the so called analysis of the problem. Because one did not trust such a deductive method, it was followed by a reasoning, the synthesis, which had the purpose to give an exact demonstration of the result. Observe that it is only half a century ago that at the secondary level problems in geometry were presented in this way ("let x be the object that should be found"; at this point analysis was started and finally, to obtain the definitive solution, a demonstration had to follow). Later on, the synthesis got lost and only the name analysis remained. Thus the denomination "analysis".

Let us now consider more recent studies. The book "Studies in Modern Algebra" [Albert, 1963] contains two papers of Saunders MacLane. The first "Some Recent Advances in Algebra" is the text of a lecture given by the author in 1938. The author tries to resume the state of algebra at that moment. He then poses the question: "What is algebra?". He tries the following characterization: "Algebra tends to the study of the explicit structure of postulationally defined systems closed with respect to one or more rational operations". However, he observes that in this definition not all aspects of algebra are justified, for instance topological operations in algebra and among them the theory of valuations.
In 1962 MacLane returned to the problem in a paper published in the same book, "Some additional advances in algebra". The last section concerns "The nature of algebra". As a consequence of the developments since 1938 he rejected his answer from 1938 that algebra would be a theory of the structures of systems defined by certain postulates. He mentions, for

instance, the theory of finite groups and certain algebraical systems which result from the application of algebra in geometry, topology, analysis.
But is is not the end of the story. In a more recent paper "Topology and logic as a source of algebra" (1946) MacLane posed again the question "What is algebra?", refering to his previous papers. Now his answer is: "But no formal definitions hold valid for long, since algebra and its various subfields steadily change under the influence of ideas and problems coming not just from logic and geometry, but from analysis, other parts of mathematics, and extra mathematical sources".

One may wonder what would be the sense of attempts to give a definition of such a steadily growing and changing subject. The question is more general: can an answer, valuable for all time, be given on the question what mathematics essentially is? It is perhaps better to ask what, from a philosophical point of view, mathematics constitutes, which are the elements by which it is composed, how these things have changed in the course of the evolution: definitions and concepts, analogies, problem solving, properties of various kinds, special or general, statements that are proved, deductive reasonings, development of theories around and about concepts.. . Such a study should not have the intention to give details about theories but should concern the theories themselves and the mutual relations between the various mathematical theories. Examples serve as illustrations of developments. The kind and form of the assertions can then be compared to those in other sciences, thus making more clear the position of mathematics among the sciences. The difference between these two ways of approaching the fundamental question will be clear. Algebraization is one of the tools by which the building was gradually constructed. One may perhaps say that it is one of the cornerstones in the presentation of mathematics as a unity; it is some explanation of this unity. We shall not make an attempt of such a study here. We return to algebra and algebraization.

We already made remarks on aspects of algebra. For a good understanding of the phenomenon of algebraization some more detailed information with regards to these points will be useful.

1. First about the algebraic notations and "calculation with letters". Just as ordinary arithmetic, the algebraic notations have been associated to mathematics, in particular to mathematical analysis, since a long time.

It can be said that this is the very beginning of all algebraization, although we understand by it a deeper aspect than "calculation with letters".

2. The preceding aspect is connected with an other aspect which can be called algebraic manipulation. Evidently algebra should not be identified with manipulations. The latter are found in any case where calculations are made and it is not an aspect characteristic for algebra. It is interesting to mention what Lebesgue said on this point.
In his "Notice de candidature à la Faculté des Sciences de Paris" (1918) Lebesgue gave comments on his work on "Théorie générale des fonctions de variables reélles", his main domain in the preceding years; see Lebesgue, 1972]. He observed that there is an essential difference between operations on numbers, which in his opinion belong to the domain of arithmetic and algebra, and operations on functions which belong to analysis. By way of example he considers two operations, working on a function f of the variable x:

$$F(x) = f(x)+1 \text{ and } G(x) = \int_0^x f(t)dt.$$

For calculating the value of F for a value x_0 of x it is sufficient to know $f(x_0)$, the other values of f are not necessary. It is an operation to be executed on numbers; it is an algebraic manipulation. For knowing $G(x_0)$, however, it is necessary to know all the values of f in the interval $[0,x_0]$. It is a functional operation. Lebesgue observes an essential difference between these two cases. To introduce new functions in analysis we need operations which work on functions. Without them, according to Lebesgue, one proceeds algebra with the different values of the function, but the function as object for itself is not introduced. Evidently Lebesgue mentions integration and differentiation as operations belonging to the domain of analysis. There would be some reason to say that even these functional operations belong to algebra, more exactly to an algebra, but then it is an algebra of a higher level, namely an algebra of operations on a class of functions (later on we shall make remarks on a notion of axiomatic integral). Following Lebesgue it is the finitary character that distinguishes the manipulations of algebra from those of analysis. It is the distrust in reasonings of continuity which has been an early source of "algebraization" : only finitary operations can be trusted.

In this context it is of interest to mention here a paper of Dedekind

"Über die Einführung neuer Funktionen in der Mathematik" (1854) ; see [Dedekind, 1932]. Dedekind discussed the way by which new functions and new operations are introduced in mathematics. He explains that this is done by an appropriate extension of already existing definitions, justified by the results of the theory based on the latter. With regard to analysis he considers from this point of view a formal definition of integration as inverse operation of differentiation and he gives a discussion of the evolution of this method. The comparison between analysis and the elementary operations in arithmetic which he gives is interesting. Arithmetic and analysis are considered from the same point of view.

3. We considered aspects of algebra. There is an other aspect that must be mentioned: the algorithmic methods . Algorithms were introduced from the beginning of arithmetic and algebra. There are many examples and theories in mathematics where algorithms are used. They are from history, but also from recent times. In the following we shall give examples. In a sense that perhaps is somewhat broader than is customary, we shall call algorithmic any method and theory by which results are obtained by means of in some way mechanical operations. These are rules that can be applied in certain cases in such a way that one can be sure to find the solution of a problem.

Algorithmic methods are to some extent inherent to the character of algebraic operations. But not only in algebra: there are algorithmic aspects in analysis. Why does one not find them in "modern algebra" (Bourbaki)? There are perhaps reasons : 1° algorithms depend too much on special structures, having thus a lack of generality; 2° there is some preference in studying subsets, subgroups, ideals,..., objects for which algorithms are difficult. A fortiori there are difficulties when the axiom of choice is used.

4. There is still an other important aspect of algebraization. It is connected with developments in algebra which can be called the study of structured systems.

Since the middle of the 19th century algebra developed itself in a direction which led to forms quite different from algebra as a "discipline of calculations". By the work of Grassmann, Cayley, Dedekind, Frobenius, Clifford and many others the study of structured systems became important. First there were hypercomplex numbers, groups and vector spaces in concrete sense, etc., but gradually one began to study, under the influence of Emmy Noether, Artin, and others groups, rings, fields in an axiomatic way.

In the first decades of our century this direction became dominating. One studies sets, provided with structures defined by means of systems of axioms, and the various notions which are associated with them, for instance homomorphisms, isomorphisms,... . One is interested in the set as a totality, more than in the properties of the individual elements. This has been of profound influence in all parts, also in analysis and it is a development continuing to our times. It is a highly important aspect of algebraization. These are many examples of this tendency in the history.

Speaking of "modern algebra" one should carefully distinguish between algebra as a discipline, as a totality of theories, and the notion of algebra as a set provided with a structure. In the second sense there are infinitely many algebras, with finite or infinite dimension; algebras of functions, real or complex, etc. 7). This reflects itself in the aspects of algebraization.

5. Finally we must consider the role of topology.

The standpoint to consider in algebra only finitary theories can be considered as abandoned. Infinite sets are nowadays indispensable: infinite groups and fields and even a non-constructive apparatus as Zorn's lemma are accepted. See for instance the Artin-Schreier theory of formally real fields which is incorporated in algebra. But the notion of a limit, should it be accepted in algebra? How with the theory of real numbers and the theory of p-adic numbers based on the theory of valuations? Perhaps the standpoint might be taken that for reasons of principle the theory of real numbers should not be a subject of modern algebra because it is just one of the aims of modern algebra to treat abstract and general systems. From this point of view the real and the p-adic numbers play a role in illustrating the general theory, not as a subject for itself. Think for instance on the general Galois-Theory (for finite fields etc.).

There is an other example. The theorem of Hahn-Banach on the extension of linear functionals in a vector space is fundamental in functional analysis. Thus, one should say that it belongs to the domain of analysis. However, reading the demonstration, one can observe that the theorem is valid in vectorspaces without any topology defined on them. Only properties of the real numbers and Zorn's lemma are used. The standpoint that it is an algebraic theorem can be defended. Is there an essential difference between this theorem and some properties of homomorphisms and isomorphisms in algebra and their applications? However, from the standpoint of Lebesgue I mentioned before, it should be a theorem of analysis. On the other hand, if it is considered as analysis, it is an example of the penetration of algebraic methods in analysis. It is then an example of the algebraic tendencies in modern mathematics.

All these remarks justify a historical study of the phenomenon and the roots of algebraization, whether this has been and still is a systematic aim, or is a more or less unconscious fact in the evolution. We study these aspects in the following pages.

CHAPTER 3 GEOMETRY AND ALGORITHMIC METHODS

We will consider the developments in the 17th century, in particular with regard to analytic geometry. This discipline is connected with the name of Descartes and his book "Géométrie", published in 1637 as an appendix to his "Discours de la méthode". This book is often considered as the beginning of analytic geometry. However, our analytic geometry -that is the traditional analytic geometry with axes, systems of coordinates- can not be found in this book. The value of it is in the application of algebra to classical geometric problems. Before Descartes "algebra" properly speaking did not exist. If, for example, a designed a segment of a line, a^2 was a square and could not be considered as a one dimensional quantity, a rectilinear segment. According to Descartes' theory a^2 was also a rectilinear segment. An algebraic equation in x and y signified an equation between one dimensional quantities, segments for instance and this opened the possibility of studying curves by means of algebra. The step to base this concept of a quantity on the concept of a number was only done in the middle of the 19th century (Weierstrass, Dedekind and some other).

Traditional analytic geometry -geometry with axes and coordinates- was gradually developed under the influence of Descartes' book. In this respect should be mentioned the dutch mathematicians F. van Schooten, the "Raadpensionaris" Jan de Wit and the great scholar Christiaan Huygens. By means of technical calculations, an algebraic calculation in accordance with the rules of algebra, geometrical properties could be found and purely geometrical reasonings were nearly eliminated. It concerns thus an algorithmic method as we mentioned before. This is analytic geometry as a kind of automatism; for more details see [Boutroux, 1920] and [Boyer, 1956]

It is just against this automatism for treating geometric problems that there came a reaction, even only half a centrury after Descartes' creation. The criticism came from Leibniz. This is a rather curious fact because algorithmic methods played an important role in the works of Leibniz: his work on a universal mathematics and, more philosophical, a General Science (Characteristica Universalis). Leibniz criticized the Cartesian method in so far as it was practiced by the disciples: all imagination was eliminated and there was no question of an application of algebra to geometry ; geometry was reduced to algebra.

The next passages of this critisism are interesting. In his "Projet d'un art d'inventer" he criticized the place attributed to algebra as

follows [Couturat, 1903]:
"On s'étonnera peut estre de ce que je dis icy, mais il faut sçavoir que (l'Algèbre) l'Analyse de Viete et des Cartes est plus tost l'Analyse des Nombres que des lignes: qouy qu'on y reduise la Géométrie indirectement, en tant que toutes les grandeurs peuvent estre exprimees par Nombres; mais cela oblige souvent a des grands detours, et (quelques) souvent les Geometres peuvent demonstrer en peu de mots, ce qui est for long par la voye du calcul. Et quand on a trouvé une equation, dans quelque problème difficile, il s'en faut beaucoup qu'on aye pour cela une [demonstration courte et belle] construction du problème telle qu'on desire. La voye de l'Algèbre en Geometrie est asseurée mais elle n'est pas la meilleure, et c'est comme si pour aller d'un Lieu à l'autre on voulloit toujours suivre le cours des rivières, comme un voyageur italien que j'ai connu, qui alloit toujours en batteau quand il le pouvait faire, et quoy qu'il ait 12 lieues d'Allemagne de Wurcebourg à Wertheim en suivant la riviere du Mayn, il aima mieux de prendre cette voye, que d'y aller par terre en 5 heures de temps. Mais lorsque les chemins par terre ne sont pas encor ouverts et defrichés, comme en Amerique, on est trop heureux de pouvoir se servir de la riviere: et c'est la même chose dans la Geometrie quand elle passe les Elemens; car l'imagination s'y perdroit dans la multitude des figures, si l'Algèbre ne venait a son secours jusqu'a ce qu'on etablisse une characteristique propre à la Geometrie, qui marque les situations comme l'Arithmetique marque les grandeurs"[l.c.,p.181].

This objection of Leibniz was directed towards an automatic use of algebraic methods. But it was not only the method which was criticized, there was also an objection with respect to the efficiency of these methods. In a letter to Tschirnhaus (1684) he made the following remark about Malebranche, an adept of the cartesian method:
"..., et je ne pouvais pas m'empêcher de rire, quand je voyais qu'il [Malebranche] croit l'algèbre la première et la plus sublime des sciences, et que la vérité n'est qu'un rapport d'égalité et d'inégalité,..... que l'arithmétique et l'Algèbre sont ensemble la véritable logique".

In an other letter he writes that he has shown "combien la géométrie de M. Descartes est bornée". The method was not even sufficient for analytic geometry because there are infinitely many transcendental problems which are beyond its scope. Descartes considered only algebraical equations. Now there are transcendental equations which can not be expressed in a purely algebraic form (for instance the exponential functions). Remark that the cycloidal curve belonged to mechanics. By way of example Leibniz mentions the equation $x^x + x = 30$, in which the degree itself is the unknown. It is a curious equation. Evidently 3 is a root. But are there any others? And what can be said about the equation $x^x+x = a$? Does it belong to analysis or to algebra? Or perhaps to arithmetic if the condition is put that x must be an entire number? And what if x is supposed to be a complex number?

One must look at these criticisms in the light of the fundamental creation of Leibniz (infinitesimal calculus) which furnished more powerful methods, applicable to more general curves. For more details on the controversies between Leibniz and the cartesians see [Couturat, 1961] and [Brunschvig, 1912].

REMARK. In history there is somewhat confusion in the use of the terms "algebra" and "analysis". When infinitesimal calculus and calculus with series were created one believed (in particular Newton) that this concerned an extension of algebra, thus leading to the name "algebra of the infinite" ("algèbre de l'infini"). Later this became "infinitesimal analysis" and eventually "analysis".

As soon as the method of Descartes was introduced it was further developed rapidly, taking an important place and neglecting non-algebraic methods in geometry. At the beginning of the 19th century some counter-direction was produced in the works of Monge and especially Poncelet. In his "Traité de propriétés projectives des figures" (1822), which contains his researches from 1813 on, Poncelet writes:

"C'est donc cette Géométrie particulière qu'il faut chercher actuellement à perfectionner, à généraliser, à rendre enfin indépendante de l'Analyse algébrique".

And further:

"En effet, tandis que la Géométrie analytique offre, par la marche qui lui est propre, des moyens généraux et uniformes pour procédér à la solution des questions qui se présentent, à la recherche des propriétés des figures; tandis qu'elle arrive à des résultats dont la généralité est pour ainsi dire sans bornes, l'autre procède au hasard; sa marche dépend tout à fait de la sagacité de celui qui l'emploie, et ses résultats sont, presque toujours, bornés à l'état particulier de la figure que l'on considère".

Poncelet proposed to remedy this defect of analytic geometry. Projective geometry was the result of his research. It is a synthetic geometry. One of the basic principles of this geometry was the principle of continuity. This principle allowed Poncelet to derive properties of a figure from those of an other figure (compare the later enumerative geometry). Several mathematicians continued this work of Poncelet. Especially Von Staudt studied the foundations of this new geometry. It is a remarkable fact in history that already in the first decades of the 19th century an algebraic method for treating projective geometry was developed next to the synthetic method which just originated from the desire to develop a theory independent from algebra. It is a direction especially studied by Möbius and Plücker. There are reasons to say that the algebraic method has shown to be superior to the synthetic method. The algebraic method has led to the creation of still other geometries, euclidean and non-euclidean 8). Is this the power of algebraic methods?
With respect to the synthetic direction J. Steiner must be mentioned. He was a violent representative of synthetic methods in geometry. He is known as a pure geometer with a high aversion of algebraical methods 9). For details in these developments see [Freudenthal, 1968, 1974].

Much later Poincaré made some remarks which resemble the criticism of Leibniz. He made these remarks in his Préface in the works of Laguerre (1898). He wrote as follows:
"Dans le programme d'admission à cette Ecole [l'Ecole Polytechnique], la place d'honneur appartient à la Géométrie analytique. Cette Science se renouvelait alors par une révolution en quelque sorte inverse de la réforme cartésienne. Avant Descartes, le hasard seul, ou le génie, permettait de résoudre une question géométrique; après Descartes, on a pour arriver à un résultat des règles infaillibles; pour être géomètre, il suffit d'être patient. Mais une méthode purement mécanique, qui ne demande à l'esprit d'invention aucun effort, ne peut être réellement féconde. Une nouvelle réforme était donc nécessaire: Poncelet et Chasles en furent les initiateurs. Grâce à eux, ce n'est plus ni à un hasard heureux, ni à une longue patience que nous devons demander la solution d'un problème, mais à une connaissance approfondie des faits mathématiques et de leurs rapports intimes. Les longs calculs d'autrefois sont devenus inutiles, car on peut le plus souvent en prévoir le résultat" 10).

This remark can be considered as an argument in favour of methods not using coordinates, that is direct methods. However, it should not be

correct to consider it as a refusal of the use of algebraic methods. In later developments of analysis, especially in functional analysis, direct methods appeared to be very important and it is just algebra which pushed these methods. There is, for instance, the notion of a vector space, one of the fundamental concepts of this theory which is connected with the development of algebra. But it is not the algorithmic aspect of algebra which plays a role here, it is structural aspects that are fundamental in this development. This shows once more the complexity of the phenomenon of the influence of algebra.
Note that also Lie had some reserves with regard to automatic methods; see [Monna, 1973b].

It is of interest to make at the end of this chapter a general remark on the algebraization of geometry. The algebraization of Descartes concerns a translation from geometry into the language of algebra, that means an interpretation of one theory into an other. Formerly one distinguished between the "cartesian plane" and the "euclidean plane". The theory of Descartes concerns in some way an external algebraization.
The algebraization of geometry by means of the theory of lattices (Birkhoff, Von Neumann) is to some extent more of an intrinsical character, because no internal structures are imposed on points and lines (mention couples of numbers in cartesian geometry).

In the next chapter aspects of analysis will be considered from the point of view of algebraization. It is an algebraization of an other character: it concerns internal algebraization.

CHAPTER 4 INFINITESIMAL CALCULUS AND ALGEBRA

In this chapter the relations between algebra and fundamental concepts in analysis will be considered: the notions of limit, derivative of a function and integral.

4.1 Algorithmic aspects

After the invention of infinitesimal calculus by Leibniz and Newton (the wellknown controversary standpoints will not be discussed here) and the work of the great mathematicians who continued in this domain, it was Lagrange who had the idea to give a foundation of the theory of analytic functions by means of algebraic methods, avoiding the concept of a limit; there was not yet a clear concept of this notion. In his book "Théorie des fonctions analytiques" (1779) Lagrange tried to reduce the theory of these functions to "analyse algébrique". The complete title is: "Théorie des fonctions analytiques, contenant les principes du calcul différentiel, dégagés de toute considération d'infiniments petits, ou d'évanouissans, de limites ou de fluxions,et réduits à l'analyse algébrique des quantités finies" 11). Lagrange still understood the concept of function in the classical sense: "on appelle fonction d'une ou de plusieurs quantités, toute expression de calcul dans laquelle ces quantités entrent d'une manière quelconque, mêlées ou non avec d'autres quantités qu'on regarde comme ayant des valeurs données et invariables, tandis que les quantités de la fonction peuvent recevoir toutes les valeurs possibles". Analysis was considered as an extension of the "algebra of the finite", it was "algebra of the infinite"; compare the remarks on p. 15.

Lagrange defined the derivatives of a function in an algebraic way. He started from the Taylor series and showed that any function is represented by such a series. The successive derivatives of a function f are <u>defined</u> as the coefficients of the development of f(x+h) as a power series in h. Thus, it concerns a <u>definition</u> in order to avoid the difficulties connected with a passage to the limit. There is some reason to say that this method found its continuation in the theory of Weierstrass who based his theory of the functions of a complex number on power series

however without eliminating limit problems. Note that even in our time this way is followed in algebra when it concerns the definition of the derivatives of a polynomial. See for instance in the book of Van der Waerden "Algebra".
Having thus defined the successive derivatives, the theory was then up to our time further developed by means of algebraic manipulations, that is an algebraic calculus with functions. This is an internal algebraic aspect of the elementary infinitesimal calculus, such as it was gradually developed in the course of time until our days. One starts with defining the general notion of the derivative of a function by means of a limit process and the general properties are then proved: the derivative of a sum of functions, a product and quotient of functions, the derivative of a composed function etc. The derivative of the elementary functions, exponential functions, trigonometrical functions etc. is determined by means of a limit process. And then it is only a question of a correct application of the rules to determine the derivative of a given function, at least for the functions which usually appear in the applications. There is in general no need to determine any more limits. One knows what one has to do in order to determine a derivative. Up to large extent it is an algorithm that must be applied in order to find the desired result. In fact it is algebra.

The same can be said about integral calculus. Having defined the notion of an integral one proves the properties: the additive property of the integral with respect to finite sums, substitution of a new variable and, important in practice, the relation between differentiation and integration (but remind the difficulties connected with this relation). Now, if an integral must be calculated, one starts with trying to find a primitive function of the function which must be integrated. One tries substitutions, transformations and algebraic manipulations and for a large class of functions - but not for any integrable function - this leads to the desired result. And again, it is algebra. And even if there is no primitive or if this can not be determined, an integral can sometimes be calculated by algebraic methods. For instance

$$\int_0^{\frac{\pi}{2}} \log \sin x \, dx.$$

There is an other example in the integration of differential equations. Gradually one has found classes of equations which can be integrated by means of elementary functions: linear equations, equations of Bernoulli and of Riccati etc. When there is the problem to integrate an equation, one often tries to reduce the equation to one of a well known type by means of algebraic manipulations.

All these are methods of an algorithmic character. It can be said that during two centuries infinitesimal calculus was to a large extent reduced to algebra. This changed gradually in the 19th century by the works of the great analysts (Cauchy, Weierstrass). Studying, for instance the general differential equation

$$\frac{dy}{dx} = f(x,y),$$

this is analysis. This leads to assertions of an other type. It is the domain of existence theorems. They are the subject of the next chapters. It may be said that"desalgebraization"of infinitesimal calculus came in. In our century, however, algebra has taken again an important place in analysis, but it is a kind of algebra of a character different from classical algebra.

4.2 Derivation and structures

There is a study of the French mathematician Bourlet (1879) concerning the introduction of the notion of the derivative of a function which is much more profound [Bourlet, 1879]. There are good reasons to discuss this work, which is not very well known, in more detail because there are indications in it of a new direction of research in analysis which is important for later developments. This is the tendency to detach the internal structure of theories and notions. It is connected with the structural aspects of algebra as mentioned before. It was the aim of Bourlet to give an internal analysis of the notion of derivative, he wants to give a characterization.

He considers a family F of functions, called "fonctions régulières", and studies maps T (Bourlet calls them "transmutations") $F \to F$ verifying the following conditions

$$T(u+v) = Tu + Tv, \tag{1}$$
$$T(\lambda u) = \lambda Tu, \tag{2}$$
$$T(uv) = uTv + vTu, \tag{3}$$
$$u,v \in F,\ \lambda \in \mathbb{C}.$$

Bourlet finds the general form of these maps and he succeeds in giving conditions which T must verify in order that T is identical with the classical derivative. To obtain his results he needs a condition on continuity, expressed in a certain form of convergence (in later developments in algebra an analogous notion is introduced, but then, evidently, there is no such a condition). Thus, it concerns here an analysis of the structure of the classical notion of the derivative with respect to characteristic properties and it is an attempt to represent this structure in a form as simple as possible. The philosophy is to detach the different parts of the operation "derivation", in particular to separate the algebraic aspects, expressed in the conditions (1), (2), (3), from the topologic aspects, expressed in the form of limits. In following this way, there might be some hope of the possibility of applying classical notions, in this case the derivative, to more general domains than those for which they were originally defined. There are some reasons to support this idea in the way by which Bourlet formulated his problem; Bourlet considered some general applications; see [Monna, 1974].

The later developments give the following picture. The idea of such a generalization reflects itself in modern mathematics in the concept of derivation in an algebra, evidently without any condition on continuity because there is no topology in that situation. But this development took place much later. This notion of derivation was introduced by Jacobson in 1937 in the theory of Lie algebras; see [Monna, 1974]. The definition is as follows. Let A be an algebra over a field K. A derivation D of A is a map $A \to A$ such that

$$D(x+y) = Dx + Dy,$$
$$D(ax) = aDx,$$
$$D(xy) = x \, . \, Dy + Dx \, . \, y.$$

This definition is evidently taken from analysis.

Historical note. At his time Bourlet could not give a definition of the concept of a derivation for such a general situation because the notion of an algebra, axiomatically defined, was only introduced in our century. The delay in the introduction of algebras as axiomatic systems is rather curious. This concept is connected with the definition of vector spaces. Now, already in 1888 Peano gave a definition of the concept of a vector space in a nearly modern form as an additive group with scalar multiplication. Even Grassmann had this concept in a somewhat less abstract form; he even defined multiplication of the elements (hypercomplex systems). See [Monna, 1979]. Now, the step to come to the introduction of an algebra over a field, axiomatically defined, seems not so difficult. Nevertheless, the definition was given not earlier than in the twenties of our centry. This was a very important progress for the development of analysis under the profound influence of algebra.

This form of algebraization, or the influence of algebra, differs in an essential way from the tendencies towards algorithmic methods. It concerns here attempts to detach structures, the tendency to reduce a theory to its most simple and fundamental form. The aim is a unification of theories, the tendency to discover the same structures in theories which at first seem to be very different and it is then hoped to come to a better and more profound understanding of theories. Can it be expected that these aims can be reached by reducing a theory to an algebraic form or, less rigorous, by applying algebraic methods as much as possible? An example is functional analysis which could not have existed without the influence of algebra .

These formal considerations of Bourlet concerning the concept of derivation found a continuation in analogous considerations in a paper by Drach from 1898. In the framework of algebraization this is an important work in the domain of analysis because Drach studies analogies between certain parts of algebra and the theory of functions which are defined by differential equations, but it is still more important for the formal algebraic methods he used. It is his aim to give a classification of transcendental functions which are solutions of algebraic differential equations or systems of partial differential equations.

His point of departure are the rational relations which exist between a solution and its derivatives. As a simple example he mentions the logarithmic function. This function verifies the differential equation

$$xy' = 1$$

and the relations

$$xy'' + y' = 0,$$
$$xy''' + 2y'' = 0,$$

are necessay consequences. However, with these relations it is not possible to distinguish between y and y+c, where c is an arbitrary constant. Here Drach mentions the parallels with the theory of algebraic equations and the algebraic relations with rational coefficients between an algebraic number and its conjugates. He compares this with the rational relations between the functions of a fundamental system of solutions of a differential equation or a system of equations and the transformation group which operates on these functions and their derivatives in such a way that these relations are invariant. Here is the analogy with Galois theory in algebra and this analogy is the guide of the work of Drach. He remarks that Picard (1883,1887) and Vessiot (1892) had earlier done some work of this type with respect to linear differential equations; see [Vessiot, 1892]. In his introduction Vessiot remarks that he wants to give a theory of integration of linear differential equations with analogy to the theory of Galois in the theory of algebraic equations. Vessiot and Drach refer to the theory of transformation groups of Lie which is, to some extent, the foundation of the theory (later on this theory of Lie has led to the theory of groups and algebras of Lie and the theory of algebraic groups, theories with high algebraic character). So, Drach's theory was not entirely new.

However, it is the philosophy behind the work of Drach by which it differs for reasons of principle from the theory of Picard and Vessiot; it is just this philosophy which plays a role in certain works of Lebesgue and Fréchet; that will be a point of discussion in following sections. The idea to put in the foreground the algebraic relations between a function and its derivatives suggests an algebraic method for introducing the concept of a derivative. Drach tries to characterize solutions by algebraic means and he wants to give a formal theory, guided by

algebraicanalogies. His purpose is "une étude pure logique" and he speaks if "l'intégration logique" of differential equations. This denomination has been chosen in opposition to the term "intégration géométrique", which can be used to design the problem of Cauchy, in which the transcendental nature of the solutions is not taken into account. Drach wants to formulate his theory for systems of elements which are determined by their mutual relations. The following quotation has been taken from his Chapter I:
"Nous définirons tous les éléments sur lesquels nous raisonnerons dans la suite, c'est à dire les nombres et les fonction algébriques, les différentielles et les dérivées de ces fonctions, et, d'une manière générale, les fonctions d'une ou de plusieurs variables qui vérifient des relations différentielles algébriques, par leurs liaisons avec les éléments d'un premier système, dont nous allons d'abord préciser les propriétés.
Nous supposerons que ce système satisfait aux conditions suivantes:... "

These conditions are the usual rules of composition of the theory of groups; Drach used them for introducing entire and rational numbers. Considering "éléments indéterminés", the notion of a variable element is also introduced by means of its rules of composition: 'Un élément variable x se compose avec lui-même et avec les nombres rationels suivant deux modes qui possèdent les propriétés de l'addition et de la multiplication des nombres rationnels, et ce sont là toutes ses propriétés" (l.c.p. 263).

These remarks must be seen as a program for his work.

Polynomials are introduced in a formal way. If $y = f(x)$ is a polynomial, a new variable dx -not depending on x- is introduced and then there is the formal development

$$f(x+dx) = f(x) + \frac{dx}{1} f'(x) + \frac{dx^2}{2!} f''(x) + \dots,$$

with coefficients depending on f. Drach call dx a differential. A new function of x and dx, represented by dy and called the differential of y, is defined by the identity

$$dy = f'(x)dx.$$

f' is called the first derivative of y. This process is continued to define the derivatives of higher order. All this resembles the method of

Lagrange.

Drach continues these formal considerations in a more general sense. He introduces in a formal way the concept of a differentiable function of one or more variables. Drach speaks then of an algebraic definition of the notion of derivative. Differentiable functions in this sense form the subject of this work.These are elements (functions) which satisfy the following conditions:

"1. Ils se composent entre eux et avec les polynomes à coefficients rationels suivant deux modes distinct qui possèdent les propriétés générales de l'addition et de la multiplication de ces polynomes : propriétés qui ont servi à définir les variables.

2. A chacun d'eux u, peut être associé un élément du, qu'on appelle sa différentielle, de telle sorte que l'on ait

$$d(u+v) = du + dv,$$
$$d(uv) = udv + vdu.$$

La différentielle d'une constante est nulle.
Les différentielles des variables indépendantes sont de nouvelles indépendantes.

3. Lorsque z désigne une fonction dérivable des n variables $x_1, x_2, \ldots, x_n$, on a identiquement

$$dz = a_1dx_1 + a_2dx_2 + \ldots + a_ndx_n."$$

The coefficients a_i are called the first derivatives of z and designed by $\frac{\partial z}{\partial x_i}$. There is still a fourth condition on the permutability of derivatives of higher order.

All this resembles the method of Bourlet. But there is an essential difference which will be clear from the following passage:
"Nous avons été amenés ainsi à définir d'une manière extrêmement précise tous les éléments du raisonnement : nombres, variables, fonctions, dérivées, etc. avec les moyens les plus simples et à partir de théorèmes généraux sur les fonctions dérivables pour déterminer et classer toutes les transcendantes du Calcul intégral, ou du moins celles que nous pouvons définir algébriquement".

Bourlet considers a certain family of transformations (transmutations) working on a certain class of functions and it is his aim to find those properties of these transformations which are characteristic for the ordinary derivative. He needs a condition on continuity.

Drach gives an algebraic definition of the derivative by means of certain conditions. He remarks that these conditions are not contradictory because they are verified by the polynomials. He shows that there exist functions which are differentiable in this sense but different from the polynomials. It is essential that here objects are defined by means of properties which are imposed; probably this was new at that epoch. It is a theory of an axiomatic character; Drach was led to it by algebraic analogies. In a more explicit form there are analogous considerations in the works of Lebesgue and Fréchet. Drach finds a justification for his ideas in some remarks of Weierstrass:
"Je mehr ich über die Principien der Functionentheorie nachdenke -und ich thue dies unablässig- um so fester wird meine Ueberzeugung, dass diese auf dem Fundamente algebraischer Wahrheiten aufgebaut werden muss, und dass es deshalb nicht der richtige Weg ist, wenn umgekehrt zur Begründung einfacher und fundamentaler algebraische Sätze, das "Transcendente" um mich kurz auszudrücken in Anspruch genommen wird, so bestechend auch auf den ersten Anblick z.B. die Betrachtungen sein mögen, durch welche Riemann so viele der wichtigsten Eigenschaften algebraicher Funktionen entdeckt hat". (l.c.p.254).

One may wonder, however, whether the principles which served Weierstrass as guide can be considered as an example of the tendency towards algebraization or, perhaps better, a form of axiomatization as meant here. It can scarcely be said that in the direction of Weierstrass there is the idea to detach structures by following the algebraic-axiomatic way. His preference for using algebraic methods -however without excluding the use of limits- found its base in a mistrust of the more towards geometry directed methods of Riemann, which in his view had a lack of exactness. The theory of Weierstrass can better be considered as a continuation of the ideas of Lagrange with their algorithmic aspects 12). The philosophy of Drach, with its reference to the theory of groups and analogies with the theory of Galois differs from it on essential points.

The influence of algebra is still more clear in the later developments. One has succeeded to formulate the theory of Picard-Vessiot on algebraic differential equations, as was continued by Drach, entirely in terms of algebra. The theory which resulted is called "differential algebra". The point of departure are differential fields. These are fields in which there is next to addition and multiplication a third operation, called derivation (compare Bourlet). To any element a of the field is associated an element a' of the field verifying

$$(a+b)' = a' + b'$$
$$(ab)' = ba' + ab'.$$

Any element of the field whose derivative in 0 is called a constant. There are easy examples.

Differential polynomials with coefficients in such a field are introduced. If P is such a polynomial an algebraic "differential equation" in the sense of this theory is an equation P = 0 and a solution is a set of elements -if necessary in an extension of the field- verifying this equation. In this way it is possible to apply methods of algebra, in particular the theory of Galois, to obtain properties. Parts of the theory of ordinary differential equations can be transformed in to algebra. Ritt (1950) remarks that this abstract theory is useful to detach algebraic and analytic methods. There are, for instance, relations between the Galois group of the equation and the possibility of obtaining solutions by means of quadratures. Especially Kolchin has done work in this area. See [Ritt, 1932], [Kaplansky, 1957; 1976].

For the rest there are connections of this algebraic differential theory with an older theory which are worthwhile to be mentioned here. It concerns the integration of algebraic functions. Before we already mentioned some algorithmic aspects of elementary integral calculus. For a class of algebraic functions the indefinite integral can be determined by means of methods which can be called algorithmic. The integral can then be expressed by means of elementary functions: algebraic functions, exponential functions, trigonometrical functions, logarithmic functions and their finite combinations. But it is not possible for every algebraic function. Liouville worked on this problem in the period from 1833 until 1841. His aim was to find the form of the indefinite integral of an algebraic function if this integral can be expressed in terms of elementary functions. He proved the following theorem:

Let y be an algebraic function of x; suppose that the integral of y is an elementary function.

Then

$$\int y dx = u_0(x) + c_1 \log u_1(x) + \ldots + c_n \log u_n(x),$$

where $u_1,\ldots,u_n$ are algebraic functions, c_i constants, n entire.

Liouville studied more problems of this type: integration of transcendental functions, integration of differential equations by means of quadratures. He proved that the elliptic integrals are not elementary. See [Ritt, 1948].

Just as the theory of Picard, Vessiot and Drach this theory of Liouville was further developed in an algebraic direction 13).
For differential fields analogous theorems can be proved and the theorem of Liouville appears then as a special case. The theory is reduced to algebra. There is the following theorem:

Let F be a differential field; suppose that the field of constants is algebraically closed.
Let $f \in F$ and suppose g is elementary 14) over F and $g' = f$. Then there exist $u_0,u_1,\ldots,u_n \in F$ and $c_1,c_2,\ldots,c_n \in K$ such that

$$f = u_0' + \Sigma c_i \frac{u_i'}{u_i} .$$

When this relation is "integrated" then one obtains the theorem of Liouville. For some generalizations see [Rosenlicht, 1969].

This is the way of the algebraization of analysis.

4.3 Limits and algebra

In the preceeding sections we considered the aspects of the algebraization of the concept of derivative. They can be summarized by stating that one wanted to avoid the notion of a limit, replacing it by concepts of algebra. The notion of a map came in the foreground, in particular the concept of a homomorphism, completed by some condition on products.

In this section we shall consider the historical developments around the underlying general concept of a limit.

In the 19th century an exact theory of the for analysis fundamental concept of a limit was developed (method of (ε,δ)). In the first decade of our century a formal theory of the concept of a limit was developed by Fréchet. To some extent his theory was comparable with the algebraization of the derivative. In his thesis Fréchet (1906) studied, with reference to the works of Volterra and Arzelà on "fonctions de lignes", a form of analysis in which the variables are "éléments indéterminés", illustrated by examples as curves, functions etc. Nowadays this would be expressed by saying that it concerns elements of an arbitrary set.

For the introduction of a general notion of a functional operator and a theory of such operators he was obliged to give a general definition of the notions of limit and continuity. He remarked that up to that time it was customary to give a definition for every special case, adapted to this special situation. Fréchet announced that, in order to be able to give a general theory, he wanted to follow a way which had some analogy with the road which is followed in the definition of an abstract group. There the theory is based on a composition of the elements of the group that is not explicitly given 15).

Proceeding in this way Fréchet introduced the limit as a primitive notion. He supposed that, given a set, there are certain sequences of elements of this set, to which corresponds a certain element of this set, called the "limit" of the sequence. This correspondance should satisfy some conditions:

1) To any sequence with identical elements this same element is associated as limit.

2) To any subsequence of a sequence having a limit this same limit element is associated as limit.

Later on some more axioms were added because this system of two axioms appeared to be too weak to base a theory upon them; the details shall not be given here. Sequences having such a "limit" are called "convergent sequences". It is a method avoiding the classical ε and δ. Fréchet used the following terminology: E is said to be of class ($\mathcal{L}$) when on a given set E a notion of limit is defined according to this axiomatic method. Later on he used the term "espaces-limites", "space ($\mathcal{L}$)". By means of this notion of a limit he introduced the fundamental topological notions (closed sets, compact sets). He gave a definition of metrical spaces and he proved that the limit, defined by means of the metric, satisfies the axioms. On the other hand, he gave an example showing that the axiomatic definition of a limit is not equivalent to the metrical definition.

The example is easy: Let E be the space of the real functions on [0,1] with pointwise convergence. It is impossible to define this concept of convergence by means of a metric on E. Later on there was an other example of almost everywhere convergence of measurable functions. In more recent years there are examples in the theory of distributions. But topological considerations were not the first purpose in this work. Topology was still young in those years and further developments in topological direction came later. The directive of Fréchet was an idea of unification and from that point of view should be understood the analogy with the theory of abstract groups which was his point of departure.

In principle this is an axiomatic theory. Fréchet refers to the papers of Bourlet and Drach. There is also a reference to a fundamental work of Lebesgue. In this framework there are reasons to say something about this last paper.
It concerns a book which has been very important for the development of analysis: "Leçons sur l'intégration et la recherche des fonctions primitives" (first edition 1904; second edition 1928). In this book Lebesgue proposed to give an axiomatic definition of the concept of an integral, without using, however, this axiomatic terminology. An integral is defined as a map of the set of all bounded real functions into the set of real numbers verifying certain conditions which, with the exception of one condition, are all of algebraic character (additivity of the integral etc.) It is well known that Lebesgue could not realize this program. Lebesgue called such a definition descriptive. As an example Lebesgue mentions the definition of primitive functions. Fréchet used the same method in his definition of a limit. In such a definition characteristic properties of the object one wants to define are announced. One formulates the conditions one thinks to be necessary for founding a theory and these conditions must be compatible and independent. Lebesgue writes: "Le procédé jusqu'ici toujours employé pour démontrer que ces conditions sont compatibles est le suivant: on choisit dans une classe d'êtres antérieurement définis des êtres jouissant de toutes les propriétes énoncées. Cette classe d'êtres est généralement la classe des nombres entiers; on admet que la définition descriptive de ces nombres ne contient pas de contradiction"(first ed.p.100) .

Lebesgue remarks that besides descriptive definitions there are constructive definitions, which are mostly used in analysis. For the last one must formulate the operations that have to be executed in order to obtain the subject one wants to define and it must be proved that these operations are possible. Hilbert used the descriptive method in his "Grundlagen der Geometrie". Lebesgue added an interesting note as to forms

of definition:
"C'est parce que l'on peut démontrer la compatibilité des conditions énoncées dans les définitions descriptives des premiers termes de la Géométrie à l'aide du système des nombres entiers qu'il est légitime de dire que la Géométrie peut être tout entière construite à partir de l'idée de nombre. Au point de vue de l'arithmétisation de la science, l'intérêt principal de la définition précise de l'intégrale, telle que l'a posée Cauchy, c'est qu'elle ramène les diverses notions de grandeurs qui interviennent en géométrie (aire, volume, longueur des courbes, etc.) à celle de la longueur d'un segment, c'est à dire de différence de deux nombres. Cette définition de Cauchy parachève l'oeuvre de Descartes qui, par l'emploi de coordonnées, ramenait toutes les géométries à celle de la droite".

Was the "arithmetization of science" the philosophy of Lebesgue? These last remarks belong perhaps more to the domain of axiomatization than to the domain of algebraization. But there are some formal correspondances. Comparing descriptive definitions and constructive definitions, I think the former, considered from the point of view of modern algebra, belong to the algebraic side because of their more formal structural character.

All the more, the phenomenon of axiomatization is complicated. In the second edition of Lebesgue's book there are some interesting remarks in this respect (partially they occur already in the first edition):
"L'emploi des définitions descriptives est indispensable pour les premiers termes d'une science quand on veut construire cette science d'une façon purement logique et abstraite. (...) La définition est dite alors axiomatique, parce qu'elle énumère les axiomes nécessaires. Elle se suffit ainsi à elle-même et forme un tout complet.

Au contraire, les définitions descriptives posées au cours du développement d'une théorie, la définition de l'intégrale par exemple, ne prétendent pas énumérer tous les axiomes sur lesquels elles s'appuient; elles ne forment pas un tout complet et ne sauraient être isolées de l'exposé du reste de la théorie".

Fréchet (1906) also made some remarks concerning these questions. The theory of abstract groups was developed "en s'abstenant de donner une définition générale du mode de composition, mais en recherchant les conditions communes aux définitions particulières et en ne retenant que celles qui étaient indépendantes de la nature des éléments considérés". In a slightly modified form this idea is also in descriptive definitions.

But there is a difference. In descriptive definitions one formulates characteristic properties of the objects one wants to define. However : "Au contraire, dans la théorie des groupes abstraits, le mode de composition est supposé défini à l'avance dans chaque cas particulier; mais on ignore volontairement cette définition pour ne retenir que certaines conditions générales qu'elle remplit mais qui ne la déterminent pas". (l.c. p.5).

So there are various forms of axiomatization in mathematics. Some examples may be of interest.

The axiomatization of geometry given by Hilbert and many other geometries.
In algebra there is the theory of groups, rings and fields.
In analysis there is the example of the so called axiomatic theory of harmonic functions. By means of a system of axioms a subclass of the set of all real functions is defined and the functions of this subclass are called harmonic. The differential equation of Laplace does not play any role in this definition. But these are more definitions than axioms. In this theory the influence of algebra is apparent (vector spaces, theory of sheaves). For a global résumé see [Monna, 1975].

In functional analysis the methods of algebra and axiomatization are indispensable.

Some historical remarks about descriptive definitions may be of interest. Already in 1898 E. Borel [1898] followed the descriptive method to introduce the concept of measure on a set by imposing the condition (axiom) of additivity. However, for proving the existence he followed the constructive way. In the same year Hadamard introduced the concept of area in elementary geometry as a map of elementary figures into the set of real positive numbers (see Oeuvres 4, 2179-2180). There is also the introduction of an integral as a primitive function. See [Lebesgue (1903)] 16).

From all this it appears that, as seen from the historical point of view, there are connections between algebraization, axiomatization and descriptive definitions. And for the rest, these questions are related to the fundamental problem of definitions in mathematics. What shall we understand by defining? It is a philosophical question which has been a subject of many discussions at the beginning of our century [Monna, 1972].

The remarks in the preceding pages concerned aspects of the history of the notion of limit in the most general situation where sets without any special structure are considered. The influence of algebra appears more clearly when one considers limits on sets provided with a structure. In that situation the notion of homomorphism becomes important when structural aspects -in particular linear structures- are used to define limits. There are developments of somewhat more recent times.

In the framework of his research in the domain of functional analysis Banach studied limits and integrals in the twenties from a fundamental point of view. These were applications of functional analysis in its first stage. He proved that it is possible to associate to <u>every</u> bounded sequence of real numbers a real number having the traditional properties of the limit such as it is defined in classical analysis. This number is called the "limit" of the sequence (ξ_n) and Banach used the notation

$$\operatorname*{Lim}_{n\to\infty} \xi_n ,$$

to distinguish it from the classical $\lim \xi_n$.
This "limit" verifies the following properties :

$$\operatorname*{Lim}_{n\to\infty} (a\xi_n + b\eta_n) = a \operatorname*{Lim}_{n\to\infty} \xi_n + b \operatorname*{Lim}_{n\to\infty} \eta_n , \quad a,b \in \mathbb{R} ;$$

$$\operatorname*{Lim}_{n\to\infty} \xi_n \geqslant 0 \text{ if } \xi_n \geqslant 0 \text{ for } n = 1,2,\ldots,$$

$$\operatorname*{Lim}_{n\to\infty} \xi_{n+1} = \operatorname*{Lim}_{n\to\infty} \xi_n ,$$

$$\operatorname*{Lim}_{n\to\infty} 1 = 1,$$

$$\varliminf_{n\to\infty} \xi_n \leqslant \operatorname*{Lim}_{n\to\infty} \xi_n \leqslant \varlimsup_{n\to\infty} \xi_n .$$

$\operatorname*{Lim}_{n\to\infty} \xi_n$ is called the <u>generalized limit</u> of the sequence (ξ_n), sometimes <u>the Banach limit</u> [Banach, 1932]. From these conditions follows that Lim is identical to the classical limit if this latter limit exists. The concept of a limit is here introduced as a homomorphism (with some supplementary conditions) of the additive structure of the set of all bounded sequences into the set of real numbers, the latter considered as additive ordered structure. It is an <u>algebraic limit</u>. Banach obtained this result as an application of the theorem of Hahn-Banach on the extension of linear functionals. The proof is non-constructive because the theorem of Hahn-

Banach is non-constructive. It is based on Zorn's lemma (Banach applied the theorem of Zermelo stating that every set can be well-ordered). This generalized limit is not unique because the extension according to the theorem of Hahn -Banach is not unique. By an adequate application of the extension theorem it is still possible to prescribe this generalized limit for a certain set of sequences which are not convergent in the classical sense (provided the value is chosen between $\underline{\text{Lim}}$ and $\overline{\text{Lim}}$). But this is only a weak improvement of the result. The theorem is not effective, this means that there is no constructive method for Lim. Notwithstanding these defects the theorem is an interesting example of algebraization. The problem can be posed whether there are any practical applications of this generalized limit.

The Limit can be applied to the classical theory of divergent series. It is the old problem to associate to any divergent series -this means divergent in the classical sense- a number in such a way that in calculations the series may be replaced by this "sum". It is the idea of "algebra of the infinite". This problem will be treated in some more details in chapter II, but it is of interest to make already here some remarks. The generalized limit gives the solution: it is sufficient to apply the Limit to the seqeuence of the partial sums of the series. This gives the solution for series for which this sequence is bounded. This is the algebraization of the problem from analysis. But what is the sense of this solution? Given an arbitrary divergent series there are no means to calculate this "generalized sum" or to determine approximations. And still worse : the "sum" is not uniquely determined. One may have some doubt whether this "algebraic" solution has more than theoretical significance. From this point of view the problem shall be treated in Part II where it concerns existence theorems.

There is an other application. There is a note of Banach, added to the book of Saks "Theory of the integral" (1937), where the author used the generalized limit to prove the existence of a Haar-measure on every locally compact group. But this result can also be proved without this Limit. In an analogous way von Neumann made use of it (see Collected Works II, p. 445). There are some applications to the theory of amenable groups.

The situation is rather curious. The existence of this Limit is founded on the theorem of Hahn-Banach. Now, this theorem is frequently used in analysis although it is a non-constructive result, which has not the property of uniqueness. Why are there no useful applications of the generalized limit? This leads to the following questions:

Which are the properties of the classical concept of a limit which cause that it takes such an important place in mathematics? One should think on properties that the Banach limit has not. Is the technique of ε and δ an essential aspect?

It is all the more curious because calculations with limits in analysis are usually performed in an algorithmic way.

REMARKS. There are some more recent studies on Banach limits. The set of all Banach limits of sequences has been a subject of research (see[Jerison, 1957], [Raimi, 1959]). Luxemburg (1962) proved the existence of Banach limits with the means of methods of non-standard analysis. It is also a non- constructive method. Can non-standard analysis be considered as an algebraization of classical analysis (algebraization of infinitesimals)?

4.4 Integrals

In the same way as in the case of the generalized limit Banach proved that to any bounded real function a real number can be associated, called "the integral" of the function, in such a way that the elementary properties of the classical integral are satisfied. This "integral" is identical with the Riemann-integral if this integral exists. One can even arrange that this "integral" coincides with the Lebesgue integral for L-integrable functions. This result is, just as the Limit, proved by means of the Hahn-Banach the theorem. It is an algebraic integral.
To some extent this result gives an answer on an old question posed by Lebesgue: to attach to every function an integral. For more about this subject see Part II. It is most remarkable that the Banach limit -and summability of divergent series- and the concept of an integral appear to have the same background.

What about the utility of this algebraization?
One should be inclined to think that with this result all the difficulties of integral calculus are eliminated: all functions are integrable and all is reduced to algebra. However, this is an illusion; it seems that there are no applications of this integral. One might think that this is due to the lack of constructivity and the fact that it is not possible to fix such an "integral" in a concrete way: there is no unicity. But perhaps there is a deeper reason. For this Banach-integral there is no theorem

analogous to the important convergence theorem of the Lebesgue integral (dominated convergence) for L-integrable functions. It is just this theorem which makes that the Lebesgue integral prevails over the Riemann integral, a fortiori over the Banach integral. For the generalized limit there seems not to be such a defect. Perhaps non-constructivity is not such a serious objection. The introduction of the Lebesgue integral is based on methods which can scarcely be called constructive. And all the more: what is a constructive method? Does it mean algorithmic methods? Is algebra constructive, for instance the Artin-Schreier theory on ordered fields? For constructive analysis see [Bishop, 1967].

In Part II we will return to this subject in the framework of the developments with regards to the existence of universal measures satisfying several conditions. Research has led there to the domain of the foundation of mathematics. All these problems are related to fundamental problems on existence in mathematics, which shall be treated in the Parts II and III.

From the remarks on generalized limits, derivations, integrals may be concluded that strict algebraization is an extreme standpoint and there may be doubt whether this is a good program. It is from the combination of algebra and topology that the progress is realized.
Nevertheless the principal question remains: <u>what are the deeper reasons of the tendency towards algebraization</u>?

In the next chapter we treat some special topics to illustrate the way of algebraization.

CHAPTER 5 ALGEBRAIZATION IN SPECIAL AREAS

In the foregoing algebraization was illustrated by developments concerning fundamental mathematical notions: the general concepts of a limit, derivative of a function, the concept of an integral. From these examples one may get the idea that algebraization consists mainly in the study of homomorphisms of structures. Indeed, it is an important aspect of the phenomenon. But is is not the only one. There are examples of theories in various areas of mathematics which show the importance of algebraic methods of an other character. In this section several examples will be given. The information must mostly be short. A detailed description of the developments would take too much place and, for the rest, would necessitate a profound knowledge of each of these domains. The only aim can be to show the deep influence of algebra.

5.1 Geometry

We already mentioned algorithmic aspects of geometry; they were connected with the work of Descartes and his successors. In a development of nearly three centuries this line of mathematical research has led to modern algebraic geometry. Several famous mathematicians have contributed to this development: Dedekind, Weber, Frobenius in the 19th century, and in our century Emmy Noether, Artin etc. In the first stage it was a study of algebraic curves and surfaces, studied by means of coordinate systems (Descartes). In a development of many centuries the theory grew out into an algebraic-geometric theory of an abstract character where often only the specialist in this area can recognize the classical geometric properties. Under the profound influence of the developments in algebra the theory is developed over algebraically closed fields where even figures can not be traced. The whole of modern algebra plays a role: the theory of rings, ideals, the theory of valu ations... . But the classical theories are still present, often in a hidden form: birational transformations, resolution of singularities, leading to deep theories. Is it algebra with geometric and topological notations, or is it still geometry? Or is it simply a convention how it is called? What is geometry? See a study of Dieudonné (1974).

This development followed the way from algorithmic aspects towards the introduction of structures. But there are some other aspects that must be mentioned.

First there is the classical aspect of constructions with only the ruler and the compass. Later on there were studies in which only the compass was allowed. To some extent this is an algorithmic aspect, not coming from algebra.

Then there are the investigations on the foundation of geometry. In his Grundlagen der Geometrie (1899) Hilbert studied axiomatic-structural aspects of geometry in connection with algebraic structures. In this study Hilbert used several algebraic concepts, for instance "complexe Zahlen-systeme", the concept of a field. He considered geometries over fields different from the field of real numbers and proceding in this way he could, for instance, give interpretations of certain geometric properties (Pascal, Desargues) in terms of the algebraic operations of the field (commutativity, associativity, questions on ordering, archimedean or non-archimedean). The evoloution has then led to several geometries and theories of algebraic-geometric character at a great distance from the old traditional geometry : finite geometries, geometries based on special groups of transformations (reflections), non-desarguesean geometries and several more. These geometries are very different from geometries which find their base in algorithmic methods. Descartes and his successors were concerned with properties of "classical" geometry. Now the <u>structures</u> of geometric theories are studied with the apparatus of modern algebra. The <u>foundations</u> of geometry are connected with structural aspects of algebra. Such results could not have been obtained with the old algorithmic methods. For recent results see [Szmielew, 1983].

5.2 Topology

In modern topology algebra became gradually more and more important and it has now a dominating role. Algebraic topology is then placed next to set theoretic topology. From the last decades of the 19th century on groups have taken an important position in topology. The Betti numbers of a variety were introduced, later on replaced by the Betti groups. The work of Poincaré have then led to the nowadays fundamental concepts of homology and homotopy. Set theoretic topology goes in some way back to Riemann and was afterwards in particular developed on the base of the work of Cantor. The classical book of Hausdorff "Grundzüge der Mengenlehre", edited in 1914 with several new editionsin a somewhat changed presen-

tation of the material under the new title "Mengenlehre", was a first culminating-point in this direction. Several definitions of the concept of a topological space were given. Topological structures can be defined by taking the notion of an open set as primitive notion. There is an other point of departure by taking an operation of closure $X \to \overline{X}$ as primitive concept; $\overline{X}$ designs then the closure of the set X. In this second approach the analogy with algebraic methods becomes apparent. The axiom $\overline{X \cup Y} = \overline{X} \cup \overline{Y}$, imposed on the operation of closure, can be considered as a homomorphism into the family of subsets. There is the axiom $\overline{\overline{X}} = \overline{X}$, which expresses that the operation is idempotent. Here is the connection between axiomatic topology and algebra. In the first chapter of the well-known book of Kuratowski on topology -that is set-theoretic topology- the author considers "le calcul topologique". For a long period -probably until 1940- it was customary to use algebraic notations to design the fundamental operations in the theory of sets, and therefore in topology. The union of the sets A and B was denoted by A + B, this was the addition (sum) of A and B. The intersection was called the product. Thus, Kuratowsky said that it concerned "l'Algèbre de la théorie des ensembles". These are algebraic analogies in set theoretic topology and set theory.
Considered from algebraic standpoint, algebraic topology has very different aspects with respect to the influence of algebraic concepts and methods. They are of a more intrinsic character. In algebraic topology the aspects of the theory of groups are of first importance. We mention homology and cohomology theories, homotopy theory, also in axiomatic form, methods using sequences of maps. As subject there are problems on classification of topological spaces, characterization of spaces, invariants etc. Here it is not the question of analogy, here algebraic concepts are of an intrinsic value. They are at the base and it is not only the analogy of a formal apparatus. There are reasons to defend the standpoint that algebraic topology has surpassed set theoretic topology. What are the reasons of this development? Could it be that set theoretic topology has been developed in a direction which can be called "pathologic"? All kinds of topological spaces and their properties were studied. Research in this area led to more and more detailed concepts and definitions. Special problems were posed for which specific methods were necessary not adapted to algebraic methods. The journal "Fundamenta Mathematica" contains interesting information on this point. Although results have been useful in other domains (even in logic), it seems to be a more special direction

of research which is no more very popular as it was formerly. Topological vectorspaces and algebras and their refinements are of a high value in modern mathematics, but they are already under the influence of the developments in algebra. Set theoretic topology was important until the twenties or thirties but then the algebraic direction began to take its dominating position in this area of mathematics.

Now, the same questions can be posed as three centuries ago with regards to geometry of Descartes. Considering modern algebraic topology, does it concern an application of algebra to topology or should it be said that topology is reduced to algebra? Is algebraization here a way of presentation of a theory, or should the standpoint be taken that it concerns an intrinsic aspect of the theory?

For the history of topology, in particular algebraic topology, see [Bollinger, 1972[, [Dieudonné 1977].

5.3 Lie groups and Lie algebras

The modern theory of Lie groups and Lie algebras can be considered as a classical example to demonstrate how a theory can change its form under the influence of algebraic methods. At the beginning the theory of groups of transformations of Lie belonged to the domain of differential equations. Later on the theory was developed in an algebraic direction (Lie algebras), that is to say in connection with the concepts of topology. Now it is grown out into a theory of high algebraic character: problems of classification treated by means of an algebraic apparatus, development of the theory over fields different from the field of real numbers, the theory of algebraic groups. The evolution has thus led to deeper theories. For a historical exposition see a paper of [Freudenthal (1968)]. For topological algebra, which is in the same area, see [van der Waerden, 1975].

5.4 Operational calculus

The history of the theory of operators is another interesting example of the utility of formal methods. The formal method, used by Lagrange in 1797 to define in a formal way the derivative of an analytic function, was already mentioned before.

But earlier in 1772, he had studied the algebraization of infinitesimal calculus. It is a development in the direction of operators. With a function f he associated an operator Δ, defined by

$$(\Delta f)(x) = f(x+\alpha) - f(x).$$

Lagrange developed an algebraic calculus with this operator introducing $\Delta^2, \Delta^3, \ldots$, but also Δ^{-1}, which evidently represents "integration". Then Arbogast must be mentioned. Fréchet called Arbogast "le père du calcul fonctionnel (11). Fréchet remarked that the directive in Arbogast's book "Du calculdes Dérivations" (1820) was the idea to develop a formal calculus with operators. It is observed that already Leibniz studied such formal theories. Leibniz's formula for the derivative of a product constitutes one of the first propositions of an operational calculus.

Formal methods were especially developed in England in the 19th century: Gregory, De Morgan, Boole. But in particular Heaviside must be mentioned. About 1887 he developed an important formal calculus with operators. It is a formal method to obtain solutions of differential equations. If D designs a differential operator the problem is to find solutions of the differential equation Df = g by means of methods of algebra; g is a given function. The idea is to represent the solution in the form

$$f = D^{-1}g,$$

where D^{-1} design the inverse of D.
Heaviside developed such an algorithmic method, without, however, giving a justification of this method. It is a symbolic method by means of which correct solutions can be obtained. But it was not clear what is really going on; the real signification was obscure. Some might have remarked that this does not matter if only the solutions are correct. Others felt the need to understand the method really. The situation resembles the creation of the algebraic-geometric method by Descartes and the reaction which came against these automatisms. Much later a solid foundation for this theory was developed but for this classical analysis was necessary (Laplace transformation). But evidently just the advantage of symbolic methods is get lost.

About 1950 Mikusinski returned to the theory of operators of algebraic character. In his theory the ring of complex continuous functions on $[0,\infty)$ takes an important place; the product of two functions is then defined in an appropriate way (convolution). See[Freudenthal, 1959].

The modern theory of operators is incorporated in functional analysis. In particular the theory of linear operators in Banach spaces is developed in great detail. The algebraic notion of a vector space is at the basis of the theory. Without the influence of the modern concepts of algebra functional analysis could scarcely have been created. There are, for instance, the notions of subspace, quotient space (in algebraic form), eigenvalues, spectral theory, inverse of an operator etc. It is a domain of high activity, penetrating in nearly every domain of analysis. Here the notion of a distribution (Schwartz) must be mentioned. It is a concept that can be considered, at least for the moment, as an end of the evolution of the concept of a function as analytical expression to the idea of a function as a map from one set into an other.
It should well be understood, however, that in this theory there is no strict algebraization. It is from the combination of algebra and topology that progress comes.

5.5 Logic

The case of logic is rather curious, the subject is as old as mathematics itself. But whereas mathematics developed more or less continuously through the ages, logic remained stationary untill the nineteenth century. Although no mean inventiveness was displayed in the antiquity and in the middle ages, logic was seriously hampered by a lack of notation. The impulses that mathematics received from the notational innovations in algebra did not touch logic and only in the nineteenth century Boole,c.s. Peirce,Frege, to mention the main contributors, created an artifical symbolism for logic.

The curious aspect of the development of logical symbolism is the premature introduction of algebraic notation. George Boole observed the regularities of the logical connectives 'end', 'or' and 'not', and created in his 'Laws of Thought' (1854), what now is called Boolean algebra, or the algebra of logic.

The algebraization of logic had set in, but too early! Boolean algebra, excellent as it was for the purpose of propositional logic, could not handle predicate logic. Peirce suggested a method for variable blending

operators, but his proposal did not catch on. In fact Boolean algebra hampered the development of logic, it was, at the time, a red herring, a poignant example of a premature algebraization.
The practice of the algebra of logic blurred the finer distinctions of logic or even made them invisible. E.g. syntactical matters and the problem of meaning, not to mention truth versus derivability, appeared in the light of Boolean algebra as marginal or even imaginary issues.
Boolean algebra opened up an algebraic practice in the old tradition, a theory of equations but without interesting polynomials (x.x=x and x+x=x). Schröder wrote a three volume monument "Die Algebra der Logik" (1890) that captured all the benefits that the new algebra had to offer.
In the mean time Gottlob Frege had created a formal _language for predicate_ logic (even including higher orders), [the _Begriffschrift_ (1878)]. Frege introduced a two-dimensional notation which was doomed to failure. Although its merits have been claimed by some, the notation was too wieldy to compete with the more flexible and natural notation of Peano (and later, Russell). If we ignore matters of symbolism, we must give Frege due credit for creating modern symbolic logic. In a sense the, let us say 'standard'-, formalization of predicate logic was an instance of algebraization. The usual presentation of the basic expressions of algebra as equations between polynomials, here is replaced by _equivalences_, A ⇔ B. The link between this kind of algebras and the traditional algebras was pointed out by Lindenbaum and Tarski, who associated with a propositional theory T a Boolean algebra A_T, the so-called _Lindenbaumalgebra_. The particular _algebra of logic_ -tradition surfaced after a period of neglect in Poland, where after Tarski and Mostowski's work in the thirties Boolean algebra was updated and the framework extended in order to incorporate predicate logic, cf H. Rasiova, R. Sikorski, [_The mathematics of metamathematics_, Warsaw, 1963]. In particular research since the fifties took into account the newer aspects of algebra that lifted it from a mere science of equations. Ideals, filters, ultrafilters, etc. were connected to properties of formal theories.
In the mean time Boolean algebra had become an independent discipline with connections with many fields in mathematics, and the algebra of intuitionistic logic had been introduced (Stone 1937; Tarski 1938). Nowadays it is usually called _Heyting algebra_ but terms like _Brouwerian algebra_ or _pseudo-Boolean algebra_ have also been used.

The logical step from Boolean algebra to an algebra that would reflect the nature of predicate logic was taken by Tarski, who introduced cylindrical algebras (1950), and Halmos, who introduced polyadic algebras (1955). So far these algebras have not caught on, and it remains to be seen what and how they contribute to logic or mathematics. It is safe to say that the mathematico-logical community has stayed conspicuously aloof.

Perhaps the most exciting incursion of algebra into the domain of logic is the introduction of category theory into logical theory and semantics. It had already been observed by Dana Scott that Boolean valued logic (or rather Boolean valued models) was perfectly suited for an independence proof of the continuum hypothesis (1967), but mathematics in a Boolean valued universe did not seem to hit the right note. The final generalization came from the side of category theory. W. Lawvere discovered that logic and semantics could neatly be handled in a topos, i.e. a cartesian closed category with subobject classifier (and usually a natural number object). In particular all kinds of notions from logic turned naturally up as adjoint functors. The price to pay was the loss of classical logic (and the axiom of choice), in a topos the principle of the excluded third mostly fails. Moreover, one has to consider in general partial objects. A more mundane version of categorical logic, the logic of sheaves was developed at the same time (Scott, et al.).

The development illustrates a well-known phenomenon in mathematics, a suitable generalization unifies many hitherto unconnected notions (in this case Boolean models, Kripke/Beth models, Cohen-forcing, Robinson forcing, sheave theory, Grothendieck sites, intuitionistic logic (mathematics)), and it brings out those points in the parent notions that could not be properly distinguished in the special circumstances. Also there is a certain amount of unexpected dividend for mathematics, e.g. in suitable topoi one can develop a synthetic differential geometry, much as Lie and Cartan practised it, complete with (nilpotent) infinitesimals.

Summing up: the algebraization of logic has after a light hearted youth turned into a promising and solid discipline that might very well be termed the algebra of mathematics.

5.6 The theory of numbers

It seems that there are tendencies of a development in an opposite direction in the history of the theory of numbers. It is in the nature of the problems in this area that in the beginning it was a theory of arithmetic, algebraic character. There was the classical number theory (Fermat and several others), the theory of algebraic numbers (Frobenius, Kronecker, Dedekind, Hilbert). Later on other methods for treating the problems were developed, especially in the domain of analysis (Dirichlet, followed by many other famous mathematicians). We may perhaps suppose that this was due to the fact that in those years new types of problems were formulated for which new methods appeared to be necessary. A classical example is the problem of the distribution of the prime numbers and its connections with the theory of the Zeta-function of Riemann and Riemann's hypothesis concerning the zeros of this function. It is curious to observe that later developments led to study certain functions analogous to the Zeta-function of Riemann, but now defined over some fields different from the field of the complex numbers. Apparently this development took place under the influence of the new concepts in algebra. This was the analytic theory of numbers which had a culminating point in the years of Landau, Hardy, Littlewood and other famous mathematicians.

Unless one wants to consider autonomous research on Zeta-functions, the theory of automorphic and modular functions and forms, Dirichlet series and what there is more in this domain as belonging to the area of analytic number theory -as is done in the classification of Mathematical Reviews- one may wonder whether analytic number theory still has the interest it had in these golden years. In his report "Getaltheorie 1946 - 1971" (1973) Van der Blij remarks that in these 25 years there has been not much progress in this field. Nevertheless he indicates the p-adic methods, coming from algebraic side (compare Hensel). Should we conclude that after all the methods of classical analysis were insufficient to attack these number theoretical problems? Or is it simply because there is something like a mathematical mode? We refer to a report of H.M. Stark about a book of Rademacher on analytic number theory where he writes: "Topics in analytic number theory", by Hans Rademacher covers all the classical aspects of a subject which is presently undergoing a revolution" [Stark, 1975].

5.7 The theory of functions

In this section we shall consider general aspects of the theory of functions from the point of view of algebraization.

As opposed to the examples treated in the preceding sections the aspect of algebraization is not apparent in this domain of analysis, at least does not present itself in an equally pronounced way as in the preceding examples. Should we speak of a failure of algebraization in this special area and might this be the reason of some decline?

(i) The theory of real functions

Under this title the strict theory of real functions will be considered, not real analysis in a general sense, but the theory which is now generally designed by this name (see the classification in Mathematical Reviews).

From the last decades of the 19th century until the twenties or thirties of our century it was a domain of great productivity, especially in the "Ecole Française" and the "Ecole Polonaise". This theory was realized under the influence of the theory of sets and set theoretical topology. These were the years in which many books were published in the series "Collection de monographies sur la théorie des fonctions, publiée sous la direction de M. Emile Borel", well known as the "Collection Borel". In this series a great variety of subjects in this area was published ; in Part II some more will be said about them. Many of these books, especially the books of the first years, contained an introduction to the theory of sets and topology, then just beginning their course. The theory of real functions was in close connection with these subjects (see the later books of Kuratowski on topology). In this domain special properties of the general notion of a real function were studied, for instance forms of discontinuity, existence of derivatives -left or right- properties of the set in which there exists no left or right derivative, properties of the set of the values of a function, problems on classification (Baire), topological properties of real functions etc. It is a curious fact that in these problems closed or perfect sets had a more important place than open sets, which later on came in the foreground under the influence of developments in topology. To some extent these problems can be considered as more or less pathologic. In a later stage some of these problems were again studied with the new methods of functional analysis. There are scarcely reasonings of an algebraic character. The area was somewhat isolated and the applications were scarce. In the first years of the century important mathematicians had some difficulties with this theory. Among them was E. Borel himself. He opposed against the study of such artificial objects and he considered them as "monstruosités". For information on these problems see [Monna, 1972].

From the thirties on, perhaps after World War II, one observes a decline of the research in this domain. The number of publications is rather small, compared with the situation in the first decades. In the introduction of the "Lecture Notes":"Variation Totale d'une Fonction", edited in 1974, Bruneau writes that it is his aim to restore with this book an "air de jeunesse à l'étude des fonctions d'une variable (ou plusieurs) variable(s) réelle(s) et plus précisément aux remarquables travaux considerés à ces fonctions par A.S. Besicovitch, A. Denjoy, A. Khintchine, A Kolmogoroff, N. Lusin, J. Marcinkiewics , F. Riesz, S. Saks, W. Sierpinski, etc. ...". It is an indication in the same direction.

Here two remarks should be made. The first concerns the situation around the measure theory. At first there was a close relation between this theory and the theory of real functions as described before. One has only to mention the works of Borel and Lebesgue. But in measure theory there has been great progress, partly under some influence of the theory of probability. But in modern measure theory it are just algebraic aspects which are important. There are, for instance, the Boolean algebras and the modern theory of integration, connected with functional analysis. The theory of measures and the strict theory of real functions are now at a greater distance from each other.

The second remark concerns the broader field of real analysis : differential equations, harmonic analysis, the theory of distributions etc. Just in these domains, where is high activity, the influence of algebra is important.

Now the crucial problem should be posed. What are the reasons that there is less interest in the strict theory of real functions? Is it a question of mathematical mode or are there deeper reasons? Does it concern an area that must be considered as closed, as sufficiently elaborated? There are problems connected with this area which seem to be closed. For instance the old problem of the existance of primitive functions. The solution is included in the Denjoy integral; there are also results around the Perron integral. In 1917 Carathéodory [Carathéodory, 1918] wrote already that the domain was closed after the revolution caused by Lebesgue.

There may indeed be deeper reasons for this diminishing interest. In this strict theory of real functions one has always studied special problems, often of pathological character, for which specific methods appeared to be necessary and there was not much coherence in this area. On the contrary, in modern mathematics one is more interested in structures, more or less general, to which general algebraic methods can be applied. There is less interest in the properties of the individuals for itself.

The situation is comparable with the developments around set-theoretic and algebraic topology where the latter has a dominating position. Thus, it may be that the classical theory of real functions is not so much suited to the application of algebraic methods and this might be the deeper reason of the decline.

But would it not be possible to consider the collection of these special problems from a higher point of view and to bring them on a higher level, leading to some structure in the area by means of the methods of functional analysis with its algebraic, topologic foundation? See for instance the results on the spaces of continuous functions. See also a paper of Chernoff (1975) on quasi analytic functions, which contains a new exposition on this subject. Or should it be observed that E. Borel was after all right when he wrote in his book "Méthodes et problèmes de la théorie des fonctions" [Borel, 1922, p. 146] : "Il faut donc se résigner a faire systématiquement ce que les mathématiciens ont été conduits à faire spontanément et sans esprit de système, c'est-à-dire se borner à étudier les fonctions qui se présentent naturellement, ce que nous pouvons appeler "les êtres réels et normaux", par opposition aux monstres artificiellement créés ou même simplement conçus abstraitement. La démarcation est délicate et, en certaines régions, ne serait pas actuellement à préciser; c'est néanmoins dans cette direction seulement que l'on arrivera à faire de la théorie des fonctions une discipline entièrement cohérente. Les fonctions anormales doivent être étudiées et connues dans une certaine measure, mais seulement dans la mesure nécessaire pour les exclure ou plutôt pour reconnaître qu'elles s'excluent elle-même du système cohérent ".

(ii) Analytic functions

It seems that the same question can be posed with regards to the classical theory of the analytic functions of one complex variable. In the first

decades of our century beautiful theories were developed. Many mathematicians worked in this domain: Borel, Julia, Montel, later Nevanlinna, Koebe, Bieberbach. The Collection Borel contains books in this area. One studied, for instance, the properties of entire functions, meromorphic functions, the behaviour of functions in the neighborhood of a singular point, the distribution of the values of an analytic function etc.
These results are perhaps for the greater part on the way which Weierstrass followed in this domain.

There are still many publications in this field. Nevertheless it seems that there is less interest in this field than before. The situation is quite different with respect to the approach of Riemann, but then one comes soon in the domain of Riemann surfaces with its topological consequences, problems on classification etc., with some algebraic backgrounds.

It can not be said that all these problems in the direction of Weierstrass are solved. On the contrary, there are important open problems. Are they too difficult, or is it simply that research went an other direction? Or are there too less possibilities for the application of algebraic methods? The foreword in[Proc. Seminar (1976)] is of some interest:

"The past decade has been a period of remarkable activity for complex function theory, (....). At the same time, new techniques of exceptional power continue to be developed (....). An optimist will see in these developments indications of a renascence of function theory, the achievements of which may ultimately rival the great triumphs of the past".
The authors, are they optimistic or pessimistic?

An example may be instructive. Hadamard (1892) observed that to define an analytic function means essentially to give a sequence (a_n) of complex numbers such that $\Sigma\, a_n z^n$ is not divergent for all z [Hadamard, 1892].
The problem is to determine the properties of the analytic function which is generated by this series by means of analytic continuation, that is, for instance, to determine the singular points, to calculate the value of the function in a given point in terms of the coefficients a_n. It is the central problem in the sense of Weierstrass. From the theoretical point of view this problem must have a solution, but it seems to be very difficult. Even a great analyst as Hadamard could only give the solution in some more simple cases. Later on there were some more results (the theorems of Fabry). It should be noted that this problem depends on the <u>totality</u> of the numbers a_n, not on the a_n individually. A change of a finite number of the

coefficients means the addition of a polynomial to the analytic function. This has no influence on the singular points and the influence on the value in a point is reduced to a simple calculation. Thus, it is an infinite dimensional problem. Probably special methods will be necessary for its solution. Methods of functional analysis? 17). Are there algebraic approaches to this problem? Or is it difficult just because there are no algebraic ways?.
Thus, if there may be some reasons to think that the theory of functions of a complex variable is no more a field of primary interest for mathematicians, two exceptions at least must be mentioned.

The first is the theory of algebraic functions, in particular in its algebraic presentation (Dedekind). It is connected with algebraic geometry (elliptic functions, elliptic curves). This is a domain of great activity. Is it just because of the algebraic connections?

The second is the theory of functions of several complex variables. There are great differences between this theory and the theory of analytic functions of one complex variable. Also in this domain there is a great activity. It is not the place here to give details about the aspects of this theory: analytic spaces, holomorphic functions on these spaces, algebras of holomorphic functions etc. But it is noteworthy that just in this theory the concepts of modern algebra and topology are important. The theory has structural aspects. more than aspects of properties of the individual objects. It is an essential aspect of mathematics of our years.

The theory of functions in a strict sense like we considered before is evidently only a special area of the broad field of analysis, one of the main domains of mathematics : differential equations, integral equations, harmonic analysis, functional analysis, analysis on Lie groups, theory of distributions etc. The concepts and methods of algebra are indispensable in these fields: function algebras, rings, ideals, maps, operations in analysis which find their origins in algebraical analogies,.. . Can these domains flourish so much just because of algebraization? A detailed description of these developments is beyond the scope of this book.

5.8 Algebra

In algebra itself the trend towards "algebraization" in the development can be traced. Therefore classical algebra has to be compared with what in the twenties -and perhaps still for more years- was called modern algebra (a name which does not make sense anay longer). In the foregoing it was already observed that until the middle of the 19th century algebra consisted mainly in the theory of algebraic equations. One studied properties of the roots, methods for solving equations etc. There were the theory of determinants, continued fractions with their algorithmic aspects. Some subjects which now are said to belong to analysis were treated in algebra. For instance the theory of real numbers, the theory of infinite series, convergence and divergence. Also the classical theorem of d'Alembert ascertaining that every algebraic equation has roots in the field of the complex numbers(evidently not in the modern form of algebraically closed fields).

In the second part of the 19th century the picture changed. Special problems came in the background and the interest was directed towards structural problems in algebra, where more general systems than the real numbers came to play a role. It is the period of introduction of the concept and theory of groups, rings, fields. It was the way towards "modern algebra". For the rest, research here advanced to large extent the teaching programs.

Is "classical algebra" nowadays superfluous? Evidently not; but it can not be said that the classical theory forms a center of interest. The old problems are represented in a new form, where the structural aspects are in the foreground. For example the theory of Galois, which contributes to throwing new light on classical problems. This is a trend which can be called the "algebraization of algebra"

Concluding remarks

In the foregoing several aspects of the phenomenon of algebraization are treated. Is it possible to attach some conclusions with them with respect to the way of mathematics?

Resuming the following aspects can be distinguished.

1. Introduction of algebraic methods in existing domains, leading to an expansion of the area (Descartes).
2. Attempts to give a sharper foundation of theories by means of a reduction to algebra (Lagrange).
3. Aspiration to get more and deeper insight in theories by considering structural aspects (Bourlet, Drach; Galois; Lie and his successors).
4. Introduction of set theoretic and algebraic structures and application

to existing areas (the young functional analysis).
5. The stage in which algebraic structures are studied for itself.

This is a description of phases in the development of mathematics. Some fundamental questions can be associated with it. The answer depend up to a high measure on the standpoint that one takes with respect to the scientific place of mathematics.

First there is the question how to judge these developments. What is their value? An other question should precede a discussion. Can algebraization be considered as an issue from mathematics itself, coming from inside and therefore a purpose in itself? If one has the opinion that a positive answer must be given, then the question of the value has scarcely any sense. Then it concerns a factual situation.

It one does not see the development in this way, there is quite an other situation. Algebraization in a broad sense should perhaps be considered as only a way gone by mathematicians. Is it the only possible way? Considering mathematics as a whole there is a tendency to incorporate special problems and theories in more general theories of a higher level in which structures take an important place. This tendency led to the introduction of sets in various domains. Structures on these sets were introduced, leading to rings, fields etc. This is the way of algebraization. But examples from history show that the methods of algebra are not sufficient for any situation. It appears that one can not do without topological structures. In general it is in vain trying to force all in the form of algebraic structures. And there are no good reasons to judge the value of a theory according to the criterion whether it has been presented in a purely algebraic form or not.

The way of algebraization has contributed to the image of mathematics as a unity. It appears to be possible to draw lines of development between areas which apparently are at great distance from each other: lines between geometry and analysis and between algebra and geometry, lines inside analysis etc, It seems that the algebraization -connected with topology- fulfilled a role in this.

Thus, it seems that the way of algebraization is a way of high value.

It is an other question whether algebra and algebraization are the most recommended waysfor approaching new domains. It is difficult to give an answer, but it is likely that the answer depends largely on the subject. The question can even be posed whether algebra itself has always had the advantage of "algebraization". In an interesting paper [Birkhoff, 1973], the author mentions new directions where there is less emphasis on structural methods.

A second question concerns the way by which algebraization is reached. What are the sources of algebraization? In the preceding we have treated several examples of mathematicians who in their creation of new areas and new theories were led by arguments of analogy: Bourlet, Drach in the domain of differential equations; Liouville and Ritt; Fréchet and his theory of limits; ways in topology. Analogies can contribute to observe common structures; there is a connection between the aspects of analogy and equivalent structures. So there is the question: is the method of analogy one of the basic tools of algebraization? Can the source of algebraization be found in the desire to discover equivalent structures by means of the method of analogy? Perhaps algebra, as fundamental discipline, can then be considered as the most obvious apparatus on the way of analogy. Does the method of analogy means a contribution to the image of unity of mathematics?

These are questions which should be treated in a more general framework. What has been the role of analogies as creative apparatus in mathematics? This question asks for a historical study on the function of the method of analogy in the evolution of mathematics.
This subject shall not be treated here.

NOTES

1. S.B. Bayer, Akad. Wiss. 1938, 27-69.
2 Siam J. Math. Anal., Vol. 6, 295-311 (1975).
3. Journal of Functional Analysis 4, 277-294 (1969).
4. Bull. Am. Math. Soc. 11, no.1, p.228 (1984).
5. The Mathematical Intelligencer 6, no.3 (1984).
6. Compare [Rosenlicht, 1969]. It is proved that this equation has no solution in any field of meromorphic functions of x which is a "Liouville extension" of $\mathbb{C}(x)$.
7. In his concept of a Universal Algebra Leibniz already had the idea of the possibility of infinitely many algebras. They were characterized by their fundamental laws, i.e. their fundamental operations. An example is the classical algebra, the algebra of the numbers, which is based on the relation of equality. He also considered algebras in which a relation aa = a is possible. See [Couturat, 1961] in particular Chapter VII "La mathématique universelle".
8. In [Boutroux, 1920] Boutroux considers the backgrounds of the principle of continuity. From the way in which Poncelet and his successors used this principle for developing his geometry, making fully abstraction from the figures and considering only laws and conditions, he concludes that the geometry of Poncelet "était., au fond, une algèbre déguiseée".
9. In a paper [Geiser, 1872-73] it is observed that this is perhaps not quite correct. Some mathematicians had the opinion that Steiner knew more about analysis than he wanted to admit.
10. Laguerre contributed to these developments. See [Monna, 1973].
11. Earlier the french mathematician Arbogast (1759-1803) considered a formal definition of the derivative of a function in a treatise "Essai sur de nouveaux principes du Calcul différentiel et intégral indépendant de la théorie des infiniment petits et de celle des limites". This treatise from 1789 was not printed but it is mentioned by Lagrange. See Fréchet, "Biographie du mathématicien Alsacien Arbogast" in Fréchet "Les mathématiques et le concret" (Paris 1955).
12. Meschkowski (1967) expressed this by saying that it concerns the "arithmétization de l'analyse".

    For comments on the works of Drach see : "Eléments pour une étude sur Jules Drach" in Cahiers du Séminaire d'Histoire des Mathématiques, 2 (1981) Paris.

13. Ritt (1948) mentions the possibility that Liouville found his inspiration in the work of Abel on the impossibility of solving the general algebraic equation of the 5th degree in terms of radicals. The classification of radicals resembles in some way Liouville's classification of elementary functions.
14. A definition of the term "elementary" is given. See the literature.
15. The theory of abstract groups was developed from the middle of the 19th century on. Several mathematicians contributed: Cayley, Frobenius, Dedekind etc.[See Wussing, 1969].
16. In his proof of the existence of an integral -the integral of Lebesgue- Lebesgue could not avoid the used of limits. Thus, it is not a strict algebraic process. An algebraic program for the introduction of an integral was much later on realized by Banach. This shall be a point of discussion in 4.4.
17. In view of these difficulties there have been attempts to use other forms of representation of analytic functions: infinite products, series of polynomials, divergent series etc.
    In this framework one has studied properties which are independent of the kind of representation, for instance the "mode de croissance" of Borel. See:[Boutroux, 1908],[Hadamard, Mandelbrojt, 1926].

PART II THE EVOLUTION OF EXISTENCE PROBLEMS

INTRODUCTION

In Part I we considered the general trend in mathematics towards algebraization and we studied its historical roots.

Part II also concerns general aspects of the evolution of mathematics but they are of a more special type. We will consider the evolution of the concept of existence, in particular its place in analysis. Existence and existence theorems are among the fundamental concepts in mathematics. We will compare the meaning of existence in classical and in modern mathematics. Especially the last decades of the 19th century and the first of the 20th century are important for the development of modern mathematics, to some extent a period comparable to the time of Descartes and the invention of infinitesimal calculus. In this period the ideas about mathematical existence changed considerably. The new image it got is one of the points that is characteristic for modern mathematics in comparison to classical mathematics. We will treat several examples of this evolution in detail.

In the following considerations we will consider developments in pure mathematics. The developments in applied mathematics and in numerical analysis form another subject of research. The situation in these areas may be very different; see [Goldstine, 1977].

CHAPTER 1 EXISTENCE IN CLASSICAL AND MODERN MATHEMATICS

1.1 Classical mathematics

What are the points - or at least some points - in which modern mathematics differs from classical mathematics? Before trying to give an answer, we will say something about classical mathematics. Every mathematician has some idea of what is meant by "classical". Nevertheless, some global remarks are useful for a good understanding of what follows. We shall not try to give a more or less complete description of the domain of "classical mathematics". Moreover, a sharp border between "classical" and "modern" can not be traced. "Classical mathematics" is not a closed subject.
First on algebra. Until the middle of the 19th century algebra mainly consisted in the theory of algebraic equations and subjects that in some way are connected with it. That is what I call classical algebra. The theory of groups gradually developed, first in concrete situations, much later in abstract form. It is the development of "modern algebra". See Part I and [Birkhoff, 1976].
In geometry we mention traditional analytic geometry: conic sections, "microscopic" theory of ellipsis, hyperbolas etc., quadratic surfaces, algebraic curves and algebraic geometry treated with the means of cartesian geometry over the reals (The "elementary" theory), theory of invariants. Elements of projective geometry, descriptive geometry. Then differential geometry of curves and surfaces; curvature, curves on surfaces (Gauss). Some initiations to the theory of varieties and topology (Riemann, Poincaré), but that is nearly "modern".
In analysis - the main subject here - there is infinitesimal calculus For example: the theory and calculations of integrals, infinite series, elementary theory of Fourier series, integral transformations and problems on inversion (Laplace), special functions (B-and Γ-function), etc. The theory of analytic functions of a complex variable: entire functions, meromorphic functions, elliptic functions, elliptic and hyperelliptic integrals. Theory of conformal representation (Riemann). Calculus of variations. The theory of differential equations,

in particular explicit solutions; singular points of differential equations. There were some results on the problem of the existence of solutions of differential equations; Cauchy treated the existence problem for the equation

$$\frac{dy}{dx} = f(x,y).$$

We shall treat this problem in Part III.
In the classical period there was also a theory on the integration of certain classes of partial differential equations (Green, Lagrange, Cauchy, G. Kowalewski). Special cases were studied, the problem was to find explicit solutions, there were examples and counterexamples - but this is still a subject of our time. It was the period in which the construction of solutions, concrete theories, took an important place.[1)]
To get an impression of what this all means one may for example as concerns analysis read the book "Whittaker and Watson, A course of Modern Analysis", edited in 1902 (Last edition 1977). The meaning of "modern" is fluctuating! See also the various "Cours d'Analyse" (Picard, Jordan, Goursat, de La Vallée Poussin).

As concerns classical analytic geometry we mention the books of Salmon-Fiedler. These are new editions of books originally written by the Irish theologian - mathematician Salmon. In 1848 Salmon published "A treatise on Conic sections, containing an account of some of the most important modern algebraic and geometric methods"; more editions followed. Then followed "A treatise on the higher plane curves" and "A treatise on the geometry of three dimensions". Salmon used new methods in algebra, for which he refers to Cayley and Sylvester [2)]. These books were adapted and extended by Fiedler about 1860, in the twenties of our century by Dingeldey, Brill, Kommerell and then they became known as "Salmon-Fiedler"; they were much studied by students in these years; now they are forgotten[3)].

These are examples of subjects of what is called "Classical mathematics". Half a century ago, and even in later years, these were nearly the principal subjects that constituted the university-curricula, with exception of special courses and more advanced seminars. This although research was much more advanced. It may be interesting for students of our time to read sometimes in these classical books.

1.2 Modern mathematics

What is not found in classical mathematics are statements about properties of collections, classes or spaces of functions or of any other objects, provided with an algebraic or topological structure or both. Results of this kind were reserved for a later period; they are typical for modern mathematics. Classical mathematics had a strong constructive character. In modern mathematics, however, theories which concern pure existence take an important place, perhaps a dominating place in pure mathematics. Theorems which express the existence of a certain object, a function etc., satisfying certain conditions are frequent. Statements in which there is no need to give an explicit form of the object. Often this is even for several reasons impossible. The problem of the existence of certain solutions seems often to be more important than the solutions themselves. The problem of finding a solution is replaced by the problem of solvability. Information on the collection of the solutions is asked. Are there "many" solutions, where the meaning of "many" has to be stated precisely? It is a kind of problem, posed long years before. But the essential point is now that one demands to determine the "quantity" of the individual solutions and the notion of "quantity" has become much more abstract: it is expressed in terms of category, measure, cardinal number, dimension.

Certain functions, having certain properties which first are considered to be "pathological", are they exceptions, or do they represent, after careful study, a normal situation? In this way old problems, coming from the classical period, can present themselves in a new strongly modified form. It is not the right question to ask which way should be preferred, the classical way or the modern way. Indeed, it is a question of evolution [4)]. First of all it must be remarked that, evidently, the periods are not disjoint. On the other hand, the second way, the way of pure existence, considered from a philosophical point of view, may be a contribution to a better understanding of mathematical theories. This may be considered as a subjective conclusion. However, it is based on experience in a time when university programs were in general still highly constructive. The situation now is very different. One may ask what, from didactic point of view, is the best way. Is it desirable to start directly with abstract theories, for example to treat Banach spaces in its abstract axiomatic form without saying something about the origins which were very concrete? It seems that some historical information on the way that was gone is highly desirable.

In the next chapter we will treat several subjects from analysis to illustrate these developments. These different subjects will be considered from the same point of view, namely the transition from the constructive phase into the existential phase. The difference between the standpoints manifests itself in these examples. It is a historical study, but not of the most usual kind. It concerns the penetration and influence of new ideas. Thus, it is a study in the area of the history of ideas. For a much older study of this type see [Boutroux, 1920]. It is a general study; the author considers the whole domain of mathematics. For a special study on historical developments about construction and existence in algebra we refer to a publication of [E. Schillemans, 1975-76]. The author considers the ancient problem of the solution of algebraic equations, in particular the role of Galois theory. He distinguishes two periods in the history of this problem. In the first period mathematicians considered as the main problem in algebra the effective calculation of the roots of algebraic equations, that is methods allowing to construct the roots. The author calls the concept - the point of view - according to which the mathematical objects in question (in this case the roots) must effectively be calculated the constructional concept. As far as concerns algebraic equations this point of view changed gradually after the works of Galois (there were earlier initations by Lagrange). But the penetration of new ideas took much time and only after some decades the concept was arrived that not the calculations are of first importance, but the notions and structures which are at the base of the theory. Schillemans calls this second period the existential concept.

This distinction between points of view in algebra resembles the developments in analysis mentioned before and which will be treated in the next chapter. It is curious that in analysis the transition in the points of view came much later than in algebra. These developments in analysis began after the works of Cantor on the theory of sets and even much later. They depended essentially on the way created by Cantor. Thus, there is some difference in the situations in algebra and analysis. This is connected with the relations between the creation of modern algebra and the first developments in the theory of sets [5].

We shall make some general remarks as an introduction to the examples of the next chapter.

The examples find their origin in classical theorems and theories. But in the new point of view they appear in a very different form. They are all of the same character. They concern theorems which, for example, express that nearly all functions of a certain class have a certain property. Or it is asked whether all elements of a certain set have some well defined property and, if not, one wants information on the set of elements which have that propriety and on the set of elements which do not have it. These theorem are not associated to constructive methods. There are reasonings that concern the entire collection, without the need to consider the individuals. Essential here is the concept of a totality of functions or other mathematical objects, provided with a certain structure. In classical analysis there are, evidently, also theorems which concern all functions of a certain class. For example the theorem that every differentiable function is continuous. But such a theorem is proved by taking an arbitrary function of that class and giving a proof for that function without any regard to the collection itself. The reasonings we have in view are entirely different. There is an other example in classical analysis. In the theory of integration of partial differential equations it was formerly customary to say that the solution depends on an "arbitrary function". In an exact formulation such a statement is insufficient: "arbitrary" needs more precision. It must be added to what collection or function space these functions belong.

This development was in the first place possible as a consequence of the works of Cantor. The theory of sets furnished the possibility to study phenomena of a collective character, to distinguish from phenomena of a special character. One may suppose that theories which concern properties of structured totalities could scarcely have been created before Cantor because there was not yet an abstract idea of a set [6)].

On the other hand there was the influence of the development of modern algebra. We mention, for instance, the methods of functional analysis which heavily depend on algebraic methods. The concept of a vector space, important as a tool in functional analysis, comes from algebra. Also topological developments must here be mentioned. Our examples are connected with functional analysis and topology. Some of them concern problems on classification, in more direct relation to the theory of sets. But they are all in some way characteristic for the transition from the constructive to the existential phase [7].

1.3 The introduction of set theory

As these new ideas in mathematics were essentially based on the introduction of set theory and the developments in topology related, it is worthwhile to consider the way in which set-theory entered in analysis, in particular in the the theory of functions. These remarks shall be of bibliographic character. We will make some remarks about books that have playd a role in this process. In the examples of the next chapter some of these books have to be mentioned again. Compare Part I, 5.7.

In the ancient theory of real functions problems of a special character were treated. Let us mention, for instance, the long history of the representation of continuous functions by means of infinite series (Fourier series; approximation by polynomials-Weierstrass). These were theorems of constructive character. There were the long discussions on the nature of the concept of a function and the questions on continuity and discontinuity. Continuous functions without derivative were for a long time considered as pathological. Only when there was a clear insight in such fundamental questions theories of existential type could be created on the base of set theory: the spaces $L^2, L^p, \ldots$. [8].

From the end of the 19th century till the twenties, thirties, of the 20th century there was great activity in this domain. In particular "l'Ecole Francaise" and later "l'Ecole Polonaise" playd an important role. In these years set theory and the first notions of topology came gradually into analysis.

In 1901 Emile Borel took the initiative to publish a series of books in this domain under the name "Collection de monographies sur la théorie des fonctions", better known as the "Collection Borel". But before 1901 some books of this type had already been published in France. It is a long series and it is evidently not the place to mention them all. Borel himself wrote several books in htis Series and after him several generations of mathematicians contributed to it. Often series of lectures were the cause to write these books and then they were edited by mathematicians who later on, contributed themselves to this domain. Also mathematicians of other countries contributed. The subjects are of various kinds. There are books in this Series which still must be called classical, not so much under the influence of set theory. For example Borel's book "Leçons sur les fonctions entières" (1900). But it can be said that in general these books live under the light of the theory of sets. The steadily growing influence of set theory and topology is apparent. In many of these books especially in those of the first years, there are introductions to the theory of sets and set theoretical topology. There are several notes on the principles of set theory, often of polemic character because the discussions in this domain were not yet finished. Looking at this Series as a whole one perceives the evolution of the ideas. We will give some details to illustrate this aspect.

The first book in this Series can be considered as a classical book with respect to the introduction of sets in analysis:
E. Borel, Leçons sur la théorie des fonctions (1898). The influence of Cantor is evident in this book. We shall not review this book; some remarks may suffice. Chapter I contains an exposition of the new theory of sets: "Notions générales sur les ensembles". In chapter II "Les nombres algébriques et l'approximation des incommensurables" one finds Cantor's theorem stating that the set of algebraic numbers is denumerable. Of special interest is chapter III "Les ensembles parfaits et les ensembles mesurables". On the one hand there is a "constructive" theory of measure theoretical concepts; we will treat this subject in the next chapter from the existential point of view. On the other hand it is interesting for topological reasons. Borel proves the following theorem:

"Si l'on a sur un segment limité de droite une infinité dénombrable d'intervalles partiels, tels que tout point de la droite soit intérieur à l'un au moins des intervalles, il existe un nombre limité d'intervalles choisis parmi les intervalles donnés et ayant la même propriété (tout point de la droite est intérieur à, au moins, l'un d'eux)". (l.c.p. 42).

Borel added the following note: "On trouvera dans ma thèse une autre démonstration de ce théorème, démonstration qui donne un moyen théorique de déterminer effectivement les intervalles en nombre limité dont il est question".

This theorem states a property which, later on, will be called local compactness. The influence of set theory is evident when this theorem is compared with the ancient theorem of Bolzano-Weierstrass stating that every bounded sequence contains a convergent subsequence. Apparently Borel had an idea of the difference between constructive methods and theories which only state the existence of certain objects.

We must add that we don't understand here - as we shall always do is the sequel - constructive in a too limited sense of finite computations. Procedures which permit approximation in connection with a passage to the limit will be considered as constructive.

At the end of this book there is an interesting note "La croissance des fonctions et les nombres de la deuxième classe". This note concerns a theory of Du Bois-Reymond on a certain classification of real functions, connected with asymptotic properties. We will consider this in chapter 2. Borel studied this theory also in his book "Leçons sur la théorie de la croissance" (1910).

It is worthwhile mentioning some other books in connection with the considerations in the next chapter.

E. Borel, Leçons sur les séries divergentes (1901)

H. Lebesgue, Leçons sur l'intégration et la recherche des fonctions primitives (1904).

R. Baire, Leçons sur les fonctions discontinues (1905).

This last book contains a long introduction to the theory of sets with the purpose to give a characterization of the real functions which are the limit of sequences of continuous real functions. This subject is connected with a general classification of real functions (Baire-classes). We return to this in chapter 2 in relation with transfinite numbers. See for this subject:

C. de la Vallée Poussin, Intégrales de Lebesgue, Fonctions d'ensembles, Classes de Baire (1916).

Volterra published two books in this Series on "fonctions de lignes" and integral equations (1913). They are initiations to the general concept of a functional and thus to abstract functional analysis. We shall consider this in Part III in an other context.

As to functional analysis there is:

P. Lévy, Leçons d'analyse fonctionelle (1922).

This is a treatise on analysis in function spaces, essentially a subject of the new period.

A book of F. Riesz contains an excellent illustration of the transformation of the constructive phase into the existential phase:

F. Riesz, Les systèmes d'équations linéaires à une infinité d'inconnus (1913).

It is concerned with the theory of systems of equations

$$\sum_{k=1}^{\infty} a_{ik}x_k = b_i, \quad i = 1,2,\ldots; \; a_{ik}, b_i \in \mathbb{R}.$$

Riesz treated also the history of this subject. First special cases were studied and the problem was to find explicit solutions. In the beginning of the 20th century the standpoint changed and the interest turned to the problem to give necessary and sufficient conditions in order that a solution of the general equation exists. It must then be specified in which space the solution exists. It is the problem of solvability [Monna, 1973b]. In the thirties of the 20th century this problem was again treated in the general framework of the theory of Banach spaces. The standpoint is then totally changed: one is no longer interested in explicit solutions (see in [Banach, 1932]).

Even yet in 1922 Borel published a book in this Series: "Méthodes et problèmes de théorie des fonctions". Then there had appeared 26 books in this series. Borel collected here "un certain nombre de Notes et Mémoires qui n'avaient pas trouvé place dans les Ouvrages antérieurs et dont certains me paraîssent cependant être le point de départ de recherches nouvelles". There is a strong emphasis on questions related to set theoretical problems.

Buth this is not yet the last book. There are books of Carleman, Fréchet, Montel, Julia. Some are of a "classical" type. Others treat problems tending into a more modern direction, for instance the introduction of normal families of holomorphic functions, to be considered as a generalization of the ancient theorem of Bolzano-Weierstrass, but now formulated in terms of sets of functions, see [Monna, 1978].

We shall not continue the review of this series any further. Most of these books are worthwhile to be read by any mathematician who is interested in the development of analysis in the first decades of our century. They contain important information on the growing influence of set theory and topology on the process of transformation from the constructive to the existential period. But evidently there are more indications for this evolution of ideas. Some other sources have to be mentioned.

In the first place the fundamental work in the Polish School must be mentioned. Several mathematicians contributed: Steinhaus, Banach, Schauder, Mazur, Ulam, Tarski, Kuratowski, Sierpinski, Mazurkiewicz. Many of their results were published in the Polish journal Fundamenta Mathematicae, founded in 1920 (Warszawa). This journal contains many papers which are interesting for our point of view. Another journal, founded in 1929 (Lwow), Studia Mathematica, contains also much information. Both Banach's book Théorie des opérations linéaires (1932) and C. Kuratowski's, Topologie, I, II, contain examples and passages which illustrate the changing points of view.

Finally there is:
F. Hausdorff, Grundzüge der Mengenlehre (1914). Especially some results in the last chapter "Inhalte von Punktmensen" will appear to be interesting for our purpose. There are two editions of this book but the second edition differs considerably from the original edition. In the first edition Hausdorff took a topological point of view. In the second edition, however, the considered mainly the theory of metric spaces. In this last edition he omitted furthermore the chapter on measure theory, motivating this by the argument that at that time there were already sufficiently many books on measure theory. However, it are just some results in the first edition - the existence of a universal measure - that we need in the following and most of the books on measure theory do not contain these results [9].

1.4 Some basic results

In Chapter 2 we shall need some results that are based on the axiom of choice or on statements that are equivalent to it. It seems to be useful to give a short review.

(i) The axiom of choice

Let W be a nonempty family of nonempty sets. Then there exists a function f wich associates with very $V \in W$ an element $f(V) \in V$.

This axiom was formulated for the first time by Zermelo in 1904. It is an axiom in the domain of the foundation of mathematics and it was a point of many discussions in axiomatic set theory. There are several equivalent formulations. We mention some.

Every set can be well ordered.

This goes back to Cantor.

Zorn's lemma.

Let W be a partially ordered set. Let a chain in W be every subset V of W which is totally ordered with respect to the induced partial order on V. Then Zorn's lemma reads:

If every chain in W has an upper bound in W, then W contains a maximal element.

This maximal element is not necessarily unique. We shall not prove

these equivalences. See [Jech, 1973]
These are non constructive statements: in general a well ordering or a maximal element cannot be determined effectively. A well ordering of the real numbers is not known. They are frequently used in analysis.

(ii) Existence of a base for vectorspaces

In 1905 G. Hamel proved the following theorem for the field of real numbers [Hamel, 1905]:

"Es existiert eine Basis aller Zahlen, d.h. es gibt eine Menge von Zahlen a,b,c,... derart dass sich jede Zahl x in einer und auch nur einer Weise in der Form

$$x = \alpha a + \beta b + \gamma c + \ldots$$

darstellen lässt, wo die Zahlen $\alpha,\beta,\gamma,\ldots$ rational sind, aber in jedem einzelnen Falle nur eine endliche Anzahl von ihnen von Null verschieden ist".

To prove this Hamel used a well ordering of $\mathbb{R}$. Later on this theorem was generalized for vectorspaces. Hamel's theorem appears then as a special case, if we consider $\mathbb{R}$ as a vectorspace over $\mathbb{Q}$. It is the following theorem:

Let E be a vectorspace over a field K. Then there exists a set

$$H = (x_\alpha)_{\alpha \in I}, \qquad x_\alpha \in E,$$

where I is a set of indices, such that every $x \in E$ can be represented in a unique way in the form

$$x = \sum_{\alpha \in I} a_\alpha x_\alpha, \qquad a_\alpha \in K,$$

where only a finite number of the elements a_α is different from zero.

This is an algebraic base, known under the name Hamel base. It is a simple consequence of Zorn's lemma.

(iii) The theorem of Hahn-Banach

This is a fundamental theorem in functional analysis concerning the extension of linear functionals. In its simplest form it is the following theorem:

Let E be a vectorspace over $\mathbb{R}$. Let p be a real function on E satisfying the follwoing conditions

$$p(x+y) \leqslant p(x) + p(y), \quad x,y \in E,$$
$$p(ax) = ap(x) \quad \text{for all} \quad x \in E, \ a \geqslant 0.$$

Let V be a subspace in E and f a linear functional on V with values in $\mathbb{R}$ verifying $f(x) \leqslant p(x)$, $x \in V$. Then there exists a linear functional F on E verifying

$$f(x) = F(x), \quad x \in V,$$
$$F(x) \leqslant p(x), \quad x \in E.$$

Banach gave a proof using well ordering. Now it is customary to use Zorn's lemma. In this form it is a theorem of algebraic character. The existence of linear continuous functionals on normed spaces is a consequence of it. We refer to textbooks on functional analysis.

Some remarks

The results on the existence of a base and the theorem of Hahn-Banach are non constructive. No method, an algorithm for instance, is known to determine a base in the general case. Neither there is a general method to determine in explicit form the extension meant in the theorem of Hahn-Banach. And there are no means to calculate approximations. These theorems are ineffective.

During the first decades of our century there were long discussions about the value of results of this type, especially among the mathematicians of the Ecole Francaise. A point of discussion was the problem what should be the meaning of "to define a function". Is definition sufficient to be sure of the existence? Or should only effective definitions be accepted? Emile Borel defended the standpoint that only calculable objects are really important in mathematics, aside of purely theoretical considerations. Baire also took this extreme point of view. Nevertheless he defined a transfinite classification of real functions. The effectivity of this classification has been a point of discussions. These discussions concerned questions on existence. The second edition (1914) of Borel's

"Leçons sur la théorie des fonctions" contains interesting notes.[10) See [Monna, 1972]. We shall treat this subject in a more detailed way in the next chapter (section 2.7 (ii)).

Furthermore, constructivity and effectiveness are, from historical point of view, somewhat dangerous concepts. Denjoy, for example, introduced a concept of integral, which now bears his name, by means of a method that he called constructive. However, he used transfinite methods which can scarcely be considered as appropriate for approximations. Here we come on problems of "constructive" and "descriptive" definitions which we already discussed in Part I, 4.3. We refer to [Saks, 1937].

CHAPTER 2 EVOLUTION OF CLASSICAL PROBLEMS

In this Chapter we will consider examples of the evolution of problems in classical analysis. In these examples the transformation of the ideas under the influence of developments in the theory of sets and topology is quite clear. Some of these classical problems were presented in a new form as a consequence of functional analysis. These are for instance

(i) The theory of divergent series.

(ii) The problem of universal measure.

(iii) The existence of continuous functions without derivative.

(iv) Convergence and divergence of Fourier series.

Abstract functional analysis, the theory of Banach spaces, was created in the twenties of our century expecially by the mathematicians of the Polish School. From the outset they applied the abstract theory to classical problems of analysis. They realized that the abstract methods were appropriate to consider certain classical problems from a new point of view and thus to place these problems in a broader framework where the attention is directed towards problems of the existence of certain mathematical entities. Functional analysis had not been created without direct thoughts on possible applications. Banach's fundamental paper "Sur les opérations dans les ensembles abstraits et leur application aux équations intégrales" (thesis 1920 Lwów; Fundamenta Mathematicae 3), where he introduced normed spaces in abstract sense, contains an introduction in which he explains the motivations to introduce this notion. The reasons were to avoid special reasonings for any special case; Banach gave examples.[11] See [Monna, 1973 b].

However, several years before functional analysis in abstract sense was introduced, there were already investigations where problems of existence, apart from constructions, were the point of interest. I have in view some work of Hamel, which is independent from functional analysis.

2.1 The functional equation $f(x+y) = f(x) + f(y)$

Already Cauchy studied this equation (1821). He proved that if a real function f satisfies the relation

$$f(x+y) = f(x) + f(y)$$

for all $x,y \in \mathbb{R}$ and if f is supposed to be continuous, then

$$f(x) = cx,$$

where c is a constant.

Evidently there is no reason that in this equation f should be continuous. Therefore in later years there were attempts to weaken this condition. There are results of Darboux (1880), Sierpinski (1920), Kurepa (1956). We mention only that the condition "f is measurable" is sufficient to prove: $f(x) = cx$. [Aczél, 1969].

In 1905 G. Hamel succeeded in giving the general solution. He used his result on the existence of a base for $\mathbb{R}$ and stated "Es existieren unstetige Lösungen der Funktionalgleichung

$$f(x+y) = f(x) + f(y)$$

und wir können sie alle angeben".
Indeed, let $(\xi_\alpha)_{\alpha\in I}$, $\xi_\alpha \in \mathbb{R}$ be a Hamel base of $\mathbb{R}$, considered as a vectorspace over $\mathbb{Q}$. Then any $\alpha \in \mathbb{R}$ can be written in a unique way as

$$x = \sum_\alpha p_\alpha(x)\xi_\alpha, \quad p_\alpha \in \mathbb{Q},$$

where the sum is finite. Define a function on I by

$$\alpha \mapsto C_\alpha, \qquad C_\alpha \in \mathbb{R}.$$

Then the general solution is given by

$$f(x) = \sum_\alpha C_\alpha p_\alpha(x).$$

This solution is ineffective: a base for $\mathbb{R}$ is not known in an explicit way and therefore it is impossible to calculate effectively a discontinuous solution of the functional equation. We can say that the solution is either a linear function, or is extremally discontinuous. One must be cautious in interpreting Hamel's term "angeben".

2.2 The theory of divergent series

We are concerned here with the old question whether it is possible to give a sense to divergent series and whether it is permitted to give them a place in mathematics, that is to say whether they can be accepted in rigorous reasonings. It is a subject with a long history. Remind for instance the numerous discussions about the series 1-1+1-1+..., connected with the names Bernoulli, Leibniz, Euler,... Is there a "sum"?

The problem arose whether it is possible to assign to any numerical divergent series a number, or a function when it concerns series which contain a variable, in a suitable way, that is: not being an addition of the terms. Several methods for summation were developed. For the history we refer to a paper by Tucciarone (1973). To give an idea we give some examples.

There are methods based on mean values. Like in the classical case one begins with considering the sequence (S_n) of partial sums. Cesàro studied

$$\lim_{n\to\infty} \frac{s_1+\ldots+s_n}{n}$$

Also iterations of this procedure were considered. In a more general way one studied weighted means

$$\lim_{n\to\infty} \frac{a_1 s_1+\ldots+a_n s_n}{a_1+\ldots+a_n} \quad .$$

To any summation procedure one imposed the condition that any series which is convergent in the classical sense should also be summable with respect to this new process and then give the same "sum". About 1900 Borel introduced infinite means.

Let ϕ be an entire function

$$\phi(x) = \sum_0^\infty c_n x^n .$$

Borel studied the expression

$$\lim_{x\to\infty} \frac{\Sigma c_n S_n x^n}{\phi(x)} ,$$

and, if this limit exists, he defined this as the "sum" of the divergent series. In particular he considered the case where

$$\phi(x) = e^x.$$

In this case one has

$$S = \lim_{x\to\infty} \frac{S_0 + S_1\frac{x}{1} + S_2\frac{x^2}{2!} + \dots}{1 + \frac{x}{1} + \frac{x^2}{2!} + \dots}.$$

This method is stronger than Cesàro's. Borel also used this method in the form of an integral. Put

$$u(x) = u_0 + u_1 x + \frac{u_2 x^2}{2!} + \dots$$

There he defines

$$S = \int_0^\infty e^{-x} u(x)\,dx$$

if this integral has a sense. He also studied the case in which not only this integral has a sense but also all the integrals

$$\int_0^\infty e^{-x} |u^{(\lambda)}(x)|\,dx,$$

where $u^{(\lambda)}$ is a derivative of u of arbitrary order. In this case he said that the series is absolutely summable. We shall not insist on this theory. We refer to the paper of Tucciarone. See also Borel's "Leçons sur les séries divergentes" (1901) where one finds several summation procedures and their mutual relations; there is also an interesting introduction to the history of analysis. Borel considered also power series and he showed that the method of analytic continuation may furnish a summation method. Consider the series

$$\phi(z) = u_0 + u_1 z + u_2 z^2 + \dots$$

and suppose it is not everywhere convergent. Suppose the series diverges for $z = z_0$. Then Borel proposed to agree that the sum of the series

$$u_0 + u_1z_0 + u_2z_0^2 + \dots$$

is equal to $\phi(z_0)$, where $\phi(z_0)$ is the value in the point $z = z_0$ of the analytic function defined by the given series, except in the case where z_0 is a singular point. This gives a method for numerical divergent series. If

$$v = v_0 + v_1 + v_2 + \dots$$

is such a series, put

$$v_n = u_nz_0^n \ (n = 0,1,2,\dots)$$

where z_0 is an arbitrary constant, and therefore

$$v = u_0 + u_1z_0 + u_2z_0^2 + \dots = \phi(z_0).$$

Borel remarks that the result is independent of the choice of z_0. The summation problem for divergent series is then "Déterminer la valeur numérique de $\phi(z_0)$ en fonction des valeurs numériques de ses termes" (Borel l.c. p. 22). He remarked that "la théorie du prolongement analytique fournit d'ailleurs théoriquement une méthode pour résoudre cette question; mais cette méthode n'est guère pratiquement applicable". We shall not continue Borel's remarks about this problem; he refers to the works of Mittag - Leffler on the theory of analytic continuation. Some passages in the introduction of his book are of interest here. Borel states that the fundamental problem in the theory of divergent series is the following:

"Faire correspondre à chaque série divergente numérique un nombre tel que la substitution de ce nombre à la série, dans les calculs usuels où elle peut se présenter, donne des résultats exacts, ou du moins presque toujours exacts" (l.c.p. 14).

As concerns "les calculs usuels" there are addition and multiplication of series. The question had already been studied (Cesàro). Borel observed "qu'on ne peut guère espérer de résoudre ce problème précédent pour <u>toutes</u> les séries divergentes; l'infinité non dénombrable des modes de divergence paraît être un obstacle insurmontable; mais ce serait déjà un résultat fort important de l'avoir résolu pour les séries divergentes que l'on peut être effectivement amené à rencontrer dans les applications".

He also made the following interesting remark:

"On pourrait d'ailleurs être amené, comme nous en verrons des exemples plus loin, à attribuer plusieurs sommes différentes à une série divergente [12]; ce fait peut paraître tout d'abord étrange et paradoxal; il n'aurait pas paru moins étrange à un géomètre du XVIIIe siècle d'entendre affirmer que l'intégrale définie

$$\int_1^2 \frac{dz}{z}$$

n'a pas seulement pour valeur log 2, mais doit être considérée comme égale à

$$\log 2 + 2k\pi i,$$

k étant un nombre entier quelconque".

Later on Borel returned to this in a discussion about analytic continuation. He considered the series

$$\phi(z) = \log(1+z) = \frac{z}{1} - \frac{z^2}{2} + \frac{z^3}{3} - \frac{z^4}{4} + \ldots,$$

which gives for $z = 1$

$$\log 2 = 1 - \frac{1}{2} + \frac{1}{3} - \frac{1}{4} + \ldots,$$

and he remarked that "d' après la théorie du prolongement analytique on doit admettre que cette série convergente a pour somme, non seulement la valeur arithmétique de log 2, qui est sa somme arithmétique, mais toutes les valeurs de log(1+z) pour z = 1, c'est-à-dire

$$\log 2 + 2k\pi i,$$

ou k est un nombre entier quelconque".

We shall return now to Borel's fundamental problem. Functional analysis gives the solution of this problem, at least, to use Borel's words, a theoretical solution under certain restrictions. In Part I 4.3 "Limits and algebra" we treated the notion of a Banach limit. To every bounded sequence of real numbers (ξ_n) there can be assigned a real number, denoted by $\text{Lim}_{n\to\infty} \xi_n$, verifying the traditional properties of the ordinary limit lim and Lim = lim for every sequence for which lim exists. This Banach limit is not uniquely determined. The existence is a simple consequence of the theorem of Hahn-Banach. The solution of Borel's problem is then given by

$$\text{Lim}_{n\to\infty} S_n,$$

at least for any divergent series for which the sequence of partial sums is bounded. Reminding Borel's remark that he would be content to have the solution for some important classes of sequences, this restriction is not very serious. Perhaps more serious is the objection that this solution does not satisfy an other condition of Borel, namely the multiplication of series and it seems to be difficult to surmount this difficulty. Nevertheless with any divergent series is associated a number and it verifies additivity.

One may suppose that Borel has been acquainted with Banach's result on the existence of Lim; Borel died in 1956 and Banach's theorem dates from about 1932. However, supposing this, it is not very likely that he would have accepted it as a real solution of his problem. He asked for a method of summation which permits calculation of the "sum" for any given divergent series, at least a method that makes approximation possible. This in accordance with his standpoint on the foundation of mathematics. Now, Banach's result is non-constructive and there are no means for calculation. The theorem expresses existence and nothing more. It says only that the problem is solvable. There are several passages in Borel's book on divergent series which support the idea that he wanted constructive methods. There is for instance a passage where he considered the criteria to decide whether a given series is convergent or divergent:

"En effet, bien que Paul du Bois-Reymond ait fourni un exemple d'une série convergente dont la convergence ne peut être démontrée par aucun des critères de Bertrand, ou peut dire qu'en pratique ces critères suffisent, c'est-à-dire permettent d'étudier toutes les séries que l'on rencontre effectivement. Dès lors, si l'on a démontré une proposition pour toutes les séries dont la convergence peut être prouvée par l'un de ces critères,..., on pourra appliquer cette proposition à toutes les séries sauf peut-être quelques rares exceptions, de sorte que cette proposition, en apparence très particulière, ne sera pas loin d'être tout à fait générale" (l.c.p.72).

I don't know whether Banach has considered an application of his theorem to the problem of Borel. He refers to some work of Mazur from 1929 ("O metodach sumowalnosći") which was difficult to consult. Anyhow he does not mention it in his book from 1932. On the other hand, Banach occupied himself with methods of summation in the framework of his work on functional analysis. In his book there is a section "Quelques théorèmes sur les méthodes de sommation". There he proves some theorems on summation methods by means of certain matrices (there are references to Toeplitz and Mazur).

2.3 The problem of universal measure and paradoxical decompositions

One finds the same situation - i.e. the evolution of a problem of constructive character towards a problem of pure existense - when one studies the problem of defining a measure for sets.

We shall not treat here the traditional measure theory with subjects as σ-algebras of sets, measures on σ-algebras. For these topics we refer to the textbooks (for instance Halmos). Here we are interested in fundamental existence theorems. First we consider the problem of assigning to every bounded subset of $\mathbb{R}^n$ a measure which satisfies some suitable conditions. There will also be some remarks on measure on arbitrary sets, not necessarily $\mathbb{R}^n$. Thus, we are concerned here with the problem of the existence of universal measures.

The origins of the universal problem are the same as in the traditional theories where the problem is to attribute a measure to certain classes of sets. This goes back to the classical problem of calculating the area or volume of elementary figures. But in the general form to attribute a real non negative number - a measure - to sets, it is a typicall a post-Cantorian problem. Cantor himself gave definitions. Later there were Jordan, Borel, Lebesgue. Their definitions were constructive: by means of coverings a measure was attributed to certain sets in such a way that at least theoretically approximations of this measure could be calculated. These developments led to the Lebesgue measure and to the Lebesgue integral. Next to this in the first decades of the 20th century theories were developed in which certain systems of axioms were the point of departure. Then problems of existence enter and constructivity comes in the background. Several problems, still studied in our time, resulted from the problem of universal measure or are connected with it: problems in set theory (paradoxical decompositions); certain problems in the domain of the foundation of mathematics, existence of measurable cardinal numbers; in analysis the theory of amenable groups, groups provided with an invariant mean; relations with harmonic analysis; group theoretical problems (existence of certain free groups). It is not the place here to treat these problems in detail.

The problem of the existence of a measure begins with Lebesgue in 1904 in his work on the concept of an integral. He posed the problem to attribute to <u>every</u> bounded real function a real number, called the integral of this function, verifying some postulates among them the additivity

$$\int(f+g) = \int f + \int g,$$

and a condition on invariance with respect to translations (a postulate concerning convergent sequences of functions is of an other character). See [Lebesgue, 1903], Considering the characteristic function of a set Lebesgue showed that this problem is equivalent to the problem to attribute to <u>every</u> bounded set of $\mathbb{R}$ an additive measure invariant with respect to translations. Lebesgue could not give the solution of this problem. An analysis of the problem led him to follow the constructive way and he could only define a measure for a certain family of sets (L-measurable sets). In 1905 Vitali proved, using the axiom of choice, that there exist sets which are not measurable in the sense of Lebesgue.

In 1914 Hausdorff turned to this problem, refering to Lebesgue. He posed the following problem, called the <u>problem of measure</u>.

<u>It is asked to associate to every bounded set of $\mathbb{R}^n$ a real number satisfying the following conditions:</u>

1. $\mu(A) \geqslant 0$, $A \subset \mathbb{R}^n$,
2. $A \cong B \Rightarrow \mu(A) = \mu(B)$.

Here $\cong$ means congruence with respect to isometric maps.

3. $\mu(E) = 1$, <u>where</u> E <u>is the unit cube</u>.

This condition serves to eliminate the trivial solution.

4a. $\mu(A \cup B) = \mu(A) + \mu(B)$ <u>if</u> $A \cap B = \phi$,

<u>either</u>

4b. $\mu(\bigcup_i A_i) = \sum_{i=1}^{\infty} \mu(A_i)$ <u>if</u> $A_i \cap A_j = \phi$ $(i \neq j)$.

If 4b is verified the measure is said to be <u>completely additive</u>.

Hausdorff postulated the invariance with respect to isometries. Later on the problem was studied when only invariance with respect to translations is postulated. This gives some differences in the theory; we refer to [Hadwiger, 1957].

Let us consider now Hausdorff's results. Hausdorff proved that the problem has no solution when the condition is imposed that the measure shall be completely additive.

He proved this for $\mathbb{R}^1$ and then a fortiori there is no solution for $\mathbb{R}^n$. Let S^1 be a circle of radius $\frac{1}{2\pi}$. Consider the map mod $\mathbb{Z}$ which maps $\mathbb{R}$ onto S^1. Then there is a 1-1 map between S^1 and the interval $I = [0,1)$. $A \subset S^1$ and $A + \alpha \subset S^1$ (obtained by a rotation α) are congruent on S^1. On I there corresponds a decomposition in congruent parts. Consider $x \in \mathbb{R}$ and let α be irrational. Then consider $P_x = x + \alpha\mathbb{Z}$. With the decomposition on $\mathbb{R}$ corresponds on S^1 an inscribed polygon with infinitely many sides which is not closed. One has either $P_x = P_y$ or $P_x \cap P_y = \phi$. Using the axiom of choice choose a number in any P_x and let A_0 be the set of all these numbers. Put

$$A_m = A_0 + m\alpha.$$

One has

$$I = \bigcup_{m\in\mathbb{Z}} A_m,\ A_n \cap A_m = \phi,\ n \neq m.$$

Suppose μ is a completely additive measure and suppose $\mu(I) = 1$. One has then $\mu(A_m) = \mu(A_0)$. Put

$$I = [\bigcup_{i=1}^{n} A_i] \cup J.$$

Finite additivity gives

$$\mu(I) = \sum_{1=i}^{n} \mu(A_i) + \mu(J),$$

$$1 = \mu(I) \geqslant \sum_{i=1}^{n} \mu(A_i) = n\mu(A_0)$$

for all $n \in \mathbb{N}$. Then follows $\mu(A_0) = 0$ which contradicts complete additivity.

This is a sketch of Hausdorff's proof.

Now remains the possibility of impossibility of a finitely additive measure. Hausdorff considered also this problem. He proved: The finitely additive measure problem is unsolvable in $\mathbb{R}^3$.

The proof is based on a theorem in the theory of sets, also proved by Hausdorff in 1914. Later on this theorem became known as the paradox of Hausdorff. It is the following result:

There exists a decomposition of the sphere S^2 in $\mathbb{R}^3$ in four disjoint sets A, B, C, Q, where Q is denumerable, such that

$$A \cong B \cong C \cong (B \cup C),$$
$$S^2 = A \cup B \cup C \cup Q.$$

The unsolvability of the finitely additive measure problem in $\mathbb{R}^3$ follows from this result. First, Hausdorff proved, using finite additivity, that $\mu(Q) = 0$ and then

$$\mu(A) = \mu(B) = \mu(B \cup C),$$

hence

$$\mu(S^2) = 2\mu(A),$$
$$\mu(S^2) = 3\mu(A),$$

thus a contradiction. The measure problem on S^2 is therefore unsolvable. The unsolvability in $\mathbb{R}^3$ follows easily. Supposing that there is a solution in $\mathbb{R}^3$, the problem on S^2 would also have a solution: to see it associate to $E \subset S^2$ the volume of the cone generated by E and the center of S^2.

We will only give a rough sketch of the proof that Hausdorff gave of the paradox. Design by ϕ and ψ two rotations around axes through the origin respectively of π and $\frac{2}{3}\pi$ radians. The axes intersect in an angle φ which will be chosen in a suitable way. The rotations ϕ and ψ generate a non commutative group G:

$$\{1 \,|\, \phi, \psi, \psi^2 \,|\, \phi\psi, \phi\psi^2, \psi\phi, \psi^2\phi \,|\, \text{etc.}\}$$

The elements of G can be arranged in four classes:

$$\alpha = \{\phi\psi^{m_1}\phi\psi^{m_2}\ldots\phi\psi^{m_n} | m_i \in \mathbb{N}\}$$

$$\beta = \{\psi^{m_1}\phi\psi^{m_2}\ldots\psi^{m_n}\phi | m_i \in \mathbb{N}\}$$

$$\gamma = \{\phi\psi^{m_1}\phi\psi^{m_2}\ldots\phi\psi^{m_n}\phi | m_i \in \mathbb{N}\}$$

$$\delta = \{\psi^{m_1}\phi\psi^{m_2}\ldots\phi\psi^{m_n} | m_i \in \mathbb{N}\}.$$

with $\mathbb{N} = \{1,2,3,\ldots\}$

The essential point is to prove that it is possible to choose the angle φ in such a way that the only non-trivial relations between the elements of G are $\phi^2 = \psi^3 = 1$.
Then the transformations of the classes $\alpha,\beta,\gamma,\delta$ are all different. One defines a partition of G into three disjoint classes T_1, T_2, T_3; we shall not give the rather complicated definition. Q is defined as a certain set of fixed points of G; Q is denumerable. Consider on S^2 the equivalence relation $y \sim x$ as follows: "there is $\sigma \in G$ such that $y = \sigma x$". Choose in each of the induced equivalence classes a point and let V be the set of these points (axiom of choice). Then the following decomposition satisfies the conditions:

$$A = T_1V$$
$$B = T_2V,$$
$$C = T_3V.$$

Indeed:

$$\phi A = B \cup C \text{ and } A \cong B \cup C,$$
$$\psi A = B \quad \text{and } A \cong B,$$
$$\psi^2 A = C \quad \text{and } A \cong C,$$
$$S^2 - Q = A \cup B \cup C.$$

For a detailed proof see [Sierpinski, 1954]. There is a compact proof in [Jech, 1973].

This decomposition is non constructive and seems to contradict any physical picture. There are more such decompositions and they are called paradoxical decompositions. One may wonder how Hausdorff came to this result and its complicated proof. There are no indications about it in his book. Later on we will return to some other paradoxical decompositions.

We consider first some more developments in the theory of measure Hausdorff remarked that the problem of measure (finite additive) was still open for $\mathbb{R}^1$ and $\mathbb{R}^2$ and he could not give the answer. He remarked however that the structure of the group of transformations (isometric maps) in $\mathbb{R}^1$ and $\mathbb{R}^2$ made it impossible to treat these cases in the same way as in $\mathbb{R}^3$. Perhaps Hausdorff had already the idea that certain aspects of the theory of groups play a role in the solution of this problem. Later on (1929) Von Neumann studied these relations in an important paper; we shall come back to it.

These results of Hausdorff's were followed by a long series of papers on the measure problem. Especially in the Polish school this problem was studied. Many studies by Banach, Kuratowski, Tarski, Ulam. Their results were in close connection with paradoxical decompositions. Many of them have been published in Fundamenta Mathematicae.

First there is a paper by Banach in 1923. He proved the existence of a universal integral (an integral defined for every bounded real function), which is identical with the Riemann integral for every function for which this last integral exists, but which is not necessarily identical with the Lebesgue integral when this exists. He remarked that it is a consequence of this result that from the six conditions Lebesgue imposed on the integral the condition no. 6 (expressing a certain convergence) is independent from the five other conditions (this was an old problem). Furthermore it follows that Hausdorff's problem concerning the existence of a universal measure on $\mathbb{R}^1$ is thus also solved: a universal measure exists - in non constructive sense. He obtained the same result for bounded sets in $\mathbb{R}^2$. Hence, Hausdorff's open problems were solved in an affirmative sense. Banach's proof was long and complicated; he used transfinite induction. Banach returned to this problem in $\mathbb{R}^1$ in his classical book from 1932. There he considers S^1, which evidently is not a restriction. He proved that the existence of a universal measure on S^1 is a simple consequence of the theorem of Hahn-Banach; the proof is given in the same way as the existence of the generalized limit had been proved. It is a non constructive result:

"A tout ensemble A de la classe K on peut attribuer un nombre $\mu(A)$ de façon que les conditions suivantes (où A et B sont des ensembles arbitraires de la classe K) soient remplies :

1) $\mu(A+B) = \mu(A) + \mu(B)$, lorsque $AB = 0$,

2) $\mu(A) \geqslant 0$,

3) $\mu(A) = \mu(B)$ pour $A \cong B$,

4) $\mu(A_0) = 1$."[13]

In 1923 the so called theorem of Hahn-Banach was not yet known (Hahn 1927, Banach 1923).[14]

In 1929 Von Neumann published important results on the measure problem. Hausdorff already considered a group, generated by rotations, and this led him to the paradox, but he did not analyse the aspects of the theory of groups in connection with the fact that the problem of measure has a solution for $\mathbb{R}^1$ and $\mathbb{R}^2$ but not for $\mathbb{R}^n$, $n \geqslant 3$. One may perhaps say that there is not much reason to be astonished about this difference because the transformation group in $\mathbb{R}^3$ is larger then the groups in $\mathbb{R}^1$ and $\mathbb{R}^2$ and so therefore the condition on invariance in $\mathbb{R}^3$ is stronger than in $\mathbb{R}^1$ and $\mathbb{R}^2$. These aspects were analyzed by von Neumann. He studied the measure problem in abstract groups and he introduced the concept of a measurable group. A group G is called measurable when there exists on G a universal measure (finite additivity) which is invariant with respect to the transformations on G. He formulated conditions in order that a group G is measurable. Abelian groups, for instance, are measurable, and also are solvable groups. He proved that G is not measurable if G contains a free non-abelian subgroup; this is the situation of Hausdorff's paradox. It is not the place here to review all the results of this large paper. It is however important to mention that he introduced the concept of a "mean" (allgemeiner Mittelwert) on an abstract group G in the following way: Consider the family F of bounded real functions f,g,... on G. It is defined

$$af: (af)(\sigma) = af(\sigma),\ \sigma \in G,\ a \in \mathbb{R},$$
$$f+g: (f+g)(\sigma) = f(\sigma) + g(\sigma),$$
$$f_\tau : f_\tau(\sigma) = f(\tau\sigma),\ \sigma,\ \tau \in G.$$

A map M: $F \to \mathbb{R}$ is called a mean if the following conditions are satisfied

$$M(af) = aM(f),$$
$$M(f+g) = M(f) + M(g),$$
$$M(f_\tau) = M(f),$$
$$f \geqslant 0 \Rightarrow M(f) \geqslant 0,$$
$$f = 1 \Rightarrow M(f) = 1.$$

Von Neumann formulated conditions in order that a mean exists. It is a theory on discrete groups. Later on means on topological groups were studied. Then one studies the Banach space of the bounded continuous functions on the group. This leads to "amenable groups", a subject studied untill our time. It is a theory of axiomatic character where there is no problem of constructivity [Greenleaf, 1969].

Tarski studied the problem of the existence of a universal measure in a still more general situation [Tarski, 1938]. He considered a metrical space S and studied maps $f : X \subset S \to \mathbb{R}$, satisfying certain conditions in some analogy to those of measure theory; it is an algebraization of the problem. He proved, in a general way, that there are relations between the existence of universal measures and paradoxical decompositions. He proved the theorem: a universal measure exists if and only if a certain fixed set (the unit set) admits no paradoxical decompositions. See the comments in "Oeuvres de Banach" (1967) and [Dekker, 1958].

In view of the negative results on the existence of a completely additive universal measure for all $\mathbb{R}^n$ and the same situation as to finitely additive measures in $\mathbb{R}^n$, $n \geqslant 3$, the problem was studied when there is no condition on invariance. It is a subject studied till now, leading into the theory of sets and the foundation of mathematics.

Banach and Kuratowski (1929) proved that the universal problem (without invariance), completely additive, with the condition that every singleton has measure 0, admits only the trivial solution (identically 0). This was proved under certain set theoretical conditions, concerning cardinal numbers. Banach (1930) and Ulam (1930) generalized this result.

When there is only additivity for finite families there is a different situation. Tarski proved (1930) that there is a non-trivial solution for any arbitrary set. He proved that there is even a solution taking only the values 0 and 1. Compare [Monna, 1946].

Considering measures which only take the values 0 and 1 leads to characteristic functions of sets and then to set theoretical problems and questions in the domain of the foundation of mathematics. Ulam introduced the notion of a measurable cardinal number as follows.

Let S be an infinite set. The problem is whether there exists a map μ of the family of all subsets of S into $\{0,1\}$ such that

(i) $\mu(\{x\}) = 0$ for all $x \in S$,

(ii) $\mu(S) = 1$,

(iii) $\mu(\bigcup_1^\infty V_n) = \sum_{n=1}^\infty \mu(V_n)$ for any sequence (V_n),

$V_n \subset S$, V_n and V_m disjoint if $m \neq n$.

If cardinal α is called measurable if there is a set S of power α on which there exists such a measure. If there is no such set, α is called non measurable.
It is clear that one is then far away from a constructive theory of area, volume and measure. It is an open problem whether there exist measurable cardinals but one knows that, if they exist, they must be extremely large. It is a problem that is related to problems on the consistence of axiomatic systems in the theory of sets. We shall not treat this here. See [Luxemburg, 1962]

We already mentioned that there are relations between the measure problem and paradoxical decompositions. We mention some results which are rather spectacular.

In 1914 there was the first example of Hausdorff. His proof needed 3 pages. Sierpinski did much research in this domain. He proved Hausdorff's result in his book "On the congruence of sets and their equivalence by finite decomposition" (1954). He followed the way of Hausdorff but he needed 18 pages. See [Jech, 1973]; he needed again 3 pages. Jech gave it as an example of what can be reached with the axiom of choice. One obtains results that are far away from any intuition. They are called "paradoxical decompositions".[15)] It concerns, for instance, problems on the existence of proper subsets of a given set which are congruent to the whole set. There is also the problem of the decomposition of two sets in an equal finite number of disjoint parts mutual congruent. There is for instance the theorem: a ball can be decomposed in two disjoint parts such that each of these parts is congruent to the given ball.

It is trival that there are sets which are congruent to a proper subset: for instance a halfline. But for a bounded subset of a line this is impossible. However, in the plane there are such bounded sets.

One defines the notion "equivalent by finite decomposition" as follows:
The sets $A,B \subset \mathbb{R}^k$ are said to be equivalent by finite decomposition in n parts if there exist A_i, B_i, $i = 1,\dots,n$, $A_i,B_i \subset \mathbb{R}^k$ such that

(i) $A = \cup A_i$, $B = \cup B_i$,
(ii) $A_i \cap A_j = \phi\,(i \neq j)$, $B_i \cap B_j = \phi\,(i \neq j)$,
(iii) $A_i \cong B_i$, $i = 1,\ldots,n$.

Notation

$$A \underset{f}{=} B.$$

Remark. This notion of decomposition in disjoint parts should well be distinguished from the decompositions that are introduced in elementary geometry to define area and volume. There the condition that the parts should be disjoint is not imposed (square and triangle etc.). This is an ancient theory, studied by Hilbert, Dehn. See [van Dalen, Monna, 1972], [Hadwiger, 1957].

This notion of equivalence was defined by Banach and Tarski in 1924. They also considered denumerable decompositions. The results are curious. Two bounded polyhedra are always equivalent by finite decomposition, even if they have not the same volume. For polygons, in $\mathbb{R}^2$, the situation is different: two polygons are equivalent in this sense if and only if they have the same area. A ball is always equivalent to a cube. This difference between $\mathbb{R}^2$ and $\mathbb{R}^3$ is in close relation with the fact that the maeasure problem has a positive answer in $\mathbb{R}^2$, but a negative answer in $\mathbb{R}^3$. This is used in the proof for $\mathbb{R}^2$. The culmination is the paradox of Banach and Tarski:

The sphere S^2 can be decomposed in 10 disjoint parts such that by means of rotations 4 of these parts can be composed to form a sphere of the same radius, and the other 6 parts can be composed to form another sphere of the same radius.
There is an analogous result for a ball. There are several results of this kind in [Sierpinski, 1954]

Some comments.
We have treated the evolution of three subjects in mathematics: discontinuous solutions of a functional equation, the existence of a universal summation method for divergent series, the existence of

universal measures. For each of these subjects the evolution has led to non-constructive results, there are no effective examples.

Perhaps the paradox of Banach-Tarski-connected with measure theory - is the most impressive because the decomposition contradicts all geometric intuition. But is there reason to be more astonished at this result than at Cantor's result, proved by him at the beginning of his fundamental work, on the existence of a 1-1 correspondance between a segment and a cube ("equal number of points"), a result that is familiar to any mathematician? And is it more mysterious than the nature of the real numbers, about which we have some intuition? Perhaps one is less astonished at the existence of discontinuous solutions of the functional equation than at the paradoxical decomposition of S^2. Is geometric intuition of another level? However, the origin of these results is the same: they are consequences of the axiom of choice or one of its equivalents.

Is there a reason not to accept the axiom of choice in view of such results? It is a question that should be answered in the framework of axiomatics and the philosophy of mathematics. We will not treat it here.

The axiom of choice is generally accepted as a mathematical method - except in constructive theories - and its consequences must then be accepted too. But in history there was a different situation. In the first decades of our century there were many discussions about the axiom of choice. In particular in the "Ecole Française" objections were raised: Baire, Borel, Lebesgue. They concerned the question as to the real meaning of such ineffective results. There were questions on "définir un objet", "nommer un objet", "l'existence d'un objet". In books of the Collection Borel one finds notes on this subject; [Monna, 1973]. In the fourties Lebesgue formulated objections. In his paper "Les controverses sur la théorie des ensembles et la question des fondements" (Les entretiens de Zürich sur les fondements et la méthode des sciences mathématiques (1941); see Oeuvres scientifiques de Lebesque, Vol V, p. 287) he wrote, when discussing the theory of Artin, Schreier on formal real fields (where Zorn's lemma is used):

"S'il était vrai que, dans le cas particulier en question, le résultat dépende effectivement de cet axiome [axiom of choice], que signifierait le résultat? On prétend avoir démontré la possibilité de faire certaines constructions à l'aide de tels instruments , mais on ne nous dit pas comment faire ces constructions. Il reste possible qu'aucun homme ne sache jamais les faire; on peut même imaginer que l'on démontre que jamais aucun homme ne pourra indiquer la loi de succession des constructions à effectuer pour résoudre le problème, pour réaliser cette construction dont on a dit avoir démontré la possibilité".

Such a criticism is directed towards results that are obtained by certain mathematical methods, and therefore also towards these methods itself.

When we discussed universal summation methods we wondered what would have been the attitude of Borel with respect to the solution by means of the generalized limit. Now we can ask the same question as to the universal measure problem. Would the non-effective solutions we treated before have satisfied Lebesgue? Were they acceptable for Borel and Lebesgue as semi-intuitionists? [Bockstaele, 1949].

In Part I we mentioned the criticism of Leibniz on the work of Descartes; also Poincaré criticized it. But there it concerned some objections against too automatical methods, not against the results. The criticism on the axiom of choice and the results which are proved by means of it, is of a very different kind: here fundamental objections are concerned. They are connected with standpoints on the foundation of matheamtics.

In the next sections we will consider some more examples of the evolution of problems in analysis. They also concern problems on existence, but there is some difference with the preceding examples. We consider the existence of certain functions with some singular properties, for instance continuous without having a derivative, functions in relation to Fourier series etc. They have all in common that effective examples were known, sometimes already before the creation of the theory of sets and functional analysis. When these theories were introduced, one could ask for information on the totality of such more or less singular functions. The properties of these spaces of functions were studied and this resulted in non-effective existence theorems, and the questions if some special property is normal or exceptional?

2.4 Continuous functions without derivative

After long years of discussions on the relation between continuity and existence of a derivative for real functions, Weierstrass was the first to give an example of a continuous function without derivative. In the course of time several other examples were given. For example

$$f : f(x) = \sum_{0}^{\infty} 2^{-n}\sin(2^n\pi x).$$

Riemann gave

$$f : f(x) = \sum_{n=1}^{\infty} n^{-2}\sin(n^2 x).$$

See also the interesting article [Neuenschwander, 1978].

Even in the period when such problems were studied with the modern methods of topology and functional analysis, more examples were given. In 1942 Lebesgue gave the example

$$f : f(x) = \sum_{n=1}^{\infty} 2^{-n}\sin 2^{n^2}x,$$

and for the proof of the non-existence of a derivative he needed only one page.

A geometrical construction of such a function was given by Von Koch in 1904.[16)]

In the twenties of our century a new direction of research developed under the influence of topology and in particular of functional analysis which was in its first years. The problem changed. The question was posed to prove the existence without refering to special examples. In this way one could hope to find an answer to the question whether continuous functions without derivative are "exceptional" or in some way "normal". The problem, thus modified, must be studied in the framework of the theory of the space of all continuous functions. Topology and functional analysis are the means.

As to topology the notion of <u>category</u> plays a role in this development. A set V of a topological space is called of <u>first category</u> if it can be represented as a countable union of nowhere dense sets. If it can not be represented in this way it is said to be of the <u>second category</u>. These notions were introduced by Baire in his research on certain classifications of real functions. There are certain analogies between sets of first category and sets of measure zero; see [Oxtoby, 1971]. The notion of category is used in some fundamental questions in functional analysis. For the work of Baire see [Dugac, 1976].

From functional analysis certain theorems on sequences of linear operators are called in aid.

One considers the Banach space C of the real continuous functions on

[0,1] with the norm

$$\|f\| = \max_x |f(x)|.$$

Then the problem is to study the subspaces of C the elements of which are the functions that have or have not a certain supplementary property: existence or non-existence of a derivative (right or left) etc. In particular Polish mathematicians studied in this area. We mention some of the results.

Using theorems on linear operators Banach proved in 1926 the existence of a continuous function which has no derivative on a set of positive measure. He considered a sequence of linear operators $(T_n)_n$

$$T_n(f) = \frac{f(x+h_n)-f(x)}{h_n}, \quad f \in C$$

where $\lim_{n\to\infty} h_n = 0$, and then applied some general theorems. See [Banach, 1932] where one finds more applications.

This first paper was followed by several others. There is an interesting paper from Steinhaus "Anwendungen der Funktionalanalysis auf einige Fragen der reellen Funktionentheorie" (1929a). In this paper the author is not only concerned with the existence of a derivative. There is, for instance, the section "Nichtdifferenzierbare Functionen mit vorgeschriebenem Stetigkeitsgrad". There are necessary and sufficient conditions for the existence of continuous functions $x(\tau)$ such that a certain integral

$$\int_0^1 |x(\xi+\tau)-x(\xi)|.\omega(\tau)d\tau$$

is infinite for nearly all ξ and a given function ω. Integrals of this kind play a role in the theory of Fourier series (for $\omega(\tau) = \tau^{-1}$).

Steinhaus proved that a certain class of continuous functions without derivative is a set of second category in C.
There are several papers of this kind in the journal Studia Mathematica. Mazurkiewicz (1931) proved the following theorem, using only topological methods in the Banach space C:

"Soit C l'espace des fonctions continues de periode 1. L'ensemble N de fonctions de C, qui n'admettent pas de dérivée (finie) à droite dans aucun point, est de seconde catégorie dans C, son complémentaire étant de de première catégorie".

Banach generalized this result, using the theory of linear operators [Banach, 1931].

For those who are interested in the evolution of the theory of real functions in this period there are several interesting papers in the above mentioned journal ("Über die Höldersche Bedingung"; Integrale vom Dinischen Typus"; etc.).

In his book on topology Kuratowski (1933,1958) treated this problem as an application of topological and set theoretical methods. He proved the existence of a continuous function such that in no point the two right derivatives ($\overline{\lim}$, $\underline{\lim}$) are finite. He used a theorem of Baire's which he formulated as "Tout ensemble de première catégorie est un ensemble frontière" and then remarks that the existence in a point of two finite right derivatives is equivalent to the existence of a number $n \in \mathbb{N}$ such that

$$\left|\frac{f(x+h)-f(x)}{h}\right| \leqslant n \qquad \text{for all } h > 0.$$

Some further considerations led him to say that the continuous functions which have a derivative are "exceptions dans la totalité des fonctions continues".[17)]

Kuratowski proved some more theorems an the totality of continuous functions. For example: "l'ensemble des fonctions continues qui possèdent la dérivée droite infinie dans une infinité dénombrable de points est analytique".

The strong influence of the modern methods in functional analysis - algebraical and topological aspects - is evident in these developments.

2.5 Fourier series

We will consider only one aspect of the long history of the theory of Fourier series, problems on the divergence of certain series and we will only make some remarks because it is difficult to detach this subject from the history of Fourier series in his totality.

Given a function f, one has studied since Fourier series of the form

$$\frac{a_0}{2} + \sum_{k=1}^{\infty} (a_k \cos kx + b_k \sin kx),$$

where the coefficients a_k, b_k are derived in the well known way from f. In particular one studied continuous functions. For a given function f the problem is to find conditions on f such that the corresponding Fourier series converges to f. Dirichlet gave some conditions under which such a representation is true; they concern conditions on piecewise monotonicity. He already considered the case where f has an infinite number of maxima and minima. In particular it was believed that for any continuous function f the corresponding Fourier series was convergent to f; however no proof had been given. Riemann studied this problem and he published his results in a fundamental paper. In his opinion it was sufficient to study functions with only a finite number of maxima and minima in the interval under consideration because other functions did not present themselves in nature. It was a physical argument, insufficient from the side of mathematics, but insufficient too from the standpoint of physics.

At this point du Bois-Reymond attacked the problem. We will take his work as a point of departure to illustrate also in this domain the evolution from the constructive phase (examples) into the existential phase. In a paper from 1873 [du Bois-Reymond, 1873 a] he remarked that Riemann, with his remark on functions that are met in nature, certainly had not in view functions which, with an infinite number of oscillations, tend asymptotically to 0. He came to the conclusion that a general theorem on the possibility of the representation of an arbitrary continuous function by means of its corresponding Fourier series could not be true. His efforts aimed to find conditions under which such a representation is true. He wrote:

"Einige Ueberlegung ergab bald die Bedingungen, unter welchen die Fourierschen Reiche bei durchgängicher Endlichkeit und Stetigkeit der darzustellenden Funktion für einzelne Argumentwerthe keine endliche bestimmte Summe haben kann".

He proved the following result:

"Die nicht darstellbaren stetigen Funktionen sind also in dieser ihrer einfachsten Erscheinung Funktionen der Form

$$f(x) = \rho(x) \sin \psi(x)$$

wo $\rho(x)$ mit x ohne maxima verschwindet, und $\psi(x)$ bei gegen Null abnehmendem x mit unendlich vielen Maximis stetig unendlich wird".

This was the first example of a continuous function whose corresponding Fourier series is not every where convergent.[18)]

Du Bois-Reymond continued this work in 1876 in "Abhandlung über die Darstellung der Funktionen durch trigonometrische Reihen". The subject of the research is "Untersuchungen über die Konvergenz und Divergenz der Fourierschen Darstellungsformeln". It is a complicated work of 123 pages and it is impossible to review it in a few lines. The problem of convergence and divergence of Fourier series is reduced to problems concerning convergence and divergence of certain integrals. We quote a passage:

"Die Aufgabe, den

$$\operatorname*{Limes}_{h=\infty} \int_0^\alpha d\alpha\; f(x)\, \frac{\sin\alpha h}{\alpha}$$

für $f(x) = \rho(\alpha) \cos \psi(\alpha)$, zu untersuchen, führt, wie viele andere Konvergenzprobleme auf die Aufgabe, die Stärke des Null - oder Unendlichwerdens nicht explizite gegebener Funktionen zu bestimmen". (1.p. 18). To perform this du Bois-Reymond developed a systematic theory of the increasing and decreasing of functions which he called "Infinitärkalkül". It is a study of asymptotic properties of functions. In the following we will come back to it.

In the chapter IV "Darstellung der Bedingungen, unter denen die Fourierschen Reihen divergieren" he gave examples, even an example of a function whose Fourier series diverges on a dense set. We quote this passage:

"Mann kann übrigens aus der Funktion $\rho(x) \sin \psi(x)$ noch andere nicht darstellbare Funktionen erhalten, deren Entwicklung nach Fourierschen Reihen oder deren Ausdruck durch ein Fouriersches Integral in jedem kleinsten Intervall unendlich wird. Dann bilden wir die Funktion

$$f(\sin px) = \rho(\sin px) \cos \psi(\sin px)$$

mit der Bestimmung $f(0) = 0$, so ist diese Funktion für jedes x stetig. Gleiches gilt von dieser:

$$F(x) = \sum_{p=1}^{\infty} \mu_p \,\rho(\sin px) \cos \psi(\sin px).$$

Setzt man endlich

$$H(x) = \lim_{h\to\infty} \int_{-A}^{B} d\alpha F(\alpha) \frac{\sinh(\alpha-x)}{\alpha-x},$$

so hat die Funktion in jedem kleinsten Intervall einen Punt, in dem sie unendlich ist, oder genauer, die μ_p können stets so bestimmt werden, dasz dies der Fall ist". (l.c. p. 101)

After du Bois-Reymond several more, and more simple, examples of continuous functions were given, whose Fourier series is not always convergent.

Lebesgue gave the following example in his book "Leçons sur les séries trigonométriques" (chapter "Existence de séries de Fourier divergentes"; Collection Borel), referring to du Bois-Reymond:

"Soient $c_1, c_2, \ldots$ des nombres tendant vers zéro; soient $n_0, n_1, n_2, \ldots$ des entiers impairs croissant indéfiniment, posons

$$a(k) = n_0 n_1 n_2 n_3 \ldots n_k,$$

et désignons par I_k l'intervalle $[\frac{\pi}{a(k-1)}, \frac{\pi}{a(k)}]$.
Définissons une fonction continue par la condition que l'on ait $f(x) = f(-x)$ et, dans I_k, en conservant les relations des nos 33 et suivants,[19)]

$$\phi(t) = c_k \sin[a(k)t]\frac{\sin t}{t} .$$

f(x) est ainsi entièrement determinée pour x assez petit, on la définira ailleurs par la condition qu'elle soit partout continue et de période 2π". (l.c.p. 85).

Lebesgue proves by the approximating of certain integrals that c_k and n_k can be chosen in such a way that the Fourier series of f diverges for $x = 0$.

He then posed the following questions:

"Existe-t-il des fonctions continues dont la série de Fourier est divergente partout?

"Existe-t-il des fonctions continues dont la série de Fourier est partout convergente, sans être uniformément convergente dans aucun intervalle?" (l.c.p. 89).

In contrast to later developments where such problems were studied in the sense of pure existence with the methods of functional analysis, in the year in which Lebesgue posed these questions (1906) this can only mean that one asked for effective, concrete, examples.

A very simple example of a continuous function whose Fourier series diverges in 0 was given by the Dutch mathematician [Wolff, 1931]. Widom (1969) gave also an example of such a function. But he also gives a proof of the existence of such a function, using the theory of linear operators in Banach spaces (theorem of Banach-Steinhaus); it is then a non constructive method.

Zygmund gave in his book on trigonometrical series [Zygmund, 1968] several examples of functions whose corresponding Fourier series has certain singularities, not only in one point but much more complicated singularities. These effective examples are complicated. For a general theory of Fourier series Zygmund used the theory of linear operators.

After Lebesgue's fundamental work, the Lebesgue integral was introduced in the theory. Instead of pointwise convergence one studied the aspects of convergence or divergence almost everywhere. Other concepts of convergence were studied, for instance convergence in mean, introduced by the works of Fischer and Riesz.

In the twenties the methods of the theory of linear operators in Banach spaces were applied to the problem of the existence of divergent Fourier series. In these years function spaces were introduced and pointwise convergence was replaced by convergence in norm. With these methods the existence of not everywhere convergent Fourier series was proved. It concerns pure existence, not demonstrations of the existence by means of examples. It is the way from construction to solvability.

In this period there are many papers on this subject in Studia Mathematica and Fundamenta Mathematicae. There are works of Banach, Steinhaus, Orlicz, Zygmund. Often the theorem of Banach-Steinhaus on sequences of linear operators in Banach spaces is applied: in a suitable way such a sequence is defined and it is then proved that there is a function f for which one of these operators is not bounded.

By means of a general form of the principle of the condensation of singularities in Banach spaces - originally from Hankel (See [Monna, 1973 a]) - the case of divergence in one point could be extended to divergence on larger sets (See [Yosida, 1968]; for a simple proof see [Wilanski, 1964], p. 118). There are generalizations to orthogonal systems of functions which are more general than the trigonometrical system (See Orlicz, "Beiträge zur Theorie der Orthogonalentwicklungen"). Banach considered orthogonal systems in some function spaces. He proved, for instance, "l'existence d'une fonction intégrable x(t) telle que l'on ait

$$\overline{\lim_{n\to\infty}}\left|\int_\alpha^\beta S_n(t)dt\right| = \infty$$

dans tout intervalle $[\alpha,\beta] \subset [0,2\pi]$", where (S_n) is the sequence of partial sums.

Still another problem is the existence of functions f in connection with convergence or divergence of series of the form

$$\sum_{n=1}^{\infty} (|a_n|^{\lambda_n} + |b_n|^{\lambda_n}),$$

where a_n and b_n are the Fourier coefficients of f and (λ_n) is a certain sequence of numbers $\geqslant 0$ (there are relations with theory of lacunary trigonometrical series). The theory of linear operators is here an apparatus.

In a paper of 1923 ("Une série de Fourier-Lebesgue divergente presque partout"; Fund. Math. 4) Kolmogoroff gave "un example d'une fonction sommable (c'est-à-dire: intégrable au sens de M. Lebesgue) dont la série de Fourier diverge presque partout (c'est-à-dire: partout sauf aux points d'un ensemble de mesure nulle)". It is an effective example in sofar as one wants to call statements "presque partout" really effective). He did not use the theory of linear operators and he remarked that "les méthodes employées ici ne permettent pas de construire une série de Fourier divergente partout".

In 1926 [Kolmogoroff, 1926] he succeeded in proving that there exists a function $f \in L^1$ whose Fourier series is everywhere divergent. He gave an effective example of such a function.

There is a relation with the concept of category in the sense of Baire (see 2.4):

<u>The set of functions</u> $f \in L^1$ <u>whose Fourier series is divergent in</u> L^1 <u>is a residual set in</u> L^1.

A residual set is defined as follows. A topological space E is called a Baire space if every nonempty open set of E is of second category. In a Baire space the complement of a set of first category is called residual. For this result see [Banach, 1967, p. 311].

In 1966 Carlson proved that the Fourier series of any $f \in L^2$ is almost everywhere convergent. Here problems on convergence than of divergence, play an important role.[20)]

2.6 Analytic continuation

We will discuss some points in the theory of analytic continuation for analytic functions of a complex variable.

In 1880 Weierstrass gave the first example of a function f, defined by a power series with finite radius of convergence for which any point of this circle is a singular point. In this situation the function can not be continued outside the circle; the circle of convergence is a limiting circle for this function (in French a "coupure"). It is the series

$$\sum_0^\infty b^n z^{a^n},$$

where $0 < b < 1$, a entire $\geqslant 2$, $ab \geqslant 10$.

The theory of singular points of analytic functions was developed and more examples of series whose circle of convergence is a limiting circle were given. There is a theorem of Hadamard (1892):

"Considérons la série

$$f = \sum_0^\infty a_n z^n.$$

Soit (n_k) une suite de nombres entiers telle que

$$n_{k+1} - n_k > Cn_k,\ k = 0,1,2,\ldots,$$

C'étant une constante indépendante de k. Alors la frontière du disque de convergence de f est une coupure pour f si $a_n = 0$ pour $n \notin (n_k)$". A series of this type is called lacunary. A theory of these series has been developed; there are, for instance the works of Fabry. See a book by Bieberbach[1955], which contains an extensive bibliography.

First there were examples, theorems of constructive character; one studied special situations where results on the existence of singular points of certain classes of analytic functions were obtained. But already Fabry and Borel studied the set of all power series with respect to the existence of power series with a limiting circle. About 1896 they expressed as their opinion that the property of a series to have a continuation is exceptional and that the existence of a limiting circle is the "normal" situation. To make such a statement the concepts "exceptional" and "normal" must be defined. This leads to theories of a structure different from constructivity: they are concerned with questions of pure existence. One wants information of the frequency of the series which have a limiting circle: are there many of such series? It seems that such a question could only be posed under the influence of set theory. We quote a passage from [Borel, 1901]:

"Pour donner une application de la méthode précédente, nous allons démontrer qu'une série de Taylor admet, en général, le cercle de convergence comme coupure.

Cette proposition a été énoncée pour la première fois par Pringsheim; il est d'ailleurs clair qu'elle n'a un sens précis que si l'on définit les mots en général qui figurent dans son énoncé et qui n'ont, en eux-mêmes, aucune signification déterminée. Nous adopterons la définition suivante: une série de Taylor sera dite générale si la valeur du $n^{\text{ième}}$ coefficient est indépendante de la valeur des coefficients précédents" (l.c.p. 147).

Steinhaus studied this problem from the point of view of probability in a paper from 1929 "Über die Wahrscheinlichkeit dafür dass der Konvergenzkreis einer Potenzreihe ihre natürliche Grenze ist". The problem is then reduced to a theory of measure in a suitably defined space. There is the following result.

Consider the series

$$\Sigma a_n z^n, \; a_n = |a_n| e^{2\pi i x_n}, \; x_n \in \mathbb{R}.$$

Suppose $|a_n|$, $n = 0,1,2,\ldots$ are given such that

$$\overline{\lim_{n\to\infty}} |a_n|^{1/n} = 1.$$

The x_n are supposed to be variable. Consider the torus $T : x = (x_0, x_1, x_2, \ldots)$ taking x_n mod $\mathbb{Z}$. Introducing an appropriate measure in T it is proved: the power series which admit a continuation form is T a set of measure 0.

There are also results of a topological character. Introducing a suitable topology in the set of all power series with radius of convergence equal to 1, one proves that the subset of the series which can not be continued is dense in this set.

Remarks

The preceding examples illustrate the evolution from the constructive phase into the existential phase. The concept of pure existence is one of the characteristics of the modern phase of mathematics. It would not be difficult to give more examples of this evolution. For instance the problem of moments (effective calculation of solutions and conditions on existence); there is the same situation in the theory of quasi-analytic functions [Carleman, 1926]. The theory of partial differential equations gives an other example. In the classical situation the theory was concerned with explicit solutions in special cases. Only later on the direction turned to conditions on existence in the general case. Potential theory - the study of the solutions of the equation of Laplace $\Delta u = 0$ - is perhaps an exception. Here the question of pure existence - rather than constructivity - was already studied in an early phase. We mention the classical Dirichlet problem [Bertin, 1978].[21)]

In linear algebra there are elementary examples. For instance questions on explicit solution of systems (finite or infinite) of linear equations and results on the dimension of the space of the solutions.

The constructive phase should not be considered as a closed phase. Let us mention a problem posed by Banach in the early years of functional analysis. He posed the question whether every separable Banach space has a base (we shall not give the definition. Compare the algebraic base of vectorspaces; see 1.4 (ii). Here it is a topological base). The question has long been open. Only in 1973 (Acta Math. 13, 309-317) Enflo gave an concrete, effective, example of a Banach space which has no base. Thus Banach's problem had been answered. But then new problems follow: the problem to characterize by intrinsical conditions the class of Banach spaces that have a base and the class of those that have not. Here construction is no more in the foreground: there is the problem of solvability. Compare the base problem is non-archimedean analysis [Monna, 1970], [van Rooy, 1978].

In number theory there are also questions on solvability - rather than finding solutions. There is, for instance, the problem of finding integer solutions of polynomial equations (quadratic forms; Fermat) or certain congruences. In some situations the solvability is equivalent with the solvability of some related equations in the p-adic numbers for all prime p. There are recent developments in this domain. It goes too far to review them here. We refer to a paper by Stephen Gelbart, "An elementary introduction to the Langlands program" (Bull. Am. Math. Soc. 10, no. 2 (1984) 177-219).

2.7 Problems on classification

The next two subjects are of a somewhat different character. They concern some problems of classification in analysis; set theoretical aspects of existence and construction are connected with them. These subjects were already studied towards the end of the 19th century. In particular the first subject had an important influence on mathematics of our time.

Note that, in principle, problems concerning a classification of real functions were not necessarily reserved for the years after Cantor. There is, for instance, a classification in simply continuous functions, differentiable functions (one or more times) etc. This is a rather trivial classification and furthermore, considered in classical analysis, it concerns rather properties of individual functions than a partition in classes. Function spaces were reserved for the period beginning with Cantor.

(i). In the section on Fourier series we already mentioned du Bois-Reymond and his "Infinitärkalkül". We shall give some information about this Kalkül and its consequences.

On the one hand this theory can be considered as a classification of some classes of real functions, on the other hand it is a theory on the orders of magnitude for the asymptotic behaviour of functions, a measure-theoretical question.

Consider the family of the increasing functions $\mathbb{R}^+ \to \mathbb{R}$ such that for any f of this family

$$\lim_{x\to\infty} f(x) = +\infty.$$

One studies these functions with respect to their mutual asymptotic relations for $x \to \infty$.

Following du Bois-Reymond it is said that, for two functions f and g of this family, the degree of increasing of f is superior to the increasing of g if

$$\lim_{x\to\infty} \frac{f(x)}{g(x)} = +\infty .$$

If this limit exists and is equal to zero, it is said that the degree of increasing of f is inferior to the increasing of g. If the limit exists and is $\neq \infty$, $\neq 0$, it is said that f and g have equal degree of increasing. Du Bois-Reymond used the following notation:

$$f \succ g,\ f \prec g,\ f \sim g.$$

For typographical reasons we write with Borel $f(x) > g(x)$, $f(x) < g(x)$. If the said limit does not exist f and g are said to be incomparable with respect to their asymptotic behaviour.[22] Du Bois-Reymond, and later on Borel, considered only functions that are comparable. In this way a notion of order of magnitude, types of infinity, is introduced. It is a classification of the family of real functions with respect to their asymptotic behaviour. Du Bois-Reymond developed a calculus for these orders of infinity and he compared it to the arithmetic of numbers:

"In den erwähnten Untersuchungen habe ich die verschiedenen Unendlich der Funktionen nach ihrer Grösse unterschieden, so dass sie ein Grössengebiet (das infinitäre) bilden...". [du Bois-Reymond, 1875]. We discovered that there are, however, some essential differences and that a calculus of magnitudes of infinity is much more complicated. He studied the following problem: is it possible to define a denumerable sequence of increasing real functions $\phi_1, \phi_2, \ldots$ verifying

$$\phi_1(x) < \phi_2(x) < \phi_3(x) < \ldots$$

such that for any given increasing function ψ there is an index $m \in \mathbb{N}$ such that

$$\psi(x) < \phi_m(x)?$$

He proved that such a sequence (ϕ) does not exist and established the following theorem (we quote it in the form in which Borel gave it (1898, Note II):

"Étant donnée une suite dénombrable quelconque (ϕ) de fonctions croissantes, on peut trouver effectivement une fonction croissante

$\psi(x)$, telle que l'on ait, quelque soit m

$$\psi(x) > \phi_m(x)."$$

Borel compared this result with the role of the axiom of Archimedes in a theory of modes of increasing. The theory du Bois-Reymond developed is of classical technical character; see the references. He created his theory for the needs of classical analysis:

"Die vorliegende Untersuchung ist fast in allen ihren Theilen aus dem Bedürfniss entsprungen, gewisse Sätze über Convergenz und Divergenz von Fourier'schen Reihen allgemein und unter genauer Feststellung ihres Gültigkeitsbereiches zu beweisen". [du Bois-Reymond, 1875].[23)]

We confine ourselves to some remarks on this work. To illustrate "increasing" and "decreasing", du Bois-Reymond mentions the elementary functions: powers, the exponential function, logarithm and their iterations. We quote again (1875):

"Diese als Zahlen dienenden Funktionen erstrecken sich, dem heutigen Stande entsprechend, von beliebig hoch getürmten Exponential-funktionen:

$$e^{e^{\cdot^{\cdot^{\cdot^{e^{x}}}}}}$$

bis hinunter zum unendlich beliebig oft wiederholter Logarithmen

$$\log(\log(\ldots\log x)\ldots) = ll\ldots x."$$

The problem was whether with these functions the whole domain of asymptotic behaviour had been exhausted.[24)]

Du Bois-Reymond studied the relations between a function and its derivatives with respect to their asymptotic behaviour. He considered expressions like

$$f(x+a) - f(x) \text{ and } \frac{f(x+a)}{f(x)} \quad .$$

A problem is "die Bestimmung des Unendlich nicht explicite gegebener Funktionen". Some chapters are: "Ueber Infinitärtypen and infinitäre Gleichheiten"; "Integrierbarkeit infinitärer Gleich- und Ungleichheiten".

Borel wrote on this subject in some of his books. He considered the theory from a somewhat different point of view. He asked whether it is possible to constitute for these types of asymptotical behaviour a theory in some way analogous to the measure theory for length of intervals ("mesure des longueurs"). He remarked that du Bois-Reymond had proved that such an analogy is not possible. Borel posed the problem to form a scale for these asymptotic types ("une échelle de types croissants"). See his Note II "La croissance des fonctions et les nombres de la deuxième classe" in (1898). Such a scale should be a set E of functions ϕ such that (i) (i) any two functions of E are comparable and (ii) given an increasing function ψ there exists in E a function ϕ whose increasing is superior to that of ψ. It follows from du Bois-Reymond's theorem that such a scale cannot be denumberable. Borel writes:

"Nous allons néanmoins chercher à en construire un, ne serait-ce que pour donner une idée des difficultés que l'on rencontre lorsque l'on veut définir un ensemble non dénombrable sans faire appel à l'intuition du continu". This construction is a transfinite process: there are the types $1,\dots,n,\dots,\omega,\omega+1,\dots,\omega^2,\dots$. There are transfinite difficulties in this process. They led Borel to introduce the concept of "fonction idéale", defined in the system of increasing functions. He wrote about this in his Note II mentioned before.[25] This passage is very interesting and it is worthwhile to quote it:

"Designons par (S) la suite transfinie de fonctions que nous venons de définir. Cette suite a les propriétés fondamentales suivantes:

1. Deux fonctions quelconques sont comparables entr'elles.
2. Étant donnée une fonction quelconque de S, il y en a une qui la suit immédiatement.
3. Une fonction quelconque étant donnée, il y a dans S une infinité de fonctions qui lui sont supérieures.

Cette suite S possède ainsi quelques-unes des propriétés fondamentales de la suite des nombres entiers. On peut en déduire un ensemble Σ qui possédera de même quelques-unes des propriétés fondamentales de l'ensemble des nombres rationnels. Il suffit pour cela de procéder exactement de même que pour obtenir ce dernier ensemble: la considération des fonctions inverses des fonctions de S donnera des fonctions croissant de moins en moins vite; en les multipliant par la variable x, on aura des fonctions à croissance plus rapide que x, mais aussi peu que possible; par le procédé de l'itération, répété transfiniment, on formera un ensemble Σ

de fonctions à croissance plus rapide que x et moins rapide que celle de x^2, par exemple. Cet ensemble Σ sera d'ailleurs de seconde puissance et aura les propriétés suivantes:

1. Deux fonctions quelconques de Σ seront comparables entre elles.

2. Si l'on considère une fonction quelconque, comparable à toutes les fonctions de Σ, et dont la croissance est comprise entre celle de x et celle de x^2, la croissance de cette fonction sera entièrement définie par la suite transfinie des fonctions de Σ dont la croissance est plus grande et la suite transfinie des fonctions de Σ dont la croissance est plus petite.

D'ailleurs, réciproquement, tout mode de division des fonctions de Σ en deux ensembles, tels que chaque fonction du premier ait une croissance inférieure à toute fonction du second définit un mode de croissance, mais à ce mode ne correspond pas nécessairement une fonction. De même que toute division en deux classes de l'ensemble des nombres rationnels définit une grandeur, mais à cette grandeur ne correspond pas nécessairement un nombre, tant que l'on n'a pas convenu d'appeler nombre les nombres incommensurables. Il y aurait lieu de même ici, pour complèter ce continu fonctionnel, analogue au continu linéaire, d'introduire des fonctions idéales, analogues aux nombres incommensurables.

Une fonction idéale, c'est un mode de division de l'ensemble Σ en deux classes telles que toute fonction de la première classe soit inférieure à toute fonction de la seconde classe et telles, de plus, qu'il n'y ait pas dans la première classe de fonction supérieure à toutes les autres, ni dans la seconde classe de fonction inférieure à toutes les autres.

Par exemple, rangeons dans la première classe les fonctions $\phi(x)$ telles que l'intégrale $\int_0^\infty \frac{dx}{\phi(x)}$ n'ait pas de sens et dans la seconde les fonctions telles que l'intégrale $\int_0^\infty \frac{dx}{\phi(x)}$ ait un sens. Nous aurons ainsi défini une fonction idéale telle que, si on la désigne par $\theta(x)$, l'intégrale

$$\int_0^\infty \frac{dx}{\theta(x)}$$

est à la fois pourvue et dépourvue de sens. Cette propriétie est tout aussi absurde pour celui qui regarderait $\theta(x)$ comme une véritable fonction, que la suivante, pour celui qui ne considérerait que de véritables nombres: il existe un nombre dont le carré est égal à 2". [26)]

After this passage there is an exposition on "la formation des nombres plus grands que l'infini introduits par M.G. Cantor". Borel shows the analogy between certain aspects of the theory of Cantor and the theory of du Bois-Reymond. There are several more interesting remarks in this note (on "indéfiniment" and "transfiniment") on fundamental discussions about set theoretical questions in these years.[27] Here is again the question we mentioned in some preceding sections: how to explain the introduction of "fonctions idéales" with the standpoint of Borel to consider only objects that can be calculated?[28]

After 1898 Borel continued his research on modes of increasing. He studied applications to analysis. See for instance the chapter "Différentiation et intégration des ordres de coissance" in [Borel, 1910]. There are relations with the theory of asymptotic series.

But we shall not follow this direction. We prefer to mention some developments in contemporary mathematics. The work of du Bois-Reymond has connections with the concepts of infinitely large and infinitely small, the infinitesimals from Leibniz. From an asymptotical point of view decreasing functions verifying

$$\lim_{x\to\infty} f(x) = 0$$

can be considered as infinitesimals. In modern mathematics the development has led to the so called "non-standard analysis", created by A. Robinson. This theory is based on an extension $^*\mathbb{R}$ of the field $\mathbb{R}$ which contains infinitely small and infinitely large elements. However, it is a non-effective theory, based on Zorn's lemma and ultrafilters. It is important in the foundation of mathematics. See [Robinson, 1966].

(ii). One can consider these works of du Bois-Reymond as a classification of a class of functions with respect to their asymptotic behaviour. There may be some doubt whether du Bois-Reymond himself regarded these results from this point of view. His aim was to study the convergence or divergence of some integrals and for this he needed a study of the asymptotic behaviour of the functions in question.
It is quite otherwise with some works of Baire: he gave a transfinite classification of real functions. This classification was connected with the problem of the representation of functions, for example the possibility of representing certain discontinuous functions in the form of a series of polynomials. About 1899 Baire proposed the following classification:

the elements of class 0 are the continuous functions; the elements of class 1 are the functions which are limits of functions of class 0 without being functions of class 0.
In an analogous way are defined the functions of the classes 2,3,..., and this is continued to define the transfinite classes $\omega,\omega+1,\dots,\omega^2,\dots$.

In later years the following problems were posed:
1. Is the class α nonempty for any α?
2. The problem to give an effective example of a function of any of these classes.
3. Characterization of the functions of any of these classes.

Baire succeeded in giving a characterization of the functions of the first class. Therefore he introduced the concepts of sets of first and second category (see 2.4). He obtained also some results for the functions of an arbitrary class. See [Baire, 1905].

We confine ourselves to some remarks and refer to [Dugac, 1976] for more information.

It has been proved, on the one hand, that none of these classes is empty but, on the other hand, that there are functions which belong to none of these classes [de La Vallée Poussin, 1916]. This classification has some constructive aspects (certainly with Baire), but the problem of effectivity has been the subject of many discussions, where in particular Borel must be mentioned. There are examples of functions of class 2, e.g. Dirichlet's function, taking only the values 0 and 1. In note III [Borel, 1905] "Sur l'existence des fonctions de classe quelconque" Borel asked "si la classification de M. Baire n'est pas purement idéale, c'est-à-dire s'il existe effectivement des fonctions dans les diverses classes définies par M. Baire". He insisted on "ce que l'on doit appeler une fonction définie". It is the question of defining an object and the existence of this object. He remarked that it is easy to see "qu'il existe des fonctions de classe supérieure à un nombre quelconque (fini ou transfini) donné d'avance". Therefore he used a reasoning on cardinals of families of functions, a set theoretical method. But such a reasoning did not satisfy Borel because it gives no means "d'en définir une, c'est-à-dire d'en désigner une de telle manière qu'on puisse la distinguer des autres". In this note Borel proved that it is possible to define effectively such a function. However, he remarked that the definition "exigera en général des opérations transcendantes pratiquement inexécutables". One may wonder whether this satisfied Borel in view of his standpoint to use only "objets calculables".

Was such a classification possible before Cantor? This seems unlikely. The theory differs, for instance, essentially from the classical theorem of Weierstrass on the approximation of continuous functions by polynomials. The theory of Baire is not a subject of classical analysis as I tried to describe in chapter 1. Here construction and existence are placed in face of each other.

Remark. We treated historical aspects in the evolution of the concept of existence in analysis. We mentioned some analogous developments in algebra (Abel, Galois). But we did not consider problems on existence in geometry. Existence in geometry needs further study which we shall not undertake in this book.

NOTES

1. In his discussion of a book of Olevskii (Fourier series with respect to general orthogonal systems) Boas writes: "The proofs presented in the book are extremely "classical"; they depend, for the most part, on ingenious and difficult constructions". (Bull. A.M.S. 82 (1976), p. 854). Has "classical" to do with complicated calculations? The well known books of Polya and Szegö "Aufgaben und Lehrsätze aus der Analysis" are they "classical"?
2. Salmon also wrote an interesting book on algebra: "Lessons introductory to the modern higher algebra" (about 1845; see ed. 1866). In those years it was a curious book: linear transformations, theory of invariants, symbolic methods; many references to Cayley and Sylvester. For a review of Salmon's works see a memory of Fiedler in Salmon-Fiedler I (1907).
3. This is only an example of classical books on analytic geometry. There were more such books, for instance B. Niewenglowski, Cours de géométrie analytique I,-,IV; prim. éd. 1894.
   Volume III contains a Note de E. Borel "Note sur les transformations en géométrie". This is an exposition of transformation groups and the theory of S. Lie in geometry. It is the text of lectures given by Borel in 1894-1895.

   For differential geometry see the classical books of Darboux, Théorie des surfaces, 4 volumes 1887. Also L.P. Eisenhart A treatise on the differential geometry of curves and surfaces (1909).
4. See a note by Anil Nerode, The limits of effectivity in classical mathematics, Notices of the A.M.S. Vol. 25, no. 5 (1978) p. A 505
5. For these developments see [Novy, 1973], [Wussing, 1969]
6. See an interesting paper by E. Borel "La théorie des ensembles et les progrès récents de la théorie des fonctions" (1909). In: "Emile Borel philosophe et homme d'action, pages choisies et présentées par Maurice Fréchet", Paris, 1967, p. 155-175. At the end Borel writes: "...M. Georg Cantor doive être considéré comme l'un des mathématiciens dont l'influence a été la plus considérable, à la fin du XIX siècle et au commencement du XXe".
   He began this paper by writing "La théorie des ensembles, qui fut d'abord la théorie des ensembles de points, est née de la Théorie des fonctions". Remind the connection between the theory of functions and

Fourier series.

7. It is worthwile remarking that in Whittaker and Watson, Modern Analysis, there are no references to set theory. Cantor is only mentioned for his work on trigonometrical series.
   There is neither a reference to set theoretical methods in Picard, Traité d'analyse.
   However, in Jordan, Cours d'analyse, there is a section on sets.
8. To perceive the great difference between the classical and the modern standpoint see: J. Schmets, Espaces de fonctions continues (Lecture notes in Math., Berlin 1976).
9. The author knows only one book that contains this result; see [Hadwiger, 1957].
10. See a paper of Sierpinski "Les exemples effectifs et l'axiome du choix" in Fundamenta Mathematicae 2. There are several non-effective examples.
11. Towards the end of the 19th century functional analysis in a concrete sense was studied. One studied totalities or classes of functions; the term "space" was not yet introduced here. See the books of Volterra and Lévy in the Collection Borel.
12. Borel added: "Et aussi à une série convergente".
13. The notation A + B for A ∪ B and AB for A ∪ B was still customary in these years.
14. I don't know whether there is such an elegant proof for $\mathbb{R}^2$.
15. There is a problem: can the fact that the measure problem for $\mathbb{R}^3$ is unsolvable be proved without paradoxical decompositions?
16. Compare an interesting paper of Kahane "Brownian motion and classical analysis" (1976).
17. His proof has some analogy with demonstrations of theorems in the theory of real functions where Borel-sets are used (F_σ, G_δ etc.).
18. See [Bochner, 1979]. The author writes that this example was given "at a time when counter examples were still at premium".
19. This concerns technioal questions.
20. See the note by R.P. Boas: Fourier series with respect to general orthogonal systems by A.M. Olevskii, Bull. A.M.S. 82, no. 6 (1976) 853-857.
21. Compare: R.P. Boas, The heat equation, by D.V. Widder, Bull. Am. Math. Soc. 82, no. 5 (1976), 691-693. Boas remarks that Widder studies the heat equation from the non constructive point of view.

22. Later on Borel remarked that this difficulty does not present itself for the "fonctions usuelles".
23. One may wonder what was the point of view of du Bois-Reymond: did he consider his theory as a theory of classification of functions with respect to their asymptotic behaviour, or was it classical analysis in connection with Fourier series? He was a most remarkable scholar; it is generally admitted that he was the first to introduce a process of diagonalization in his theory of increasing. See [van Dalen-Monna, 1972].
24. It is worthwile to mention that Euler already considered degrees of infinitely small and infinitely large. See [Bos, 1974].
25. Borel added: "La première considérations des fonctions idéales est due à Paul du Bois-Reymond; j'ai cherché à compléter un peu ses trop brèves indications". I could not exactly verify this remark.
26. Compare these "fonctions idéales", with the Dedekind cuts.
27. The notes I and III in [Borel, 1898] are also worthwile reading.
28. Compare the note p. 124, 125 in [Borel, 1910] where Borel writes that all the French mathematicians who worked in this field (with the exception perhaps of Hadamard) agreed with some rules formulated by Poincaré:

    "1° Ne jamais envisager que des objets susceptibles d'être définis en un nombre fini de mots; 2° Ne jamais perdre de vue que toute proposition sur l'infini ne doit être que la traduction, l'énoncé abrégé de propositions sur le fini".

    See also "la Conclusion" p. 145 in [Borel, 1922].

PART III THE EVOLUTION OF MATHEMATICS

INTRODUCTION

In Part I we considered the growing influence of algebra and algebraic methods in mathematics and we designed these developments as the "algebraization" of mathematics.

In Part II we were concerned with aspects of the evolution of the concept of existence in mathematics and in particular we studied its relations to constructivity.
The considerations in these parts were for the greater part of a historical descriptive character, although general aspects are the main purpose.

It is the aim of Part III to place these studies in a more general framework. The considerations in this Part are to be seen as a contribution to a study of the headlines of the evolution of mathematics. In such a study questions of a philosophical kind can be posed. Thus, the character of Part III differs somewhat from that of the preceding Parts.
They also concern historical aspects of the evolution, but now we also ask for reasons and causes of developments. The significance of some results -for instance problems on existence- will be discussed. Furthermore we shall treat connections with other domains of science.

In this direction one can study, for instance, the evolution of mathematical concepts, such as the concept of existence, the introduction and evolution of axioms, forms of mathematical definitions etc. They will not all be treated here; the field is too broad to undertake here such a study.

First we are concerned with the concept of existence. In particular we consider the historical relations with developments in physics. This leads in a more general sense to considerations on the mutual influences of mathematics and physics and mechanics in the evolution of mathematics. Comparing the classical period with the modern period we introduce the idea of external respectively internal developments.

A study like this can scarcely expected to be objective. There are subjective points of view and there may be opposition. One finds many statements of the form "it seems that...", "I think that...". It is difficult to avoid this when it concerns not only a description of facts. It is not always possible to give definite answers. Even the choice of the subjects and the references is subjective. The references are not followed up to the most recent years. In some way this study should be considered as a program for further research. In such a study it would be interesting to consider the philosophical problem of the sense attributed to the mathematical entities in the various periods of history. Then the general problem of external influences should be taken into account. There are only some hints with respect to these problems.

CHAPTER 1 CONSTRUCTION AND EXISTENCE

1.1 Strong and weak existence

In Part II we studied the historical evolution of existence theorems in analysis comparing classical results with modern theorems. The aim was to find aspects which to some degree are characteristic for contemporary analysis in comparison to classical analysis. Whereas classical analysis was highly constructive, in modern analysis reasonings concerning properties of collections, classes of functions provided with an algebraic or topological structure are frequent, even dominating, and this leads to existence theorems that are non-constructive.

Here we shall consider the development of the concept of "existence" in mathematics in particular the aspects thereof in the 18th and 19th centuries in comparison with the modern period. By the modern period we understand the period beginning with Cantor.

First some general remarks. In the 18th and 19th centuries, anyhow before Cantor, analysis was to a high degree constructive. When it concerned theorems or theories, stating the existence of certain functions, for example functions satisfying a differential equation, the results were mainly obtained by means of methods of constructive type. The object was given in an explicit form or at least methods were used which allowed to obtain approximations, more or less useful for calculations. Existence theorems of this constructive type will be called strong existence theorems. They state the strong existence of certain objects.

The situation in modern analysis has other aspects. Strong existence has still its place, but a more weak notion of existence is important, perhaps even dominating. When we speak of existence we think of propositions such as "There exists $X \in \Omega$ such that....", where Ω signifies a certain well defined universe or space, "For any..., there is $X \in \Phi$ such that....", "Almost all $X \in \Psi$ have the property A", etc. In most cases these are non-constructive propositions in the sense that the proposition does not indicate any method to calculate or to give in an explicit form the object which is pretended to exist. Certain results can not even be obtained by means of constructive proofs for reasons of principle 1). But in general the mathematician does not occupy himself with a construction in such situations, unless he is a constructivist for reasons of principle or he has the domain of numerical mathematics or applied mathematics as his field of research. Construction is not an important aspect in his theory. It may be that some mathematicians prefer constructive proofs; it is then a question of psychology and there are some indications that in recent years the interest in constructive methods is growing. It concerns a concept of abstract existence. When we speak of existence we have a certain idea of it which is not associated with construction or methods of approximation. This notion of existence is independent of all constructions. It is a pure existence. Existence theorems of this type will be called weak existence theorems. They state the weak existence of certain objects.

In Part II we treated several examples of existence theorems of this weak type. We considered, for instance, the functional equation of Cauchy

$$f(x+y) = f(x) + f(y), \; x,y \in \mathbb{R}$$

and we observed that there exist infinitely many discontinuous solutions of this equation. This means: it concerns here weak existence. There are no means to give a solution in an explicit form, even not in a form allowing approximations. What is the significance of existence in these situation? Should we say that these discontinuous solutions exist in a certain "ideal world" whatever this might be, that is shall we find the answer in the standpoint of platonism? It is a problem of the philosophy of mathematics and the mathematician working near the frontier will scarcely be interested in such a question, unless his domain of research is the philosophy and the foundation of mathematics. His answer shall be that such a result, the "existence", is proved within a certain theory or axiomatic system (for instance Zermelo-Fraenkel) by means of the methods for proving theorems that are accepted in this theory. It is the way of proving that leads to the result and therefore the result is - or must be - accepted. Poincaré expressed this by saying that a proposition like "There exist X with property A" does not mean anything else than saying that it is impossible that there exists no object with property A.
The proposition: "it is impossible that there exists no object with property A" must be understood in the following sense: "if there were no object with property A there would occur somewhere in Mathematics a contradiction". (see als Note 15).
We shall not treat here this philosophical problem, which has been a subject of many discussions 2); [van Dalen, 1978], [Chandler Davis, 1974].

1.2 Existence and physics

We consider strong and weak existence from a historical point of view. The following questions may be posed:

(1). When does the concept of weak existence appear for the first time in the history of mathematics (evidently not under this name; the term is introduced here)?

In particular:

(2). Was something like a notion of weak existence already present in the minds of the mathematicians of the 19th century? What was a "solution" and when was a problem considered as being solved?

Our remarks on these questions shall mainly concern analysis, but a historical study of these questions in algebra and geometry would also be of interest.

First there is reason to ask what has been the role of the works of Cantor in the introduction of weak existence in analysis? Indeed, it must be stated that theories of non-constructive character were above all developed in the post-Cantorian period. As to Cantor himself, one perceives the weak aspect in his work on algebraic numbers and the existence of transcendental numbers; this is considered as the start of the works on the theory of sets. Our concept of existence is in some way tied up with the notion of a totality, a certain universe or space connected with the problem and in this universe the problems of existence arise. The formation of totalities is in some way connected with the capacity of human mind to consider several objects together in their relation to each other, making abstraction from the individuals; they are then considered and studied in their totality from certain points of view. A next step is to study abstract totalities (sets). This is perhaps a general scientific phenomenon (for instance problems of classification).

The question can be posed whether pure, weak, existence would have been possible in earlier stages. I think the conditions and means were not yet present then - although some exceptions will be treated later on.

What are these means? We already mentioned some in Part II: the axiom of choice, Zorn's lemma and all the theories that are based on them. For instance the theorem of Hahn-Banach and some other fundamental theorems in functional analysis (the theorem of Banach-Steinhaus). There are topological reasonings, for example on compacity (existence of convergent subsequences). Evidently all this in combination with traditional indirect proofs. This apparatus is post-Cantorian. By means of these tools classical constructive theories could be transformed into more general abstract theories. For example: the classical theory of integral equations and the modern theory of linear operators; the equivalence of the existence of solutions of a differential equation with the existence of fixed points of certain transformations (see the Preface in[Banach, 1932]). Besides these existence problems there are problems on unicity. Already Cantor studied them in connection with trigonometrical series at the start of the theory of sets. However, already Cauchy studied a problem on unicity; we shall return to it.

Finally there are the operators "sup" and "inf" in the field of real numbers. However, they are not post-Cantorian and they were used earlier to prove certain existence theorems.

But I think the lack of an adequate tool is not the only reason to explain why pure existence, weak existence, was mainly developed only after Cantor. It may be that there is a more profound reason. It is well known that classical analysis was developed under the strong influence and in connection with the needs of mechanics and physics. Large parts of analysis found their origin in physical problems; later on such theories were further developed in a more autonomous way. I think that for this reason the question of the existence of certain objects did not present itself as a real problem for the analysts: the existence was sure for physical reasons. The problem was to give a construction. This means, strong existence and weak existence were not separated in the minds of the mathematicians.

There is still an other argument. The conditions that were necessary for developing theories in a rigorous form as was required later on were not yet realized in the classical period. The rigour failed even in the definitions. In the 17th century there is no exact defininition of the notion of a curve, perhaps caused by the influence of mechanics. There are the well known difficulties, connected with the definition of the concept of a function. One did not feel serious difficulties with implicit functions. Even in books of the 20th century one reads that the solution of a partial differential equation depends on a "arbitrary function", without any more precision what this means (see for instance [Forsyth,1903], a well known book in the first decades of our century). Nowadays such existence theorems, weak theorems, are proved in a well defined universe, a space or an algebra of functions. The situation of analysis in the 18th and 19th centuries had not yet reached the level on which such theories could have been developed. Construction and algorithm prevailed.

1.3. Differential equations

The history of the theory of differential equations presents an interesting example of problems on strong and weak existence.

In the old classical period the purpose of the theory of differential equations was the very concrete problem to solve the equations which presented themselves to the mathematicians and the physicists. One applied substitutions, transformations and all kind of manipulations of algebraic character to be able to treat large classes of equations, either to obtain the solution in an explicit form or to reduce the solution to quadratures. The concern of the mathematicians was to "integrate the equation". We treated these aspects in some detail in Part I. We conclused there that it was properly speaking algebra, a formal theory. Even in the first decades of the 20th century this side of the theory was an important purpose in the university programs for analysis.

With regards to partial differential equations Lagrange wrote 3) : "Par cette méthode on peut donc intégrer, en général, toute équation aux différences partielles du premier ordre dans laquelle ces différences ne paraissent que sous la forme linéaire, quelque soit d'ailleurs le nombre des variables; du moins l'intégration de ces sortes d'équations est ramenée à celle de quelques équations aux dérivées ordinaires; mais on sait que l'art du Calcul intégral aux différences partielles ne consiste qu' à ramener ce Calcul à celui des différences ordinaires, et qu'on regarde une équation aux différences partielles comme intégrée lorsque son intégrale ne dépend plus que de celle d'une ou plusieurs équations différentielles ordinaires" 4).

In this classical period it concerned the strong existence of solutions. It was a theory of an algorithmic character. I believe there is no great risk in supposing that existence for itself, weak existence of solutions, did not yet present itself as a real problem. In this question the influence of mechanics and physics should be taken into account because many differential equations came from physical problems, and in such cases there was in a natural way a solution.

From about 1820 on Cauchy treated in his lectures a general existence theorem. In the literature it is often mentioned as "the first demonstration of the existence of solutions of a differential equation". It concerns the equation

$$\frac{dy}{dx} = f(x,y), \quad x,y \in \mathbb{R} .$$

Under some conditions on f - which later on were improved by Lipschitz - he proved that there exists a unique solution satisfying given initial conditions : $y(x_0) = y_0$. Later on Cauchy treated in the same way some equations of the second order. Cauchy had several methods for proving this result, for example:

(i) approximation of the solution by means of polygons and the proof that this process converges.

(ii) Supposing f to be an analytic function, a method of developing the solution in a Taylor series and the proof of the convergence in an adequate interval (this method is known as "Calcul des limites").

(iii) method of successive approximations.

Cauchy incorporated this theorem in his "Cours donnés à l'Ecole polytechnique à Paris", but in those years he did not publish this result. Only about 1840 the world of mathematicians became more acquainted with these methods through the initiative of Moigno. There is reason for some remarks.

In the form in which this theorem usually is formulated -at least in our time- it is of the kind of a weak existence theorem. We are not accustomed to associate this statement with something like a construction or method of approximation. From the point of view of philosophy we read it as pure existence.

However, the proofs given by Cauchy were of constructive type; they are connected with methods permitting approximation. That is to say: for any given function f there is a possibility of approximating the solution.

Now the question arises what was the attitude of Cauchy with regard to these aspects. Was there in Cauchy's mind already a notion of existence, pure, weak existence, separated from ideas about constructivity? This means, mathematical existence as a philosophical notion for itself, not attached to constructivity in the background. Did he have a concept of separation between what we have called weak and strong existence? Or should we suppose that in his thoughts the old algorithmic ideas prevailed, i.e. to give methods of approximation, but now applicable to general situations?

In view of this question it is remarkable that he did not publish his result. Why did he not? Taking into account that Cauchy used to be quick in publishing his results and the large number of his publications one may wonder whether Cauchy has been aware of the great value of this result. Did he see its real significance in the evolution of mathematics? It should be remarked that in 1814 Lacroix had already given a solution of this differential equation in the form of a series, however without proving its convergence; apparently this was not felt to be necessary because existence for itself was in those years not yet a problem. Cauchy accomplished this defect; he felt the necessity of rigour. Was, from a philosophical point of view, an abstract notion, such as pure, weak, existence conceptually possible in the stage of the evolution of mathematics of those years? 5).

Until our times the theorem of Cauchy takes a place in textbooks on analysis. Consulting older books it appears that often great value is attached to the aspects of unicity and the constructive character, perhaps more than to the fact of the existence itself. Should it be concluded that for these authors the idea of construction, approximations, has prevailed? 6).

Cauchy's theorem was important for the development of the theory of differential equations because it can be considered as the start of a new period: the theorem is no more a result of the ancient formal theory, it is really analysis 7). Later on it was observed that Cauchy's result was only a <u>local result</u>: for analytic equations -an important case- it concerns a solution in a neighbourhood of the initial conditions, whereas in the formal theory it concerns global solutions. Thus, gradually new methods were developed for treating the initial value problem with the purpose of eliminating this objection. Differential equations in the complex domain were studied (there were already some studies by Cauchy), opening the possibility of an application of the methods of the theory of analytic functions. In particular, the method of analytic continuation furnished a tool for obtaining general results extending the local point of view. The names of several mathematicians are connected with this development: Fuchs, Poincaré, Klein,[Painlevé, 1897], [Boutroux, 1908]. One studied properties of the solutions directly from the equations, without knowing the solution in an explicit form: singular points of the solution where one had to distinguish between singular points which depend on the initial conditions and those who do not, behaviour of a solution in the neighbourhood of a singular point, problems on classification, group-theoretical problems in the case of multiform solutions (monodromy). There were the connections with other domains, for instance elliptic functions and, more general double-periodic functions, domains that were studied earlier. Some special equations, already studied in the formal period, came to play a role: for example the equation of Riccati. One studied functions that were defined by a differential equation, for instance Riemann's P-function. In this way the local theory was transformed into a theory with quite different character. Construction, approximation were no more on the first place. Existence, pure, weak existence was important on this way.

However, it is not this way of the history that we want to follow. We will consider some works of Peano and Perron which are an excellent illustration of the transformations of the idea of strong existence into weak existence.

In 1886, that is about 40 years after Cauchy's result had got some familiarity in the mathematical world, Peano proved that the equation

$$\frac{dy}{dx} = f(x,y), \quad x,y \in \mathbb{R},$$

under the only condition that f is continuous, has solutions y_{min} and y_{max} such that any solution of the equation satisfies the inequalities

$$y_{min}(x) \leqslant y(x) \leqslant y_{max}(x) \quad \text{for all } x \in \mathbb{R}.$$

In a paper of 1890 he returned to this problem, studying also questions of unicity.

What is interesting here is the method by which Peano proved this result. Cauchy used as a tool the convergence of sequences of functions, a method that permits approximation. The proof of Peano rests on an other principle: it is the introduction of the supremum and infimum of families of functions. These operators are applied to two families $y = y(x)$, determined respectively by inequalities

$$\frac{dy}{dx} < f(x,y)$$

and

$$\frac{dy}{dx} > f(x,y).$$

It is the method of approximating the solution from below and from above. This method of approximation was earlier used in mathematics: the method of exhaustion, developed in Antiquity for calculating volumes and areas (Archimedes); approximation of real numbers with the aid of continued fractions; Dedekind cuts. In these cases it is a constructive method, appropriate for calculating approximations. But in the case of Peano it concerns families of functions and in this case the method is in general not well suited to calculate approximations. In the case of Cauchy there were reasons to wonder whether for him the question of existence for itself was the main problem or if approximation was the essential point. For Peano however I think it concerns pure existence without side reflections on constructivity 8).

With regard to the problem of the evolution of the concept of existence it is of interest to mention a paper of [Mie,1893] in which Mie is concerned with Peano's theorem on differential equations. It was Mie's aim to present Peano's results in a more customary form because in the original form these papers were difficult to read as a consequence of a rather unusual terminology. There are some passages in the introduction of Mie's paper which are valuable for our considerations.

Speaking of the existence of integrals of a system of differential equations and the methods to determine these integrals Mie observes that in the research, in so far analytic equations are considered, it concerns always of a "rein formales Rechnen mit Potenzreihen". To be able to treat more general cases, one should return, in Mie's opinion, to the fundamental definitions:

"Und doch kann nur durch eine Begründung, die auf die Fundamente zurückgeht, unser Causalitätsbedürfniss befriedigt werden, was man von jenem rein rechnerischen Verfahren, welches zuerst Cauchy für den Fall von Funktionen complexer Variablen anwandte, gewiss nicht sagen kann".

Here several questions arise.

May we conclude that Mie attributed not much value to calculations and that it was his opinion that a formal calculus is not a contribution to a real understanding? What does he mean with "Causalitätsbedürfniss"? It seems that Mie attached more value to the non-constructive method of Peano than to the algorithmic method of Cauchy. May we suppose that methods, like that of Peano, which in some way seem to be directly connected with the principles of the problem and are attached to the character of the problem, are more valuable than procedures of calculation, algorithmic methods, which are perhaps used without bringing out the real significance of the problem? Was Mie the only one who had such opinions? It is not very likely.

This are questions attached to the problem of the mutual relations between existence and construction in the evolution of mathematics. They can not only be posed with respect to Mie's remarks, but these are questions which ask for an answer in a broader framework. They ask for a general historical research on the thoughts of the mathematicians in the 18th and 19th century (and earlier) on the character of mathematics. Problems on existence and construction are only part of the questions that then should be posed and such a study should not be limited to differential equations.

In the history of the evolution of mathematics there have been more criticisms of this kind. In Part I we treated some objections of Leibniz, Monge, Poncelet, Poincaré, which concerned too automatical, algorithmic, methods. There is some philosophy behind these questions: should mathematics be seen as a tool to derive properties or as the science of the properties of mathematical systems?

In the course of the years Peano's work has been continued several times. In particular a paper of Perron (1915) must be mentioned in which he used the principles of Peano. The proof of the existence is given by means of two families of functions, majoring resp. minoring functions. In an introduction Perron writes that his proof "durchsichtiger scheint als die seither für diesen Fall [only continuity of f] gegebenen Beweise". And he remarks that this demonstration "in der Praxis gestatten wird die Integralkurven in ihrem gesammten Verlauf zu verfolgen". It is a rather confusing situation. Did Perron consider his proof as a constructive method or is this remark only a reference to the theory of Cauchy, being a local result in the analytic case?

Some years later (1923) Perron used the same method for treating a fundamental existence theorem in analysis, Dirichlet's problem. At that time weak existence had already found its place in mathematics since several years. It concerns a classical problem in the theory of partial differential equation, potential theory. For a given open set $\Omega \subset \mathbb{R}^n$, it concerns the existence of a function u, defined in Ω, called a harmonic function, which in Ω satisfies the equation of Laplace

$$\Delta u = \sum_{i=1}^{n} \frac{\partial^2 u}{\partial x_i^2} = 0,$$

which moreover takes given continuous values f on the boundary of Ω. This problem has a long history. Because there is not always a solution, methods were developed by which in a suitable way to any (Ω, f) a harmonic function is associated which is identical with the solution if it exists. Some of these methods are of an approximative character in the sense that they consist of an approximation of Ω by suitable open sets and convergent sequences of harmonic functions (Schwarz, Kellog, Wiener). Perron treated the problem in a direct way, introducing in a suitable way two families of functions and using the operators sup and inf. His method leads to a weak existence theorem. There is no constructivity.

Now, reminding of the remarks and objections of Mie, there is a question. Is Perron's method superior because it leads to a better understanding of the problem? First, if the answer should be given in the affirmative sense, this is not because the procedure is non-constructive. There is no reason to reject constructivity; non-constructivity should not be considered as a purpose in itself. But there are deeper reasons. The method of Perron furnishes a better comprehension of what the problem is about because the problem can be placed in a broader framework: it can be considered as the n-dimensional generalization of the approximation of a line segment by convex and concave functions (curves). Furthermore, this method is important because there appeared to be an opportunity for application to the axiomatic theory of harmonic functions. It is a theory with still broader scope than the classical theory in $\mathbb{R}^n$; it is based on locally compact topological spaces 9).

In this context there is a reason to mention an other approach of the Dirichlet problem. It is a non-classical method: the problem is studied from the point of view of functional analysis taking into account the linear character of the problem. The problem is considered as the problem of studying a certain linear operator $f \to H_f = u$. It is a non-constructive method. This way could be used in axiomatic potential theory and in some generalizations;[Monna, 1975], [Bertin, 1978].

In the preceding theory we have an example of methods of proving results which show their special usefulness because they can be used for building more general theories than the theory for which they were originally invented. This may be a reason to prefer the direct, non-constructive, method of Perron -and the functional-analytic approach- above methods of approximation.

Is this a general aspect of the evolution of analysis? In Part II we treated several examples of the shift in the evolution from problems of finding a solution towards questions of solvability. As a consequence of this shift weak existence theorems came more to the front. To some extent the developments of the Dirichlet problem is another example of this evolution. Problems of solvability are often studied in the framework of abstract theories, axiomatic systems, and in such theories the notion of weak existence in axiomatic form is of great importance. There are reasons to suppose that the weak aspect is especially valuable in the creation of new theories as a consequence of its flexibility, which opens the possibility of application to new situations. It is perhaps a more valuable way in such situations than methods of calculation which are often adapted to special cases. Let us give another example.

The abstract theory of linear operators, formulated in normed spaces, furnishes the framework to treat integral equations, parts of the theory of linear differential operators, that is the theory of differential equations. The abstract theory, with its weak aspects, is a means suitable to unification of theories which at first sight are very different. We already mentioned the theory of fixed points of certain transformations (Brouwer, Schauder) and applications to differential equations.

We mentioned before that these weak theorems, results of an abstract character, are obtained by profound means, tools less simple than the operations of classical analysis: the axiom of choice, Zorn's lemma etc. These tools lead to abstract theories of great flexibility, and great generality. But mathematicians must pay a price, for some mathematicians perhaps too high a price, namely the loss of effectivity. There is a good reason that we mentioned above the operations sup and inf as a tool in these developments. In some cases these operators are of a constructive type (real numbers), but in other situations they are non-constructive, or at least less constructive. Earlier we mentioned, for instance, the application to families of functions. To some extent sup and inf are more simple than the axiom of choice and Zorn's lemma. And when a comparison is made between the discontinuous solutions of the functional equation of Cauchy and the method of Perron, it must be observed that in the latter method there is still something of an idea of constructivity. Anyhow, the method of Perron can be replaced by a constructive method yielding the same result and this is impossible for the equation of Cauchy. This suggests something like a measure of simplicity 10).

1.4 Weak existence in the pre-Cantorian period

The remarks on Perron's work and the evolution of the theory of harmonic functions concerned developments in the 20th century. We will now consider some aspects of the concept of existence in the 19th century.

In a fundamental paper of Gauss on potential theory there is a passage which makes plausible that Gauss had some idea of weak existence, well to be distinguished from strong existence (without using these terms). It is a paper from 1839 and it has been of a high value for the development of potential theory [Gauss, 1839]. Gauss considers a surface S in $\mathbb{R}^3$ and continuous distributions of mass ρ on S. His aim is to prove the existence of a distribution ρ_0 on S with a certain special property. To this end Gauss considered the family of all the integrals

$$\Omega = \int_S (V-2U)\rho ds,$$

where V is the potential of the distribution ρ and U a given continuous function on S. The problem is to minimize Ω for all possible distributions ρ of the same total mass. It is a well known fact from history that Gauss' reasoning to prove that Ω takes a minimum value for a certain distribution ρ_0 was not correct. It was the start of a long series of investigations up to our century [Monna,1975]. Gauss' procedure was non-constructive. One may suppose that physical considerations have played some role in his thoughts about the existence of ρ_0 because Gauss' work on potential theory was in close connection with his investigations on magnetism and electricity. However, Gauss pretended to give an exact proof. With respect to the question of strong or weak existence the following passage is of

interest.
"Die wirkliche Bestimmung der Vertheilung der Masse auf einer gegebenen Fläche für jede vorgeschriebene Form von U übersteigt in den meisten Fällen die Kräfte der Analysis in ihrem gegenwärtigen Zustände. Der einfachste Fall, wo sie in unser Gewalt ist, ist der einer ganzen Kugelfläche;...". Gauss gave the calculation of the minimizing distribution for a somewhat more general case than that of the sphere. We may conclude from this passage that for Gauss pure existence was separated from a calculation (strong existence). However, there is no great risk in supposing that Gauss would not have understood the case of discontinuous solutions of the functional equation of Cauchy, even if we suppose that the difficulties about continuity and discontinuity would have been sufficiently clear at that time. May we suppose that Gauss would have rejected such solutions? Gauss' work was strongly based on physical intuitions. Was our abstract idea of existence present in the thoughts of Gauss?

In the same area there is a fundamental paper of Dirichlet [Lejeune-Dirichlet, 1876]. Dirichlet also studied problems in potential theory. Among them is the so called "Dirichlet's problem". It is the problem of the existence of a certain harmonic function we considered before. Dirichlet writes : "Die Aufgabe, jene Function zu finden, lässt sich nicht lösen: es kann nur von einem Existenznachweis derselben die Rede sein. Letztere hat keine Schwierigkeit".

Dirichlet reduced this to the problem of minimizing a certain integral, which depends on a family of suitably defined functions. It is an integral of the same type as considered by Gauss:

$$\int[(\frac{\partial u}{\partial x})^2 + (\frac{\partial u}{\partial y})^2 + (\frac{\partial u}{\partial z})^2]dT.$$

Dirichlet's proof of the existence of a minimum value contains the same error. It is known under the name "Dirichlet's principle". But we may conclude that Dirichlet apparently had some knowledge of a notion of weak existence, independent of calculations.

In later years Riemann used an analogous method in connection with Dirichlet's problem as a point of departure for his investigations on the theory of analytic functions of a complex variable (conformal representation). Thus, the foundation of the theory was not correct. But it should be observed that, once the solution of Dirichlets's problem was supposed to be known, the "minimum principle" does not play any role in the further development. The theory developed in an autonomous way and in the theory which resulted the constructive aspect dominated.

One may wonder to what extent physical considerations were a guide in the thoughts of Dirichlet and Riemann. Anyhow, physical intuitions played a role. Even F. Klein, continuing the works of Riemann, used physical images [Monna, 1975]. Was pure existence a concept that was in some way familiar to the contemporaries of Gauss, Dirichlet, Riemann, or should we suppose that these last were in advance?

Because in these examples it concerns the existence of the minimum of an integral, one is inclined to think on the calculus of variations where the problem is to determine the minimum or maximum of an integral depending on a curve. At least this was the classical form of the problem, already studied by Euler and Lagrange; we have not in view modern developments. In the classical period the main problem was to give in an explicit form the solution of extremal problems: the brachystochrone problem, the isoperimetric problem etc. For a systematic theory of these problems the solution of such problems was reduced to the problem of determining maximum and minimum of functions of one variable by the introduction of a parameter, replacing the system of variable curves by a family of curves depending on this parameter. In this way the integral is reduced to an ordinary function. This procedure led to a differential equation (equation of Euler-Lagrange) and the problem was to solve this equation. In our terminology it was a theory of strong character.

The way in which Gauss, Dirichlet and Riemann studied their extremal problems was quite different: they considered in a direct way a family of functions without introducing a parameter. In his work "Grundlagen für eine allgemeine Theorie der Funktionen einer veränderlichen complexen Grösse" Riemann was also engaged in the minimum value of a certain integral Ω, defined for a family of functions λ. He wrote:
"Die Gesammtheit der Funktionen λ bildet ein zusammenhängendes in sich abgeschlossenes Gebiet,.... Für jedes λ enthält nun [Ω], einen endlichen Werth,...".

The integral Ω is thus considered as a function defined on the collection of the functions λ, without intervention of a parameter. In this reasoning of Gauss, Dirichlet, Riemann one recognizes the concept of a functional, a notion that was introduced in early functional analysis towards the end of the 19th century, a development that was post-Cantorian. In this new discipline weak concepts take an important place. From the point of view of constructivity there is a difference between extremal problems for functionals and the problem for functions of one real variable The first indications of this new area are thus in the works of these mathematicians. But they were not sufficient to found or even to start the development of functional analysis. This was reserved for the period that begins with the works of Cantor.

1.5 Mathematicians and the problem of existence

We considered in the foregoing some aspects of the development of the concept of existence in mathematics, in particular with respect to developments in the area of differential equations. Existence is one of the fundamental concepts in mathematics. A more profound study of the significance of this notion for the evolution of mathematics would be desirable and then other disciplines as analysis should be taken into account. In this section we shall make remarks on the attitude of mathematicians with respect to the problem of existence. First about Gauss. One knows that Gauss has given four proofs of the existence of roots of algebraic equations in the field of complex numbers (in modern terms : $\mathbb{C}$ is algebraically closed). Earlier D'Alembert had given a proof. Taking into account that, as we mentioned before, Gauss had perhaps some insight in questions on weak or strong existence, it would be interesting to analyse these four proofs from this point of view. A second remark concerns the historical problems of infinitesimals. It is a subject much studied by historians and my remark is no more than a suggestion. Is it conceivable that the classical difficulties around infinitesimals found their origin in problems on existence and construction as a profound reason? On the one hand strong existence could not be explained in a satisfactory way in some connection with physical concepts. On the other hand there is a non-constructive aspect in the infinitesimals as will be clear when one considers the modern approach with the methods of non-standard analysis. Therefore any effort in the classical constructive period to incorporate infinitesimals in an exact way into a theory must necessarily has led to a failure, thus being a source of confusion. Are there any indications on constructive attempts in the ancient works?.

A third remark is of a somewhat psychological character. It concerns the introduction of the "Riemann integral", introduced by Riemann in his paper "Uber die Darstellbarkeit einer Funktion durch eine trigonometrische

Reihe". He posed the following question: "Die Unbestimmtheit, welche noch in einigen Fundamentalpunkten der Lehre von den bestimmten Integralen herscht, nötigt uns, Einiges voraufzuschicken über den Begriff eines bestimmtes Integrals und der Umfang seiner Gültigkeit. Also zuerst: was hat man unter $\int_a^b f(x)dx$ zu verstehen?"

What is the reason that Riemann posed his question in this formulation? Was he concerned with a problem on existence? Was for Riemann the concept of an integral something that existed a priori in some sense and which should be explained? Or was his main concern the extension of the integral in a more or less constructive way to bigger classes of functions as was usual at that time (the Riemann-sums)?

I think it can scarcely be expected that we find the concept of abstract, pure, existence, i.e. weak existence, in the ancient classical period, with the exception of some indications in the works of Gauss, Dirichlet, Riemann as we mentioned before. I think most of the results of classical analysis -anyhow before Cantor- are not well adapted to the weak concept (infinitesimal calculus, series, special functions, explicit solutions etc.).

What can be said about developments in the more recent period, let us say the period after Cantor? After Cantor the notion of weak existence gradually penetrated. But the "weak" point of view was not accepted by all mathematicians, although the group of those who did not accept it formed a minority. For those it was a question of principle to accept in mathematics only constructive methods. Here should be mentioned in the first place L.E.J. Brouwer who from the first decade of our century on developed systematically and in all its consequences intuitionistic mathematics (a discipline which is more profound than only constructivity), a domain studied up to our time. However, Brouwer did also fundamental work on topology, an area that is not always constructive. Earlier we mentioned Bishop and constructive mathematics. But before Brouwer there were already constructivists.

We mention Kronecker. It was his philosophy that in mathematics all must be based on and reduced to numbers and in particular natural numbers. In this context we refer to his famous words: "God made integers, all else is work of man". See [Kronecker,1892].
For reasons of principle he only accepted the finite and demonstrations in a finite number of steps. From the last decade of the 19th century on there

were R. Baire and E. Borel, in principle also constructivists. These are well known episodes in history. But what is of interest here is to know whether Kronecker and Borel accepted in their work all the consequences of their philosophy. This seems by no means to be the case.

With respect to Kronecker we refer to his "Vorlesungen über Zahlentheorie". One may wonder whether his use of the results of the theory of infinite series (among them Dirichlet series) is in concordance with his philosophy. In his "Vorlesungen über die Entwicklung der Mathematik im 19. Jahrhundert" F. Klein writes, when discussing the relations between Weierstrass and Kronecker,"Poincaré hat von Kronecker gelegentlich gesagt (Acta Mathematica, Bd.22), er habe nur deshalb so grosse Erfolge in der Mathematik (in der Zahlentheorie and Algebra) gehabt, weil er seine eigenen philosophischen Lehren zeitlich vergessen habe".

As to Borel, for him is the difference between his philosophical point of view and the mathematical life in practice perhaps even more pronounced, although he warned several times in notes and passages that anormal objects -objects that are not in concordance with his philosophy- should nevertheless be studied for theoretical reasons. To convince oneself one should only read passages that Borel wrote in his books in the "Collection Borel". We mention some examples to illustrate Borel's ambivalence in the practical life of doing mathematics.

First we refer to his introduction of what is nowadays called local compacteness of $\mathbb{R}$ in his book from 1898 "Leçons sur la théorie des fonctions". We already mentioned this example in Part II 1.3. He introduced this concept in terms of coverings by intervals. The proof is non-constructive and not at all in accordance with his philosophy. Anyhow the fact that Borel added a note in which he says that in his thesis there is a "constructive" proof throws special light on this situation. There is scarcely need to observe how important Borel's result was for further developments.

Two other interesting examples can be found in his book "Traité du calcul des Probabilités et ses Applications", Tome II, fasc. 1, "Applications à l'arithmétique et à la théorie des fonctions" (Paris 1926). In chapter I, "Application de la loi des écarts à l'étude des nombres décimaux", Borel applied the theory of probability to study the decimal development of an arbitrary real number. The problem is to find asymptotic properties of the frequency of a given digit in such a development. With regard to this frequency he defines what he called exceptional real numbers and numbers that are non-exceptional, the latter called normal numbers. The result is that the set of exceptional numbers has measure zero. It is a non-constructive theory and Borel was afraid that this theory had no use. He remarks that there are no means to decide, for instance, whether π,e or $\sqrt{2}$ are normal or not.

For our purpose the section in this book "Le principe de Zermelo et un paradoxe de la théorie des ensembles" is most curious.
He is concerned there with the paradoxical decompositions of a sphere which we treated in Part II (paradox of Hausdorff and further developments). Borel did not mention Hausdorff. Here the problem of existence is apparent; decompositions of this type are far away from constructivity, it is a property on weak, pure, existence. Borel made some restriction: "... nous avons raisonné comme si les ensembles A,B,C étaient définis, au sens précis du mot, alors qu'il n'en est rien". Apparently here is the question of existence and definition.

It is striking that this result, later on often quoted as a consequence of the axiom of choice without any bindings with an impression of reality, can be found in a book that had been written by a scholar who, from philosophical point of view, was strict in refusing non-constructive methods and results .

It seems that as far as Baire is concerned, the situation was not very different. And, more recently, what to say about Hermann Weyl, who was also a constructivist?

Is there a great risk in supposing that most mathematicians are ambivalent in their conviction?

1.6 Fundamental concepts in mathematics

The introduction of the concept of weak existence, existence in an abstract form, introduced after a long constructive period must be considered as a capital development in the thought of mathematicians which profoundly changed the face of mathematics. How did mathematicians of the period in which this development started, experience themselves the introduction of such a concept? It is a historical problem with some psychological backgrounds. Such a question should be placed in the broader framework of the history of the creation and evolution of fundamental concepts in mathematics. Existence is only one of these concepts. There are several more, for instance:

(i) The concept of "construction" and its development from its concrete geometrical form in Antiquity to constructions and approximations in algebra and analysis in our times.

(ii) The introduction and evolution of the concept of an "axiom" from its geometrical origins to axiomatic theories in modern time: the introduction of vectorspaces, abstract algebras, normed spaces etc. The concept of "existence" finds a place in this framework.

(iii) Introduction and evolution of the concept of a function.

(iv) Introduction and evolution of the notion of a group from concrete groups to abstract groups.

Some of these subjects were studied. For the concept of a group see [Wussing, 1969]. For the evolution of the concept of a function see [Monna, 1972], [Youschkevitch, 1976].

For normed spaces and functional analysis see [Monna, 1973 b]. Some of these subjects are of a philosophical type, others are more concrete.

Historical studies are often concerned with a more or less chronological description of certain evolutions. But we have in view studies of a more intrinsic character; they should be more than descriptions. One asks for an explanation of the facts. These are questions belonging to the domain of the psychology of mathematical research [Hadamard, 1949]. What are the causes of the evolution? How did essentially new concepts arose in the thoughts of mathematicians? What has been the influence of other disciplines? Were new concepts introduced gradually or should they be considered "discontinuities" in the line of the evolution?

In general it is difficult to get information with respect to such questions from publications because mathematicians are not accustomed to give information on the way in which they came to their results. It is e.g. wellknown that Gauss only published polished work: "Pauca sed matura". He once referred to the fact that, once a cathedral has been finished, one removes the scaffolding. Usually they publish their results in a more or less definite form. If still possible, personal information would be valuable. For some recent studies see [Monna, 1983], [Monna, 1984].

CHAPTER 2 CAUSES OF EVOLUTION: EXTERNAL AND INTERNAL RELATIONS

2.1 Mathematics and physics

Referring to the remarks in section 6.1 of the preceding chapter we will treat in chapter 2 some aspects of the creation and evolution of fundamental developments in mathematics. Problems on "existence" are a subject in this area but our consideration will be more general. Especially we propose to discuss aspects of external respectively internal causes.

The creation of infinitesimal calculus in the 17th and 18th centuries by Fermat, Leibniz, Newton, the Bernoulli's, Euler,... took place under the strong influence of the developments in physics, astronomy and mechanics. We shall express this by saying that it concerned a development that was guided, even caused, by external influences. It is a terminology that is perhaps not entirely satisfying -anyhow for the classical period- because in that phase of the evolution there is some difficulty in separating mathematics and physical sciences. Nevertheless, we introduce it here, because this qualification is useful to be able to distinguish aspects in the evolution in stages where such a separation is much more apparent.

One finds external influences in the 19th century, and also in the 20th century there still are impulses coming from developments outside mathematics. They come now not only from physics, but also from other disciplines such as biology, economy, computer science etc. But the character of the external influences gradually changed and I think the situation now is in some way essentially different from the situation in the classical period: we observe a process of emancipation that has led to mathematics as an autonomous science. In the 17th century and in a large part of the 18th century scholars were concerned with the creation of a new discipline; in those years mathematics and the natural sciences still formed a certain unicity. A norm, a certain guide for this creative process was found in mechanics, astronomy and physics, which supplied mathematical problems during the course of this process. But in the course of the 18th century the elements and techniques of infinitesimal calculus had been established in great lines and mathematics developed in a more autonomous way. The natural sciences still supplied problems, sometimes of a more incidental character and mathematicians found themselves obliged to develop theories for handling them. But the origins of such theories came soon in the background. This process has led to what is called pure

mathematics 11).
There are important classical works on the border of mathematics and natural sciences. Lagrange's "Mécanique analytique" (1788; third ed. 1853) is concerned with theoretical mechanics but, although impulses from mechanics are apparent everywhere, it is in fact a treatise on mathematics which finds its culminating point in Lagrange's general equations of dynamics.

The same remarks can be made with respect to the works of Laplace on celestial mechanics where certain transformations were introduced, later called the Laplace-transformation. See also his work on the theory of probability.

Much later, already near modern time, there is Poincaré's "Les nouvelles méthodes de la mécanique céleste" (1893) in which he introduced asymptotical series 12).

Gauss' works form an important source for works of this kind. The mathematical aspect of potential theory finds his origins in the study of terrestrial magnetism (1839) ; in fact it is mathematics. Furthermore there are his works on differential geometry, inspired by studies in geodesics. Apart from the origins it concerns pure mathematical developments 13). External influences can be found in the works of Dirichlet and Riemann. F. Klein used physical analogies to illustrate certain aspects of the theory of analytic functions of a complex variable.

These are examples from the classical and the late classical period. We treat in more detail some examples of developments and the introduction of new concepts in more recent times; they are also influenced by external impulses.

(i) In functional analysis, the notion of a functional is one of the fundamental concepts. It was introduced at the end of the 19th century by Volterra, who designed them by "fonction de lignes". This was the start of functional analysis. His studies in the areas of physics and mechanics led Volterra to introduce and study the "fonctions de lignes". His investigations covered several domains: mechanics, theory of elasticity, magnetism, electromagnetic theory. He posed the following question : "Est-il possible de se borner dans la Philosophie naturelle aux fonctions d'un nombre fini de variables?" [Volterra, 1913]. Volterra remarked that this is not sufficient: "Il est évident que si l'on regarde un phénomène comme l'effet d'un nombre fini de causes, on fait une abstraction, car on néglige des éléments qu'on considère comme très petits par rapport à d'autres éléments qui sont prépondérants. On ne fait ainsi qu'un examen approximatif du phénomène, mais on entrevoit facilement qu'il y aura des cas, où, pour approfondir d'une manière convenable la question, il sera nécessaire de passer du nombre fini au nombre infini d'éléments variables".

Volterra explained this by considering mechanical and physical systems. He referred to Picard who considered two kinds of systems. There are systems in which the future states of the system only depend on the actual state (or on those in an infinitesimal interval Δt preceding the actual state). According to Picard they belong to the domain of "non-hereditary mechanics". Such problems lead to ordinary or partial differential equations. On the other hand there are systems which belong to the domain of "hereditary mechanics". For such systems the future of the systems depends not only on the parameters characterizing the present state but also on the parameters characterizing all past states: the memory of the past is conserved. In this case one has to take into account functions which depend on infinitely many variables. They lead to integral equations or integro-differential equations. Volterra wrote: "On envisage des quantités qui dépendent de toutes les valeurs qu'une ou plusieurs fonctions prennent dans un champ donné". Volterra gave several examples: the Newtonian potential in a point, the magnetic force, examples concerning the temperature inside a solid in relation to the temperature on the border, etc.

Making abstraction from such examples Volterra studied functions of infinitely many variables, or in geometric form functions which depend on a curve. In the first decades of the 20th century such studies led to the notion of a "functional".

Here we have a concrete example of the introduction of a concept under external influences. It concerns a physical realization of our abstract notion of a functional, a strong existence, a model, of our abstract concept of a function of infinitely many variables.

Were mathematicians in those years still in need, a feeling of necessity perhaps, of a physical realization, strong existence? Did an abstract introduction, as we are accustomed to (although it can scarcely be called abstract according to our standards), not satisfy them? An indication of some answer can be found in Volterra's "Leçons sur les équations intégrales..." 1913). In Chapter I he considers by way of historical introduction the "Idée générale de fonction". With regard to

the evolution of the concept of function he refers to the works of Descartes, Leibniz, the Bernoulli's. He remarks that "la Physique mathématique a contribué à l'extension de l'idée de fonction". To illustrate this he refers to the problem of the vibrating string. He observed that this problem, as is wellknown, is reduced to the problem of finding the solutions of a second order partial differential equation . Now, the "general integral" of this equation depends on two "arbitrary functions" (Volterra referred for this to D'Alembert). Writing on Dirichlet's general definition of a function as a map Volterra remarks:
"L'idée de Dirichlet, qui ne définit aucune relation analytique entre les deux variables, descend évidemment d'une façon naturelle de la loi physique".

It is not easy to know the thoughts of Dirichlet on this point but Volterra's conclusion as to the relation with physics is at least not evident. Dirichlet gave the definition in a publication "Ueber die Darstellung ganz willkürlicher Funktionen durch Sinus- und Cosinusreihen", which is far away from physics. Was there a need for physical realization even for ordinary functions?

Volterra gives many interesting informations on relations between mathematics, mechanics and physics: classical mechanics, variation principles, Hamiltonian mechanics, hereditary mechanics, electricity, phenomena of hysteresis, elasticity, meteorology (Bjerknes), all this in connection with functions of infinitely many variables.

On the one hand these books contain applications of the theory of the "fonctions de lignes", but on the other hand they should mainly be seen as books in which a pure mathematical theory is developed in an autonomous way, without further external influences, soon far away from primary motivations. However, it is not always easy to trace a sharp border between these two aspects.

In later years, especially in the first decades of the 20th century, the theory of the "fonctions de lignes" was incorporated in functional analysis where physical origins can no longer be recognized. In modern books on functional analysis there are scarcely references to physical origins; the name "fonction de lignes" got lost and was replaced by functional. Other influences, algebra (groups, vector spaces) and topology came to play the main role: these are internal causes and influences. External influences were soon forgotten.

(ii). The theory of distributions is an other example. This more recent theory has some origins in physics; perhaps they are more wellknown. The theory begins with a paper of L. Schwartz (1945) who writes as follows: "Depuis l'introduction du calcul symbolique, les physiciens se sont couramment servis de certaines notions ou de certaines formules dont le succès était incontestable, alors qu'elles n'était pas justifiées mathématiquement. C'est ainsi que la fonction y(x) de la variable réelle x, égale à 0 pour $x \leqslant 0$, à 1 pour $x > 0$, est couramment considérée comme ayant pour dérivée la "fonction de Dirac" $y'(x) = \delta(x)$, nulle pour $x \neq 0$, égale à $+\infty$ pour $x = 0$, et telle que, de plus $\int_{-\infty}^{+\infty} \delta(x)dx = +1$".

It was the aim of Schwartz to eliminate this lack of exactness and therefore he introduced a generalization of the notion of a function. This generalization consists in defining in a suitable way a functional, called a distribution, which is a generalization of the ordinary function. The operations of differentiation and integration can be applied on distributions in a generalized sense without any limitation. Thus, this is a solution for the difficulties we mentioned before. The theory of distributions is now a theory of large extent where the methods and results of functional analysis are used, useful in various domains of analysis, for instance the transformations of Fourier and Laplace, differential equations. In a short time it developed to an abstract theory, without further references to physical origins.

Was there a need from the side of physisists or was there the need of rigour from the side of mathematicians? I do not know the answer. We have here an example of a situation in which the physical image, the strong existence, is incorrect and for which the methods of pure mathematics, abstract definitions, can give a remedy.

(iii). The kinetic theory of gases is at the origin of a pure mathematical theory, known as ergodic theory. One finds the elements of the kinetic theory of gases in works of D. Bernoulli. Especially the work of Boltzmann contributed to the development of a mathematical theory of this physical area which ultimately led to statistical mechanics. Gibbs (1901) gave important contributions. It is not the place here to give detailed information on this theory. We only give some indications.

Studying the behaviour of a gas, one considers a gas as a dynamical system, composed of molecules, satisfying the equations of dynamics of Hamilton. By means of a system of coordinates and moments of the molecules this system is represented as a point in a euclidean space of a sufficiently large number of dimensions $\mathbb{R}^N$ (the phase space). This is a method to study the evolution of the system in the course of time under the working of the equations of Hamilton. The problem is, for instance, to determine the mean values of certain quantities which characterize the state of the system etc., using measure theory. Therefore Boltzmann introduced the method of considering infinetely many replica of this system, all with the same energy but with different initial conditions, representing them all in $\mathbb{R}^N$ 14). The Hamiltonian equations generate transformations in $\mathbb{R}^N$ and the problem is to study the orbits of the systems in $\mathbb{R}^N$. In the framework of his investigations Boltzmann formulated the so called "ergodic hypothesis". This hypothesis concerns a certain global geometric property of the orbits in the course of time; in later years this hypothesis was a point of many discussions. In this way results of a probabilistic character are obtained, formulated in terms of measure in $\mathbb{R}^N$: distribution of the molecules in the course of time (distribution of Maxwell-Boltzmann), results concerning the tendency towards uniform distribution, problems about diffusion, definition and properties of entropy etc. Boltzmann used an old theorem of Liouville (1838) which expresses the invariance of measure under the working of certain transformations. In later years Ehrenfest continued Boltzmann's work. His contributions to the theory were of a fundamental character 15).

This theory was the origin of an abstract theory of mechanical systems. The terminology is still in some way connected with physics, but it is a pure mathematical theory and in the results the origin is not easy to recognize. One considers a group (or a semi group) of transformations on certain differentiable varieties, provided with a measure, and one studies the orbits under these transformations. This leads to results on convergence, existence of certain limits, problems on the existence of certain mean values etc. The theory is formulated in terms of functional analysis. We refer to books of Hopf (1937) and Jacobs(1960). The fundamental contributions of Ehrenfest are not mentioned there.

It is interesting to compare the concrete theory of Boltzmann-Ehrenfest with the modern abstract theory which found its origin in this physical theory. There are some interesting analogies with weak and strong theories as mentioned in Chapter 1. The former theory can then be characterized as a "strong theory", a constructive theory in the sense of being concrete. The latter theory is then a "weak theory". To some extent the strong theory is a realization, a model of the weak theory. But this weak theory has several more aspects. This "weak theory" is connected with problems on geodesics, with the theory of probability. The concept of entropy plays a role in some domains of statistics, information theory and some more domains distinct from physics. It is the force of "weak theories" to furnish methods in various areas. Hopf (l.c.) already made a remark, mentioning a certain analogy: "Statistik is Masstheorie. Es ist deshalb wohl verständlich, dass in den folgenden Teilen die masstheoretischen Gesichtspunkte vor den topologischen den Vorrang einnehmen. Den Mathematikern, die dem "fast alle" oder "bis auf eine Nullmenge" keinen Geschmack abgewinnen können, sei entgegnet, dass sich nur so dass, was in der Natur "in der Regel" sich ereignet, mathematisch interpretieren lässt. Handelt es sich jedoch um die effektive Konstruktion eines mathematischen Objektes in einer Klasse von Objekten, so ist allerdings das "fast alle Objekte der Klasse" nur ein schwacher Ersatz".

In connection with theory of gases the Brownian motion is an other example of a physical theory that was generalized to abstract mathematical theories. One knows that Einstein started the research in this physical theory in the first years of our century. In the twenties this theory was incorporated in the general framework of stochastic processes and Markov processes where an abstract brownian motion was defined. It is worthwhile mentioning that there is a very curious relation between these subjects and Dirichlet's problem in potential theory [Lamperti, 1966].

(iv). More examples of such developments could be given. But we confine ourselves to some short remarks about typical examples of connections between mathematics and physics. Partly they are of bibliographical character.

In Chapter 1 we already mentioned potential theory. It is a classical theory which found its origin in physics. A new abstract theory, called "axiomatic potential theory" was developed in the fifties of our century. It is a more general theory than the classical theory, which is a theory in $\mathbb{R}^n$. The theory is now developed in a locally compact topological space. Harmonic functions are not defined as solutions of the differential equation of Laplace, but in an algebraic way by means of the theory of sheaves. This theory has a broader scope: there are applications to parabolic equations. This axiomatic theory is the result of modern internal developments in mathematics and is far away from physics.

Hadamard found inspiration in physics to publish his wellknown book

"Le problème de Cauchy et les équations aux dérivées partielles linéaires hyperboliques" (Paris 1921; 1932). It is a sequel to his book "Leçons sur la Propagation des Ondes et les Equations de l'Hydrodynamique". In the foreword he mentions the works of Kirchoff and Volterra on waves. There are several references to Huygens' theory of light. Remind that the most simple example of a hyperbolic equation is the equation of light

$$\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} - \frac{\partial^2 u}{\partial t^2} = 0$$

In this book Hadamard made also some interesting remarks on relations between the differential equation of heat - a parabolic equation - and quasi-analytic functions. This class of functions was introduced without any connection with physics, but in some relation to analytic functions. Next to Hadamard they were studied by Denjoy and Carleman 16). The physical origins are clearly reflected in Hadamard's book. It is a treatise of "classical" character, giving explicit formula's for the solutions. In this respect it is in an essential way different from axiomatic potential theory, where modern tools are used. But Hadamard wrote these books several years earlier.

We mention the recent developments around the differential equation of Korteweg-de Vries. This equation is connected with research on waves in canals; it is a source of pure mathematical studies. See a paper of F. van der Blij "Some details of the history of the Korteweg - de Vries equation" in "Two decades of Mathematics", 1978; see Bertin .

The introduction of new mathematical concepts in physics, in particular in quantum mechanics, has been a reason for several mathematicians to write books, which in some way were intended to serve as introductions to this new apparatus. These books are concerned with analysis, some with subjects in algebra, for instance on groups and group representations. But it must be said that in most cases the references to physics are scarce. We mention some, not having the pretention to be complete.

A. Wintner, Spektraltheorie der unendlichen Matrizen, Einführung in den analytischen Apparat der Quantenmechanik, Leipzig 1929.

G. Julia, Introduction mathématique aux théories quantiques, I, II, Paris 1931.

B.L. van der Waerden, Die Gruppentheoretische Methode in der Quantenmechanik, Berlin 1932.

We mention these books to give an idea of what was published in these years in the field of the relations between physics and mathematics and evidently it is only a choice. A question of a somewhat fundamental character character can then be posed. Such books, in particular those on the algebraic side, are they mainly concerned with applications of the theory of groups - and all that is connected with it - to physical theories or, conversely, are they also a contribution from the side of physics to the creation of new concepts and theories in mathematics? In other words, do they concern a matter of mutual influences? Compare some recent papers [Mackey, 1980], [Choquet-Bruhat, 1980].

We come to some conclusions. The external influences on the development of mathematics in modern time are of essentially other character than those in the classical period. There are still influences of physics, but now there are also impulses from other disciplines such as computer science, theory of programming, connected with discrete mathematics. Nevertheless mathematics - at least what is called pure mathematics - is more autonomous now. The development of pure mathematical theories finding their origins in such impulses goes soon in an abstract direction and I think the origins are soon forgotten and pushed to the background. It would be interesting to know how long the external influences can be observed during the execution of such programs of research. I think it is not for a long time. As soon as such subjects are born, they begin their own independent life. It seems that in this respect the situation was a different one in the classical period.

2.2 Internal evolutions

The situation in the 19th century was rather complicated with respect to the causes of the progress of mathematics. There were mathematicians who seem to have been far away from external influences: Kummer, Kronecker, Frobenius, Dedekind, Cantor. Other mathematicians worked on all domains. In connection with the changing character of the external influences, the following questions arise:

Is it possible to specify in some measure the arguments by which the creation of new ideas and essentially new fields were - and still are - directed in later periods?

Are it mainly the mutual influences of the various disciplines inside mathematics that are responsible for progress in modern time? What are the criteria? In other words: does it concern mainly an internal development?

There are many examples of internal developments in the history of mathematics. Number theory during centuries has been an internal subject. In the last few years this situation has changed. Cryptography has entered the stage see [Lenstra, 1983] to get an impression. There are the researches of Lagrange on problems about the resolution of algebraic equations which later on were important for Abel and Galois. In Part I we mentioned his efforts to detach infinitesimal calculus from infinitesimals and the concept of limit, reducing it to algebra. Lagrange even remarked that algebra is only a branch of the theory of functions (see Volterra, Leçons sur les fonctions de lignes, p.16). These are only some examples. But it is not the aim of this book to write a history of the mutual relations in mathematics: this would comprise nearly the whole history. We will only make some remarks about the period in which modern mathematics started its development - a part of the 19th century - and about mathematics of our time.
Then we must think of the internal influence of the theory of sets. The concept of weak existence is mainly due to this influence.
In Part I we described the tendency towards algebraization of mathematics. By means of results of modern algebra subjects of analysis were given a broader scope. To give an example, let us mention the applications of the theory of fields with a valuation, in particular the fields provided with a non-archimedean valuation. These are valuations in which the norm satisfies the sharp inequality

$$|x+y| \leqslant \max(|x|,|y|),$$

instead of the ordinary inequality

$$|x+y| \leqslant |x| + |y|.$$

The p-adic fields, introduced first by Hensel in 1908, are an example. They were first studied in algebra and number theory. In later years they were introduced in analysis. In all these internal developments topology had to play a role; [Monna, 1970], [van Rooy, 1978], [Taibleson, 1975].

We will make some remarks on the role of geometry, next to algebra a classical subject. In section 1.2 we made the remark that problems on pure existence are often connected with the existence of certain objects in a suitably defined universe or space. In modern analysis the term "space" is very commom: space of continuous functions, spaces of analytic functions, L^p-spaces, Banach spaces of functions etc. Theorems are formulated in terms of spaces. What is the significance of such a geometric denomination? The term "space" suggests an idea of something as "reality", of constructivity in the sense as is usual in classical geometry. Constructivity is then used in a sense that is different from constructivity like we used before in analysis when we discussed problems on explicit solutions, approximations and strong existence. The problem we have to discuss is: What is and has been the role of geometry as an internal influence in analysis?

Before discussing this question, an other one must precede: What is geometry?

We already treated this question in Part I when we discussed the aspects of algebraization. What is called "geometry" developed from a theory of curves and surfaces in the course of centuries to an algebraic-geometric theory where it is perhaps sometimes difficult to say whether it is really geometry or algebra provided with geometric denominations. Is it only a convention? Whatever this may be, an idea of construction and constructivity in a naive classical sense, constructivity which is expected when the term "geometry" is used, is absent in a large part of the subjects that are studied under this name. Moreover, what do we understand by an idea of the perception of "reality" in mathematics, an impression of visualization? Remind the long history of non-Euclidean geometry. Subjects as Incidence geometry (linear or nonlinear), Ring geometry, Finite geometries, Ordered geometries,.... have not much to do with naive reality. Mathematics has its own reality. Perhaps we could say that geometry is a discipline which by its internal structure, the theorems and the objects which they concern, and the way in which the results are formulated, has some analogy with spatial phenomena in a naive sense as we perceive. Ideas of analogy are frequent in mathematics. We do not intend to make an attempt to give a definition which, moreover, we do not need.

Geometry has played a role in analysis, and still does, although this is perhaps not modern abstract geometry, but parts of geometry that can be called "classical". We have not in mind here the trivial situation of the representation of some functions of one or more variables by means of systems of coordinates. There are aspects of a more intrinsic character. Some are concrete, others are more abstract.

Remind the controversies between Weierstrass and Riemann with respect to the foundation of the theory of the functions of a complex variable: the arithmetization of Weierstrass and the approach of Riemann with Riemann surfaces connected with potential theory. Riemann followed the geometric way. Before we mentioned that Klein used certain phenomena in the theory of electricity to illustrate the theory with concrete images (a strong theory). Such an explanation depends essentially on the geometric access. It is not possible in the arithmetical approach of Weierstrass. Both aspects are still present in our time. On the one hand there is the algebraic approach of the theory of algebraic functions (Dedekind and his successors). On the other hand there is the geometric way, for example in the theory of conformal representation with its connections with non - Euclidean geometry, classification of Riemann surfaces, varieties etc. 17).

In modern analysis there are geometric aspects of a less concrete nature. They were introduced since the twenties of our century, partly already earlier. We already mentioned the notion of "space". In most cases it concerns spaces of an infinite number of dimensions. The "points" in these spaces are no more points in the classical, naive and trivial sense. Sometimes they are functions, sometimes they are elements of a set provided with certain structures, algebraic or topological. There are terms as space, subspace, projections, metrical aspects etc. This are concepts of a linear character, but there are also notions of a non linear character: the notion of convexity, the notion of cone when aspects of an ordering play a role. What is the sense of these terms? They are not constructive concepts in a naive classical sense. Is it only a question of terminology or are there deeper reasons to use them? It seems that it is not only a question of terminology.

This question can best be answered by posing an other question : can one imagine modern analysis- or at least those parts in which the terminology and definitions of geometry are used, and this is a large part- without these geometric concepts? Are they indispensable? One could try, but there are reasons to believe that this would be in vain, and, moreover, what would be the advantage? There are, for instance, the theory of Banach spaces and its applications, the role of convexity in several domains (in functional analysis and also in complex analysis), extremal points of convex sets, the theorem of Krein-Milman with its extensions and relations with some problems in measure theory, refinements of convexity, locally convex spaces, fixed points of mappings, retractions etc. 18).

It is indeed possible to define locally convex spaces in an algebraic analytic way by means of families of semi-norms. But semi-norms are in close connection with convex sets and it is just this relation which explains their significance. Minkowski introduced them in his research in number theory and just there convex sets played a role ("Geometrie der Zahlen", 1893). Therefore, the algebraic-analytic definition is rather artificial and one does not see very well the reasons to prefer it.

Thus, geometric methods and concepts are important, often essential, however, in an indispensable combination with concepts of algebra (groups, vectorspaces). But this concerns perceptions of a "reality" which should not be taken in too strict a sense, not in a naive sense of "classical geometry". A contribution to geometric intuition may be very useful and stimulating, often in some way necessary. But it may be dangerous: one should not have too much confidence in such a perception of "reality". To give an example, consider problems of best approximation in analysis. These problems are often studied in a geometric form by considering the shortest distance of a "point" (a function) to a certain vector-space (consisting of functions) or a convex set; the concepts of a metric and orthogonality are used. This is an intuitive geometric picture but one should be aware that certain conditions are necessary in order to be sure that a shortest distance exists. As often in mathematics analogies can play an important role in the creation of theories 19).

The geometric way gives us a tool to explain situations in abstract theories with the aid of geometric images. One can perhaps say that sometimes they take up the place of physical images which, as we have seen, were used in older periods with the aim to come to strong, constructive, illustrations of theories. These geometric images must then be interpreted in an adequate way. I think this are means which for reasons of analogy are scarcely dispensable. For some interesting remarks on the place of geometry see Hilbert's Pariser lecture from 1900 15).

Next to the function of algebra and geometry as disciplines for itself, thereis then ultimately the question of the relative significance of algebraization and geometric methods in the evolution of mathematics. In the preceding considerations there are reasons to think that the aspect of algebraization comes on the first place, being of more intrinsic and vital value than geometric influences. Algebraization is an all penetrating method, which influences the form and the character of the results. Characterizing the evolution in a very global way, one can say that algebraization is a continuation of the way which was first gone by Descartes. The role of geometry and its methods is not like that because there it concerns more the way of pictures, an intuitive setting of theories which may be stimulating but does not lead to a deeper characterization. I think it has sense to speak of "algebraization", but not of "geometrization". On the contrary,"algebraization" applies to geometry .

Final remark.

These reflections on external and internal influences on the evolution of mathematics, and in a more strict sense on the mutual relations between algebra, geometry and analysis in the way of the evolution, necessarily lead to the fundamental question of the criteria by which mathematics is, and formerly was directed. It is a problem that is connected with the still deeper problem of the essence of mathematics. We refer to [Monna, 1984]. See remarks from Dieudonné and Cartan 20).

NOTES

1. The situation can change when less general conditions are imposed. The proof of the theorem of Hahn-Banach, for instance, is non-constructive. But under less general conditions on can give a constructive proof. See [Bishop, 1967].
2. For historical documentation we mention the philosophy of H. Vaihinger, which he developed in his book "Die Philosophie des Als ob" (Berlin 1911). This philosophy has some connections with the problem of existence. According to this philosophy certain notions of mathematics are only fictions, sometimes logical contradictory. But they owe their significance just to this fact.
   Vaihinger developed such a philosophy for several sciences and for mathematics he applies it to the historical problem of differentials (the infinitesimals, considered to be $\neq 0$ in calculations, but if necessary equal to 0). See also non-standard analysis by Robinson. Here it concerns weak existence.
3. Oeuvres de Lagrange, IV, p. 625, "Sur différentes questions d'analyse relatives à la théorie des intégrales particulières". This passage is in "Sur l'intégration des équations aux différences partielles du premier ordre".
4. The problem of the reduction to ordinary differential equations is solved for partial differential equations of the first order. In 1930 Bieberbach remarked that at time it was an open problem whether such a reduction is possible for equations of the second order; attempts had not been successful. I don't know the present state. It would be interesting to have the history of this problem. See: L. Bieberbach, Differentialgleichungen, Berlin, dritte Auflage 1930, p. 343.
5. With respect to the attitude of Cauchy concerning such questions a passage in the "Note Historique" in Bourbaki, Fonctions d'une variable réelle, should be mentioned. Speaking on the question of the existence of the derivative of a function ("Sujet de foi ou non") Bourbaki remarks: "Cauchy, à vrai dire, ne s'y intéresse guère".
   With regards to this question on the sense of existence in the thoughts of Cauchy, E. Neuenschwander drew my attention to a passage in the introduction, written by Chr. Gilain, to a hitherto unpublished Course of Ordinary Differential Equations by Cauchy, now published by C.N.R.S.

Reading this, there are perhaps some reasons to suppose that Cauchy had some idea of a separation between existence and constructivity, but I think the question remains whether he considered both concepts as independent aspects.

6. See for instance:

   (i) Ch. de La Vallée Poussin, Cours d'analyse infinitésimale (1906). Cauchy's theorem is found in the section with the title "Calcul approché de l'intégrale".

   (ii) L. Schlesinger, Einführung in die Theorie der gewöhnlichen Differentialgleichungen auf funktionentheoretischer Grundlage (Berlin, 1922).

   In an introduction Schlesinger writes about "das Bedürfniss nach Nährungsverfahren". He used a method by which the problem is reduced to difference equations, considering the process of convergence. He remarks that already Euler used this way to obtain approximations of the solutions "ohne sich jedoch mit der qualitativen Abschätzung des begangenen Fehlers zu beschäftigen". And then: "Der erste, der versucht hat, dies zu tun, war Cauchy in seiner (1823 gehaltenen) Vorlesungen an der Pariser Ecole Polytechnique".

   Did Schlesinger consider Cauchy's result mainly as a method of approximation?

7. In Part I we treated some developments at the end of the 19th century which went just into an opposite direction. See the remarks on the works of Picard, Vessiot, Drach.

8. For Peano's papers see:

   H.C. Kennedy, Selected Works of Giuseppe Peano, London 1973.

   H.C. Kennedy, Peano, Life and Works of Giuseppe Peano, Dordrecht, Holland, 1980.

   For other demonstrations see:

   (i) J. Walter, On elementary proofs of Peano's existence theorems Am. Math. Monthly, 80, 282-286 (1973).

   In this paper there are references to the problem of constructivity.

   (ii) J. Walter, Proof of Peano's Existence Theorem without using the Notion of the Definite Integral, J. of Math. Analysis and Applications 59, 587-595 (1977).

   Both papers contain an extensive bibliography.

9. In an analogous way Perron introduced a concept of integral, introducing in a suitable way two families of functions and applicating the operators

sup and inf. He introduced this procedure to settle the relation between integration and differentiation as inverse operations.

10. See a paper by A. Kolman, The concept of "simplicity" in the physico-mathematical sciences". In: For Dirk Struik, Dordrecht 1974.
11. I shall not discuss the problematic of pure mathematics versus applied mathematics.
12. Indepently of Poincaré asymptotic series were introduced by Stieltjes in an other area, in this case of "pure mathematics". See
    S.C. van Veen, Thomas Jan Stieltjes (1856-1894).
    In: Chapters in the recent history of mathematics. Special issue of Nieuw Archief voor Wiskunde on the occasion of the Bicentennial celebration of the Wiskundig Genootschap 1778-1978.
    Math. Centrum, Amsterdam 1978.
13. See: Carl Friedrich Gauss 1777-1855. Four lectures on his life and work. Communications of the Mathematical Institute Rijksuniversiteit Utrecht 7-1978.
14. Boltzmann gave the following motivation: "Wenn man irgend eine Curve discutieren will, deren Gleichung einen willkürlichen Parameter enthält, so pflegt man sich oft alle Curven gleichzeitig vorzustellen (....), für welche dieser Parameter in continuirlicher Aufeinanderfolge möglichen Werthe von seinem kleinsten bis zu seinem grössten Werthe hat". [Boltzmann, 1895-1898].
15. P.u. T. Ehrenfest, Enc. der math. Wissenschaften Band IV 2, II Heft 6 (abgeschlossen 1909), Begriffliche Grundlagen der Statistischen Auffassung in der Mechanik.
    It is an important study in this area. On the one hand it contains a review and a bibliography of earlier researches in this domain. There are references to E. Borel, J. Hadamard, P. Poincaré. On the other hand there is a systematical treatment of mathematical character of the ideas of Boltzmann, containing a profound criticism of methods and results, in particular concerning the ergodic hypothesis. Questions of existence (weak or strong) are also a point of discussion. An ergodic system being defined in accordance with Boltzmann as a mechanical system such that, considering the phases in the course of time, the orbit of such a system passes through every point of the phase space being compatible with the total energy, Ehrenfest writes:
    "Nun ist aber die Existenz ergodischer Systeme (d.h. die Widerspruchsfreiheit ihrer Definition) durchaus zweifelhaft: Es ist bis

jetzt nicht einmal das Beispiel eines solchen mechanischen Systems bekannt, bei welchem die einzelne G-bahn jedem Punkt den zugehörigen "Energiefläche" beliebig nahe kommt" (l.c.p. 31).

May we conclude that for Ehrenfest existence meant "free of contradiction", that is weak existence? Compare the remark on Poincaré, p. 116 .

In this publication Ehrenfest treated also the problem of axiomatization of mechanical systems ("Das Axiomatisierungsproblem der Kinetostatik"):

"...Das Schema soll in sich widerspruchsfrei sein. Diese Tendenz zur Axiomatisierung bildet einen wesentlichen Factor in der ganzen neueren Entwicklung das kinetischen Theorie" (l.c.p. 53).

Furthermore: "Inwieweit hat Gibbs das angekündigte Ziel erreicht, eine in sich widerspruchsfreie statistische Mechanik zu begründen?"
Is the influence of mathematical developments on physical theories apparent?
Remind that Hilbert had already posed the problem of axiomatization of physics in his famous problems (Paris (1910). See
(i) "Die Hilbertschen Probleme", Ostwalds Klassiker 752, Leipzig 1971.
(ii) Proc. of symposia in pure Mathematics; Mathematical developments arising from Hilbert's problems I,II, Providence 1976. For axiomatics in physics there is the interesting book:
R. Giles, Mathematical Foundations of Thermodynamics, Oxford etc. 1964. This book contains references to Bourbaki.

16. It seems that they were first introduced by Hadamard. See: Cahiers du Séminaire d'Histoire des Mathématiques 1, Paris 1950, p. 66.

17. To illustrate we mention some books, older books, and some more recent. The choice is rather arbitrary.
(i) G. Julia, Principes géométriques d'analyse, Première partie 1930, deuxième partie 1932, Paris.
(ii) L.V. Ahlfors, Conformal invariants, Topics in Geometric Function Theory, New York etc. 1973. In the foreword the author expresses his preference for the geometric approach.

In his studies on the theory of automorphic functions Poincaré remarked: "La géométrie non-euclidienne est la clef véritable du problème que nous occupe". (Acta Math. 39, 1923, p. 100).

Of fundamental importance is:

H. Weyl, Die Idee der Riemannschen Fläche, Leipzig 1913, 1923.

We quote the following passage from the "Vorwort":

"Man begegnet noch hier und da der Auffassung, als ob die Riemannsche Fläche nichts weiter sei als ein "Bild", als ein (man gibt zu: sehr wertvolles, sehr suggestives) Mittel zur Vergegenwärtigung und Veranschaulichung der Vieldeutigkeit von Funktionen. Diese Auffassung ist von Grund aus verkehrt. Die Riemannsche Fläche ist ein unentbehrlicher sachlicher Bestandteil der Theorie, sie ist geradezu deren Fundament. Sie ist auch nicht etwas, was a posteriori mehr oder minder künstlich aus den analytischen Funktionen herausdistilliert wird, sondern muss durchaus als das prius betrachtet werden, als der Mutterboden, auf dem die Funktionen allererst wachsen und gedeihen können. Es ist freilich zuzugeben, dass Riemann selbst dies wahre Verhältniss der Funktionen zur Riemannschen Fläche durch die Form seiner Darstellung etwas verschleiert hat - vielleicht nur, weil er seinen Zeitgenossen alzu fremdartige Vorstellungen nich zumuten wollte; dies Verhältniss auch dadurch verschleiert hat, dass er nur von jenen mehrblättrigen, mit einzelnen Windungspunkten über der Ebene sich aubreitenden Uberlagerungsflächen spricht, an welche man noch heute in erster Linie denkt, wenn von Riemannschen Fläche die Rede ist, und sich nicht der (erst später von Klein zu durchsichtiger Klarheit entwickelten) allgemeinen Vorstellung bediente, als deren Charakteristikum man dieses nennen kann: dass in ihr die Beziehung zu der Ebene einer unabhängigen komplexen Veränderlichen, sowie überhaupt die Beziehung zum dreidimensionalen Punktraum grundsätzlich gelöst ist."

18. To get an impression of the influence of geometrical concepts one has only to look in general treatises on functional analysis. There are also books on more special topics which may serve as illustration, for instance:

(i) H. Bauer, Konvexität in topologischen Vektorräumen; Vorlesung an der Universität Hamburg 1963/64.

(ii) R.R. Phelps., Lectures on Choquet's Theorem, Princeton 1966.

See [Monna, 1970] for the concept of a spherically complete space, in geometrical as well as in algebraic context.

19. Compare the extremal problems of Gauss, Dirichlet, Riemann we mentioned before.

20. J. Dieudonné, Orientation génerales des Mathématiques en 1973, Gazette des mathématiciens, Soc. Math. de France, Octobre 1974.

For some comments see:

B. Malgrange , A propos d'un article de J. Dieudonné, Gazette des Mathématiciens, Soc. Math. de France, février 1975.

See also:

H. Cartan, Médaille d'ordre C.N.R.S. 1976. Gazette des Mathématiciens, Soc. Math. de France, février 1977.

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