HISTORIA MATHEMATICA 3 (1976), 297-320

T. S. KUHN'S THEORIES AND MATHEMATICS: A DISCUSSION PAPER ON THE "NEW HISTORIOGRAPHY" OF MATHEMATICS

BY HERBERT MEHRTENS, HAMBURG

SUMMARIES

A discussion of the applicability of the concepts of T. S. Kuhn's theory of scientific revolutions, stimulated by the statement by M. J. Crowe of ten "laws" concerning change in the history of mathematics. The concepts "revolution" and "crisis" are rejected, while the concepts centering around the sociology of the scientific community are accepted for systematic use in historiography of mathematics. This is supplemented by the consideration of extra-mathematical influences. Finally the "laws" of Crowe are shown to be explainable with those concepts.

Angeregt durch zehn "Gesetze," die M. J. Crowe für die Entwicklung der Mathematik aufstellte, wird die Anwendbarkeit der Begriffe aus T. S. Kuhn's Theorie der wissenschaftlichen Revolutionen auf die Mathematik diskutiert. Die Begriffe "Revolution" und "Krise" werden zurückgewiesen, während jene Begriffe für einen systematischen Gebrauch in der Mathematikgeschichte akzeptiert werden, die die Soziologie der wissenschaftlichen Gemeinschaft betreffen. Ergänzend werden aussermathematische Einflüsse diskutiert. Schliesslich werden Crowes "Gesetze" mit Hilfe der entwickelten Begriffe erklärt.

1. *INTRODUCTION*

In a paper published recently in *Historia Mathematica* M. J. Crowe tried "to stimulate discussion of the historiography of mathematics by asserting ten 'laws' concerning change in mathematics." [Crowe 1975, 162]. His starting point is the "new historiography of science", whose basic book is T. S. Kuhn's essay *The Structure of Scientific Revolutions* [1970a], the first edition of which appeared in 1962. There has been much discussion on Kuhn's theses since. Eventually Kuhn had to refine some of his concepts, which was done in the important *Postscript 1969* to the second edition of his book and in future papers [1970b, 1970c].

Kuhn states that "historians of science have begun to ask new sorts of questions and to trace different ... lines of development" [1970, 3]. Here is the core of what has been called the "new historiography" of science. Historians of science explicitely take up the conceptual and theoretical background mainly from philosophy and sociology. They try to pose good and fruitful questions in order to gain a broad and adequate understanding of how the sciences and mathematics develop. For the field of mathematics Crowe has pointed to two important examples, namely, a paper of I. Lakatos [1963/64] and the studies of R. L. Wilder [1968, 1974]. Crowe's own "laws", though, leave the questions still to be asked and provide little conceptual material for a better understanding of the history of mathematics.

Still the "laws" point to some important regularities that have incited me to take up the discussion. I shall try in this paper to tie up those regularities in a conceptual frame that is basically Kuhnian.

In the main part I shall discuss the applicability of Kuhn's theory and concepts to the history of mathematics. Starting with a short description of Kuhn's theory, I shall then discuss the concepts in connection with mathematics, going from the general pattern of change, revolutions, crises, through more specific concepts like the scientific community, to the different elements of the disciplinary matrix. Section 3 will turn to extramathematical influences, and in section 4 Crowe's "laws" will be discussed in terms of the concepts developed in the paper.

2. Kuhn's Concepts and their Applicability to Mathematics

2.1. Kuhn's theory

Kuhn's basic concept is that of the "scientific community." "A scientific community consists ... of the practitioners of a scientific specialty. To an extent unparalleled in most other fields, they have undergone similar educations and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it ... Within such groups communication is relatively full and professional judgement relatively unanimous. ... Communities in this sense exist, of course, at numerous levels. The most global is the community of all scientists." [Kuhn 1970a, 177]

The group is constituted by the common background of its members. That is what Kuhn called the "paradigm" of the specialty "A paradigm is what the members of a scientific community share, *and*, conversely, a scientific community consists of men who share a paradigm."[*ibid.*, 176] This concept found many critics, and Kuhn had to refine it into the "disciplinary matrix": "'disciplinary' because it refers to the common possession of the practioners of a particular discipline, 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification." [*ibid.*, 182]

As elements of the disciplinary matrix Kuhn lists the following four (though there are more):
-- "symbolic generalizations", expressions like f=ma that are legislative as well as definitional [*ibid.*, 182-183],
-- "beliefs in particular models", like the belief that heat is the kinetic energy of the constituent parts of bodies [*ibid.*, 184],
-- "values" about the qualities of theories, of predictions, of the presentation of scientific subject matter, etc. [*ibid.*, 184-6],
-- "examplars" or "paradigms", concrete problem solutions that show how the job should be done [*ibid.*, 187-91].

Kuhn now distinguishes two main forms in the development of science: "normal" and "revolutionary" (or "extraordinary") science.

Along the lines of the accepted disciplinary matrix the scientist is able to choose problems which are relevant and solvable with high probability. The elements of the disciplinary matrix act like rules in assuring the solvability of the problem. Furthermore the exemplars provide the guidelines for research. This kind of work is like "puzzle-solving." A failure to solve such a normal problem will be attributed to lack of patience or intelligence in the scientist. Only after such failures become spectacular - either because of the reputation of those who tried or because of their number - will the elements of the disciplinary matrix be questioned. The type of research where no spectacular problems turn up is "a strenouos and devoted attempt to force nature into the conceptual boxes supplied by professional education" [Kuhn 1970a, 5]. Kuhn calls it "normal science." To him the "puzzle-solving" is the demarcation criterion for mature sciences.

Regularly nature shows "anomalies", phenomena that turn out to be resistant to the customary pigeon-holing. Often such phenomena are laid aside for later generations with better tools. Sometimes the persistent failure to deal with an anomaly leads to small deviations in the disciplinary matrix which eventually allow integrating the anomaly in a fairly "normal" way in the theory. If this does not happen, the scientific community is disturbed. Its members gradually come to recognize that there is something wrong with their basic beliefs. This is the state of "crisis" in the scientific community. The otherwise strong bonds of the disciplinary matrix tend to be loosened, and basically new theories and solutions, new "paradigms" may evolve.

There is no rational choice between the old and the new paradigm. The reasons for the choice of a theory (explanatory power, fruitfulness, elegance, etc.) act rather as values than as rules of choice. A shift of paradigm is like a "gestalt-switch"; the scientists perceive nature in different ways. The concepts, symbolic generalizations, etc., if retained in the new paradigm, have a different meaning because of a new linguistic context. This incommensurability thesis has been much discussed; its elaboration by Kuhn [1970c, 259-277] shows the way he views scientific development very clearly.

Kuhnian revolutions are not only fundamental changes in world view occurring once every century. Revolutions are "a little studied type of conceptual change which occurs frequently in science and is fundamental to its advance." [Kuhn 1970c, 249-50] Toulmin has used the word "micro-revolutions" for this [Toulmin 1970, 47]. The revolutions are always seen with respect to a scientific community which may be very small; and revolutions may involve only parts of the disciplinary matrix.

2.2. *The Application of Kuhn's Concepts to Mathematics*

2.2.1. *The Pattern of Scientific Change*

Kuhn's starting point is the social psychology (or sociology) of the normal scientific community. The counterpart of the scientific community is nature. From this he develops his dualistic view of scientific change.

In principle this is transferable to mathematics. Without going into the question of whether mathematics is in any way concerned with nature, one can say that mathematics is about something that offers resistance to the mathematician and calls for treatment. More than in the natural sciences the problems to be treated are determined by mathematics itself. But this is only a difference of degree. The relation between the mathematicians and their subject is very much like that of the natural sciences.

Kuhn takes the scientific community as clearly identifiable, relatively isolated within the greater communities, and as relatively free from extra-scientific influences. This turns his whole theory into a strong idealization that has found severe criticism from historians of science [cf., e.g., Meyer 1974]. If there is any such pattern of change in mathematics, it is certainly not easily seen. There are intertwining developments of different mathematical disciplines, extramathematical influences of various kinds, and so on. There does not appear to be a general pattern of change in mathematics that can be applied in historiography. But there are many regularities (as Crowe's "laws" show) and these can be treated and partly explained by Kuhn's concepts. Thus I will discuss the applicability of the concepts, starting with "revolution" and going to the elements of the "disciplinary matrix."

2.2.2. Revolutions

In the application of Kuhn's concepts to mathematics there are generally two questions involved: is there such a thing in mathematics? and if so, is the concept of definite, fruitful use in the historiography of mathematics? First, are there revolutions in mathematics? Consider this example:

Until well into the Nineteenth Century the Cambridge and Oxford dons regarded any attempt at improvement of the theory of fluxions as an impious revolt against the sacred memory of Newton. The result was that the Newtonian school of England and the Leibnizian school of the continent drifted apart ... The dilemma was broken in 1812 by a group of young mathematicians at Cambridge who, under the inspiration of the older Robert Woodhouse, formed an "Analytical Society" to propagate the differential notation. ... This movement met initially with severe criticism, which was overcome by such actions as the publication of an English translation of Lacroix' "Elementary Treatise on the Differential and Integral Calculus"*(1816). The new generation in England now began to participate in modern mathematics.* [Struik 1948, 246-8]

For the English mathematical community this was a revolution. A substantial part of the disciplinary matrix, the commitment to the Newtonian system of notation, was overthrown. This is but one very suggestive example, and there are more.

Still, Crowe holds that there are no revolutions in mathematics ["law" 10 in Crowe 1975, 165]. He would probably reject the given example by saying that this is not a revolution *in* mathematics. To him "the preposition 'in' is crucial" [*ibid.*, 166]. Unfortunately he does not explain what *in* mathematics means, except that nomenclature, symbolism, metamathematics, methodology, and historiography are not *in* mathematics. Probably Crowe has the "contents" or the "substance" of mathematics in mind (what is this?).

But take an example: a piece of mathematics very much in the sense of Crowe would be Taylor's theorem, which has been invariably valid since its publication in 1715. But is it of the same content in Taylor's original publication and in modern textbooks? There is always a wide background connected with such a theorem. Today the function concept is completely different, infinitesimal analysis is set up on the basis of general topology, with Taylor's theorem the mathematician has a generalization to Banach spaces in mind, and so forth. Still there is something more than mere tradition connecting the theorem of 1715 and that of today. The example should show that this "content" is difficult to grasp. One cannot possibly strip the contents from nomenclature, symbolism, metamathematics, etc.

There is a danger for the historian of mathematics in this preposition *in*. The mathematician of today tends to declare all history the prehistory of the mathematics he knows. Thus everything which is included in or derivable from modern mathematics is *in* mathematics. The historically significant features like the use of concepts, the general beliefs concerning the discipline, etc. are naturally not *in* mathematics. Crowe has shown that he is not guilty of such a standpoint. But I should very much like to know how he explains his preposition "in".

To close the discussion of this point I shall take up another example. Take, say, van der Waerden's *Moderne Algebra* of 1930 and any algebra textbook of the 1830s. The difference is striking; the complete set of the closely connected realms of contents, terminology, symbolism, methodology, and the implicit metamathematics has changed. All these elements are interwoven: a concept, e.g., is not only determined by its proper content as given in the definition, but it also is determined by the connexions in which it is used. Thus there is a "metaphysics" to it. Furthermore every single one of the elements is substantial to the theory as it historically occurs. Consequently, I should say that changes in methodology, symbolism, etc., are changes *in* mathematics.

Few, if any, mathematical theories have been completely overthrown, but many theories have become obsolete or have been modified to an extent that there is hardly any resemblance. These changes have frequently been the consequence of an interplay of changes in the "contents" and the "metaphysics" of a discipline. An example is the transformation of classical algebra to modern algebra, the "death" of invariant theory [cf. Fisher 1966, 1967] being a part of this general development.

So far I have shown that there are events in the history of mathematics that might be termed "revolutions" and that there is no point in distinguishing such events with respect to their being "in" mathematics or somewhere else. The example of a "revolutionary" development which opened this section is very suggestive, because in this case the connotations of the word "revolution" are appropriate. There are many words which can be used to express the historical importance of an event; "revolution" is one of these. The implicit analogy to political history is a means of expression for the historian as a writer. But if one is looking for a concept that is to play a methodological role in historiography, guiding and helping research and interpretation in the history of mathematics, this imaginative force of such a connotation-laden word is rather a danger. This is obvious when one comes to talk of "micro-revolutions." Here the connotations are certainly misleading.

This answers the second question, whether the concept is useful. After I have rejected Kuhn's dualistic scheme as a general pattern for the historical development of mathematics there is nothing left to justify the use of the concept of "revolution" as part of a methodically applied conceptual frame.

It would be very nice to have such a conceptual frame for the classification (and explanation) of types of change in the history of mathematics. I know of none. My proposal is to use the concept of "normal" mathematics (which will be discussed below) as a tool in the assessment of mathematical innovations: relate the innovation to the contemporary mathematical background (the disciplinary matrix), see if it is a piece of normal mathematics, and if it is not, treat it individually, finding out what exactly is non-normal about it and why this is so.

2.2.3. Crises

Like "revolution" the concept of "crisis" has its main value as part of Kuhn's pattern of scientific development. Not accepting this general pattern for mathematics one can still debate the role of Kuhnian crises. They mark the phenomenon that in a given mathematical community - for whatever reasons - the common commitments of the group are questioned and, consequently, the stability of this social system is at risk. The mathematicians will be more apt to develop ideas deviating from the common background of their group and stimuli from outside the community will be more easily accepted. In this way one can perceive crises as functional for scientific development.

We know of crises in mathematics, the so-called "foundation crises."[1] Lacking space to elaborate this, I can only state that I doubt that a functional view of these crises as explained above is historically adequate. This is partly the result of an ambiguity of the term in its intuitive application to mathematics. It means always a social crisis of the community of mathematicians; but it is also seen as a crisis of mathematics, which implies that if a basic logical contradiction turns up, then there is a crisis. The problem with the concept thus is to see what historical evidence can justify the use of the term "crisis." Having no solution to this problem and with strong doubts about the possibility of using the concept in historical explanations (concerning mathematics), I should rather not give it a systematic use [2].

The remaining problem is how the functions of crises in Kuhn's scheme are accomplished in mathematics. For one thing -- as Crowe has stated ["law" 9 in 1975, 165] and Lakatos [1963/64] has illustrated -- mathematicians have a vast repertoire of techniques for handling problems that might generate crises. One of these techniques is to ignore foundational problems and to rely on the applicability or the fruitfulness of mathematical concepts and theories. Further the mathematical communities are open systems. The interaction of the mathematical disciplines and their relation to extra-mathematical fields as well as the variance among the individual mathematicians allows basic changes in the long run without going through crises of the whole community [3]. The following sections will elaborate some of these ideas.

2.2.4. Anomalies

Anomalies are phenomena that do not follow the expectations from the accepted disciplinary matrix. A prominent example in mathematics is Euclid's fifth postulate, which eventually led to new geometries and to the overthrow of the "metaphysics" of geometry.

The example shows that anomalies play a decisive role in the history of mathematics. As stated above anomalies frequently can be handled by existing techniques. Often concepts are modified to integrate or to exclude anomalies (cf. Lakatos' "monster-barring"; [1963/64]). Inquiries into the ways mathematicians react to anomalies can give valuable background to the historiography of mathematics.

"Anomaly" is a relative term. It points to the relation between a phenomenon and the expectations and the background of the mathematicians. As such it is an important concept for the assessment of the background and of innovations in the history of mathematics. The fifth postulate did not agree with the characteristic self-evidence of Euclidean axiom and postulates. It led to the abandonment of the belief in one unique geometry, resting on self-evident assumptions.

A less familiar example is the invention of ideal primes and ideals by Kummer and Dedekind. Kummer tried to prove Fermat's theorem (which itself is an anomaly in number theory). He applied techniques acquired only recently in the realm of complex integers. His expectation was that the methods and theorems of the theory of natural numbers should be extendable to algebraic integers. So he came to the erroneous idea that these were uniquely resolvable into prime factors. After the error had been pointed out to him by Dirichlet, he tried to integrate this anomaly and invented his ideal primes, more exactly the divisibility by ideal primes. Dedekind took up Kummer's line of research, attempting to develop a general theory of algebraic integers along the lines of classical number theory in the form given to it by Gauss and Dirichlet. Retaining the concept of congruence in its central place, he tried to develop the theory in terms of higher congruences. But in this approach he encountered further anomalies. Eventually he was led to the invention of the concept of "ideal", completing the theory by the use of "fields" and "modules". This solution, reached only after years of strenuous work, was a deviation from the usual background in that the new entities were non-constructively defined sets of numbers. In summary, algebraic number theory was developed by the interplay of the force of the leading principles given by Gaussian number theory on one hand and the anomalies showing up in different fields of algebraic numbers on the other hand. This very rough sketch should have shown the often important role of anomalies [6].

There are many more examples of different degrees of conspicuousness. The reaction of the mathematical community to an anomaly depends on the strength of beliefs that are violated by the anomaly. On the Pythagorean background the incommensurable must have been scandalous. But the background does not have to be that of the whole community. An unspectacular example is E. Schröder's proof of the independence of distributivity in the axiomatics of Boolean algebra [4]. Schröder had given distributivity as an axiom in his first presentation of the algebra of logic [Schröder 1877]. C. S. Peirce, whom Schröder admired very much, in a paper of 1880 affirmed distributivity as a theorem but omitted the proof. Related to Schröder's expectations this was highly anomalous, and consequently he gave the matter much consideration. He was led to the division into the two distributive inequalities and found the proof for one. By a model taken from his other researches he managed to show the other one to be unprovable. These inquiries made him see the applicability of the structure defined by those axioms that in his setup came before the law of distributivity. He stated the concept of a lattice (with *0* and *1*) which he called "logischer Kalkul mit Gruppen" and applied it to problems of today's universal algebra [Schröder 1890, I, Anhang 4-6]. The anomaly thus had led to interesting developments, which nevertheless were without effect on the history of mathematics: almost nobody was interested in structures generalizing Boolean algebra for the following thirty years. Schröder had not given any other proofs of independence. But soon the modern axiomatic method made these a standard procedure and the law of distributivity was no longer an anomaly. Peirce's proof, which he eventually produced, just rested on another axiom.

This example should show, besides the role of an anomaly, the general pattern of my application of Kuhn's concepts. I have rejected the concepts "revolution" and "crisis" in spite of the existence of phenomena that might bear these names. The reason was that these concepts cannot be formed into forceful tools for historical inquiries. I believe and have tried to show that the concept of "anomaly" is such a tool. It is a clue to important causal connections. Relating innovations in mathematics to the contemporary background, it helps to understand and assess historical developments.

2.2.5. Normal Science

The kind of work Kuhn calls "normal science" is done in mathematics, too. For example, most doctoral theses are "normal" "normal" mathematics. A thesis is decisive for the academic career, and consequently there is a strong tendency to take up problems that promise to be solvable by standard methods. Most of mathematics is done in a normal way following the rules learnt from the usual texts, solving standard problems, filling holes in a theory, generalizing concepts, sharpening conditions, etc. The elegant, comprehensive, streamlined, final textbook version of a theory is only attained after a period of normal research on the theory. This normal type of research is furthermore a sign showing that a discipline or theory has become an accepted part of mathematical work.

Adequate description of normal mathematics in history is a difficult problem. There is a normal tendency to concentrate on the great mathematicians and their discoveries and to overlook normal mathematics. This is unsatisfactory, but an attempt to describe a development in mathematics completely may result in an unreadable collection of facts. A possibility to avoid this dilemma might be the generalizing description of the main streams in the normal work on a theory, using the elements that are guiding the normal work (which will be discussed below under the heading "elements of the disciplinary matrix").

Since I do not want to talk about "revolutionary" mathematics, one might object that there is nothing left but normal mathematics. But "normal" is, like "anomaly", a relative term. It relates a piece of mathematical work to the contemporary "norms". A mathematical paper can be extraordinary in many ways: in the choice of the problem, in the applied methods, in the extension of known concepts, and so forth. For a proper understanding of the historical status of a mathematical contribution it is essential to see exactly what is non-normal about it and to find out how this could come about

2.2.6. Scientific community [3]

The foremost value of the concept of scientific community is that it does away with the impression which often can be won from discussions on mathematics and its history, namely, that mathematics is a package of eternal, spiritual truths gradually unwrapping itself in the course of history, visible only to the inner eye of singular geniuses who make them accessible to diligent research students. In fact mathematics is man-made; its vital basis is the social interaction of mathematicians in their scientific community. No mathematician starts from nothing. He has to build up on mathematical tradition. In the course of his mathematical education, be it formal or otherwise, he acquires a "tacit knowledge" about mathematics, the way to talk about it, its aims and methods, etc., which enable him to communicate with his fellow mathematicians. He becomes a member of their community, more or less conforming to its way of doing things and to its norms.

He strives for recognition by his colleagues. Even outsiders such as H. Grassmann try to get their message across to the mathematical community, in Grassmann's case by the elaboration of his *Ausdehnungslehre* in a second edition.

I am not trying to explain everything about mathematics in social or sociological terms. Much depends on the personal biography of a mathematician, which seldom can be investigated closely enough. Furthermore there are phenomena which seem not explainable at all, at least not in any satisfying way. Nevertheless the social conditions, especially those of the closer community of mathematicians, are the basis of the development of mathematics.

In spite of this basic value of the concept of scientific community, there are serious shortcomings to it. It is certainly applicable to the mathematics of the twentieth century in the sense of Kuhn that first the community is to be identified and then the disciplinary matrix. But going back in history it becomes difficult to identify fairly clear-cut mathematical subcommunities. One can identify the community of all mathematicians, but even this is partly coextensive with the community of astronomers and of physicists. There is the danger of a rather "presentist" approach to history. Thus the concept should be used with some care (but it should be used). In treating mathematical disciplines it should be established whether there was a corresponding community that can be identified (which can be done by the inspection of the communication between the members). Then there is the question as to which of the common commitments are specific to the subcommunity. Here is the connection to the concept of disciplinary matrix, which is the complement to that of the scientific community.

2.2.7. Disciplinary matrix

In the preceding sections I have frequently used the word "background" in talking of the common commitments of the members of the mathematical community. This background is meant by the concept of disciplinary matrix. The concept is the main tool for analysis of the common background and at the same time it is of high explanatory power for historiography of mathematics.

The disciplinary knowledge of a mathematician consists of theories, theorems, methods of proof, methods of presentations, a symbolism, a terminology, and so on. Furthermore there is a set of beliefs concerning the general value of mathematics, what it is about, and more things like that. Then there exist values about the aesthetics of mathematics, the role of applications, methods of proof, etc. This complicated and far reaching background is a bit different for each individual. But it is acquired in a learning process in a given social environment on a subject of very definite structure, mathematics. Consequently, there is a strong common background of the members of the mathematical community. The community bonds are the different types of communication, books, papers, correspondence, etc., the basis of which is the common language, the common knowledge and the common commitments, in short, the disciplinary matrix. Thus the disciplinary matrix has an important social function for the community and an emotional function for the individual member. The mathematician who sticks to the common commitments of his group can feel safe; he secures his identity by relying on the disciplinary matrix. This phenomenon explains a large group of Crowe's "laws" as will be shown in section 4.

The disciplinary matrix determines the things the mathematical community is rather conservative about. At the same time it is the guideline for normal mathematics.

2.2.8. The elements of the disciplinary matrix

For a closer investigation of historical developments there should be some treatment of the important elements of the disciplinary matrix. To do this convincingly one should try to classify the different elements that are determinant for the work of mathematicians. Evidence for the importance of the categories must be given. The ways mathematicians acquire the beliefs, norms, etc., should be investigated, and furthermore the priorities in the matrix and the interdependence of its elements ought to be discussed. Finally there should be an attempt to analyze the relation of the matrices at the different levels of mathematical communities. This could be done in a detailed case study which is not in the scope of this paper. I can but give some propostions, raise some questions, and give a few examples.

Of the elements of the disciplinary matrix I shall discuss five: beliefs in particular models, values, exemplars (or paradigms), concepts, and standard problems. The first three have been given by Kuhn. The latter two I believe to be specific to mathematics.

2.2.8.1. Beliefs in particular models

Here I refer to such "metaphysical" beliefs as "mathematics is the science of magnitude", "arithmetization gives an appropriate basis to analysis", the programmes of formalism, logicism, and so on. Such basic beliefs seem to be what Crowe has in mind in his "law" 5 [1975, 163] where he mentions the multilayered "knowledge" of mathematicians. There are models also in a more heuristic sense, such as the view of a curve as the path of a moving point. I am not convinced, though, of the usefulness of the latter concept of "model" in the historiography of mathematics.

2.2.8.2. Values

In the history of mathematics values play an important role for the mathematical community as well as for the individual mathematician. There are values that belong to the larger communities of all scholars such as those R. Merton [1968] has explored. The community of mathematicians shares values about how research should be done, how results should be presented, and about the worth of subjects, methods, and problems. One such value of high priority is that mathematical innovations should be fruitful and applicable (in mathematics or outside of it). The system of values changes historically. An interesting question is the comparison of the values of fruitfulness and of rigor. The history of imaginary numbers shows that fruitfulness dominated rigor. Another instance is Dedekind's statement that he did not publish his ideas on irrational number immediately because the matter was "so wenig fruchtbar" [1930/32, III, 316]. Rigor as a value gained more and more weight during the nineteenth century. This development displays two closely connected points about values. First, the historical variation of values is determined by the material circumstances of life which allow living according to such values or not. On the other hand values influence these circumstances. Here the second point comes in. In the case of the value of rigor it bears the more weight the easier it is to exert rigor in mathematical work. These means of rigorous mathematics have been created by mathematicians like Weierstrass, Dedekind, Peano and others who valued rigor in their work exceptionally highly. Thus there is not only the interplay between the values of the mathematical community and the material circumstances, but also the interaction between individual and community values.

2.2.8.3. Exemplars, paradigms

The concept of paradigm was from the beginning the most spectacular in Kuhn's theory. Paradigms are shared examples that structure the mathematicians' perception and guide their research. I should like to use the word "paradigm" for achievements that govern mathematical development in many ways and for a lond period. Examples are the *Elements* of Euclid, Archimedes' procedures in caluculus, Gauss' *Disquisitiones Arithmeticae,* Boole's *Laws of thought,* and similar works. A paradigm in this sense is much more than a problem solution. It embraces basic concepts, standard problem-solutions, a specific symbolism and terminology, and often it has a strong value-generating force.

In Boole's *Laws of Thought,* for example, the main paradigmatic elements were the application of algebraic symbolism and procedures to logic and the treatment of logical equations. Until about 1900 the book was of central influence on the development of mathematical logic. By that time a new programme was pursued. If we want to speak of a "paradigm" in this case, it was generated by various papers and books by C. S. Peirce, Dedekind, Frege, Peano, and others. The leading principle was no longer the application of mathematics to logic but, conversely, the application of the means of symbolic logic to the foundations of mathematics, which was made possible by the results attained in the preceding period. The change of paradigms in this case is quite complicated and can hardly be termed "revolutionary". Still there are traces of the incommensurability-thesis to be found, as in Schröder's lack of appreciation of Frege's aims [cf. Lewis 1966]. Maybe this type of change of paradigms could be treated in terms of Lakatos' conception of competing research programmes [Lakatos 1973].

The more restricted types of paradigms, which I shall call "exemplars", are exemplary problem-solutions. One example is the geometrical representation of complex numbers which acted as an exemplar in the quest for a similar system of space analysis resulting in Hamilton's quaternions [cf. Crowe 1967, 5-12]. The example displays an important trait of exemplars. They may suggest solutions of problems quite different from the originally solved problem. In the example the problem was the "possibility" of imaginary numbers; they were given a material substratum by geometrical representation. The representation is convertible, and what was sought with the complex numbers in mind was an algebraical representation of three-dimensional space.

Many exemplars show an important trait of mathematical development, namely, the fact that achievements in one field act in specific aspects as exemplars to another field. Again, the geometrical representation of complex numbers is an example. The different interpretations of complex numbers made visible the abstract process of interpretation which was taken up by the English mathematicians in their conception of a symbolic algebra [Novy 1973, 194]. Exemplars influence the mathematicians' way of seeing his subject. This is still clearer in the application of the concepts of algebraic number theory to algebraic functions. After Dedekind had worked out his theory of algebraic numbers fairly thoroughly he perceived the similar structure in the realm of algebraic functions. In collaboration with H. Weber he worked out the theory of algebraic functions strictly along these lines [Dedekind 1930/32, 283-350].

2.2.8.4. Concepts

Maybe concepts are the analogy to Kuhn's symbolic generalizations [5]. They certainly play an important role in the history of mathematics. The concept of function, e.g., is a well known theme of historiography. As elements of the disciplinary matrix concepts are closely connected with values and with the beliefs in "metaphysical" models. For the working mathematician who does not care much about questions of ontology the concepts he knows determine what exists in mathematics. From this there arises a, generally implicit, idea of what kind of concepts are allowed. The reaction to deviations is individually different; Kronecker is a well known extreme.

Like exemplars, concepts guide normal mathematics. They also set boundaries. In Hamilton's quest for a vectorial system, the prevailing concept of multiplication restricted the possible operations to commutative ones. This example will be elaborated in connexion with Crowe's laws below.

There is much to be said about concepts in mathematics. Wussing [1970] has proposed a pattern for the historical development of scientific concepts which is, in aspects, quite intriguing and might be applied to the problems of the development of disciplinary matrices.

2.2.8.5. Standard problems

From the unique prime factorization of natural numbers descends a heap of factorization theorems in modern algebra. One could consider this as an exemplar, but a new factorization theorem in some esoteric branch of modern algebra is not formed according to given exemplars. Rather factorization is a well known procedure, there are certain techniques, and factorization problems are generally recognized as worthwhile objects of research. In short, factorization is a standard problem. Further, standard problems do not call for a complete solution as given by some prototype. This is more clearly visible in the case when the problem did not initially find a solution such as the word problem in groups and other algebraic structures. Most of the "open problems" listed in textbooks are of a standard type. Besides factorization, examples are decomposition, representation, axiomatization, generalization, etc. These problems are of different levels. Generalization, for example, has been considered by Wilder [1968, 173] as an "evolutionary force" in mathematics. I should rather not talk of such an everacting "force." The value "mathematical results should be as general as possible" has been of varying weight in the course of history. What I have in mind is generalization as a standard procedure occuring in the developed mathematics of the nineteenth and twentieth centuries. As soon as there is a hierarchy of structures, the expansion of theorems valid for one structure to the more general one is attempted. In the case of generalization the close relation between values and the standard problems is visible. Successfully solved problems (not necessarily standard) influence the values about problem choice, and the values determine the range of (not the individual) standard problems. Since the values about the quality of problems are "tacit", they are acquired through the knowledge of prevailing standard problems. The application of the problems is guided by values; much generalization is done but not published because it is just trivial generalization. There has to be more to it, "fruitfulness" for example. Standard problems are frequently (always?) generated by exemplars and they are one of the causes of multiple discoveries, in many cases bringing forward new exemplars or paradigms.

2.2.8.6. Further elements

The elements of the disciplinary matrix are not separated sharply. There is strong interaction and interdependence. Thus one might structure the matrix by other concepts than mine; this is open to discussion. Further elements that might be considered and given individual discussion are "symbols", "methods", and, perhaps "restrictions". Since I have described models, values, and concepts as acting partially restrictively on mathematical work, "restrictions" are subsumed there, but it is discussible if in certain historical cases this assumption is feasible. In the same way the discussion should in general include all periods in the history of mathematics (which I have not tried to do).

2.3. Conclusion

The general pattern of T. Kuhn's theory of the structure of scientific revolutions seems to be not applicable to mathematics. But many of Kuhn's conceptions remain valuable for the historiography of science even if the basic pattern of the theory is rejected. The concepts centering around the sociology of groups of scholars are of high explanatory power and--in my opinion--supply key conceptions for the historiography of mathematics. They illuminate the relation of mathematical achievements to the contemporary background. In this way they can serve to explain and understand them and do more justice to the mathematicians of the past, their efforts and failures, than the prevailing tendency to view past mathematical achievement only in the light of their long-term effects.

The examples used are almost exclusively from the 19th and 20th century. I do not insist that the concepts are applicable in all cases. I do not wish to make historiography of mathematics completely Kuhnian. It should have become clear that I am looking for satisfying historical understanding and not for some nice theory. Taken in this way, Kuhn's conceptions and possibly others will serve historical thinking in two ways. First, new phenomena come into view, new developments and new causal connexions are seen. Secondly, the explicit application of a conceptual background makes discussible the usually completely implicit presuppositions on which the historiography of mathematics is founded. Thus it is useful to have different systems that can shed light on history and historiography of mathematics from many different angles.

The concepts developed thus far must be completed in different ways. Of these the role of extramathematical social influences is discussed in the next section. The final section will show what I have called the explanatory power of the concepts by discussing the regularities of the history of mathematics which have been stated by Crowe in his "laws".

3. A Necessary Supplement: The External Social Background

One of the weaknesses of Kuhn's conceptions correlating with the strong idealization of the scientific community is the lack of consideration of extradisciplinary influences. I have already pointed to the interaction of the mathematical disciplines, and there is much to be said about the relations of mathematics to nonmathematical factors ranging from astronomy to the general material conditions of society.

I shall confine myself to the imbedding of the mathematical community in society. The social position of the mathematician and his community is dependent on the structure of society and its development. Consequently, the development of mathematics is not independent from the fact that the mathematicians are amateurs, philosophers, civil servants, academicians or university teachers. By the way, in my view the social status of the mathematicians in society is a proper part of the history of mathematics.

As Ben-David argues [1971, 6-16] there might be no systematic influence of society on the contents of the sciences; but, as his book amply illustrates, the social structure of society is a precondition for the establishment of a scientific role that is the basis of a scientific tradition. The specialization of Ben-David's lines of inquiry to the mathematician is a worthy subject for study.

As to the social status of the members of the scientific community and its influence on the development of mathematics, I I point to two examples. The first is given by J. Needham [1956, 1964]. By a comparison of the social structures and the scientific (and mathematical) traditions of China and Western Europe in the scientific renaissance, he shows that the structure of society has been a factor in the rise of the New Science that fused mathematics and nature-knowledge. He argues that the fact that the Chinese scholars were part of the bureaucratic system was unfavorable to such a development, while it was favoured by the status of the men of knowledge in the mercantile society of the West. It should be added that Needham's papers do not try to prove that connection but rather give tentative arguments. Still they are a convincing contribution to the debate on the causes of the scientific renaissance, and they apply to mathematics as well as to the natural sciences.

My second example alludes to Crowe's "law" 4, which states that rigor in mathematical theories is frequently acquired only late in the historical development. The given evidence points mainly to the facts that fruitfulness is a higher value than rigor and that the 19th and 20th century brought about higher standards of rigor. I shall try to give tentative explanation of the latter fact. The mathematical community of the 18th century consisted of scholars and practitioners. The two groups were already quite separated, but connexions still existed. The ways to ensure material security were varied and often difficult [cf. Duveen/Hahn 1957]. The institutional basis of the community were the academies and scientific societies The academies institutionally joined mathematics and its fields of applications. Furthermore the political status of the academies gave them the twofold task of the quest for truth and the production of useful knowledge. Thus the mathematicians were concerned with "applied" mathematics, too, so that, e.g. the results of higher calculus had a ready and strong justification although they rested on a doubtful basis.

By the beginning of the 19th century a new social class had become dominant. In consequence of this change the system of education was modified and strengthened. The French revolution led to the establishment of the *Ecole Polytechnique* and the *Ecole Normale* which were of enormous significance in the history of mathematics. France was the center of mathematics and science at the turn of the century, it was the model for scholars in other countries. The German university reform, starting with the foundation of the university of Berlin 1809, merged traces of the French model with specifically German philosophical ideas and, most important, was an attempt to establish the autonomy of scholars without violating the boundaries set by the political state of the German countries. The scientists did not take part in the reform; still it turned out to be very favourable to the development of mathematics and natural sciences. The main point is that the teaching was done to students who could stay at the university to become professors of mathematics. The institutional background was provided by the *Institute* and *Seminare* that were founded during the century. Thus the mathematicians could teach the mathematics they were themselves working on.

These developments influenced the relation of pure and applied mathematics in many ways. First, the connection of mathematics to technology was cut off by the separation of

Technische Hochschulen and universities. Thus, e.g., descriptive geometry disappeared from the university curriculum. Secondly, the model of the foundations of *Seminare* was the *mathematisch-physikalisches Seminar* of Königsberg; its fourth follower, the important *Seminar* of Berlin was already purely mathematical. The institutional structure tended to separate fields, and thus pure mathematics was favoured. Thirdly, the teaching of students as potential mathematicians had two effects On one hand the mathematicians could teach things they were interested in, and the teaching got swiftly up to the level of actual research, thus forming a strong tradition. On the other hand, the mathematicians turned their professional interest to the things they were teaching, and thus became more concerned about the elementary parts of their disciplines.

In summary, the bonds to the fields of application were loosened, there was a firm social status for the mathematician, and the main task was the recruitment of the next generation of mathematicians. The first generation of university mathematicians was still concerned with applied mathematics, too; the later generations turned more and more to pure mathematics. That means mathematics turned to itself. This fact together with the effects and the needs of the new style of university teaching turned rigor into a value of high priority.

This rough sketch of an explanation of the development of rigor in the mathematics of the 19th century can be completed by evidence from many details. For some of these cf. Ben-David [1971, Ch. 7] and Lorey [1916]. My explanation gives only part of the truth. There are intramathematical factors as well, which are not discussed here.

4. The Explanation of Crowe's "Laws"

In his "laws" M. J. Crowe [1975] has described some regularities in the history of mathematics. He did not try to explain these regularities. Many of them can be explained in terms of the concepts developed in the preceding sections of this paper. I shall do this to show how the concepts can be applied and, more generally, to show the importance of the sociology of the mathematical community in the history of mathematics. Furthermore the mere statement of such regularities is not of much worth to historiography, there has to be some theoretical framing.

Some of the "laws" have already been discussed. The statement that "revolutions never occur in mathematics" (law 10) has to be judged according to if and how one uses the term "revolution". There is nothing to explain about it. The same applies to the term "crisis". Crowe states that mathematicians possess techniques to handle logical contradictions and thereby prevent crises (law 9). Such techniques belong to the disciplinary knowledge. A closer analysis of the "law" throws up thorny philosophical questions on mathematics that are not at stake here Another statement on the disciplinary knowledge is that "the 'knowledge' possessed by mathematicians concerning mathematics at any point in time is multilayered" and that there exists a "metaphysics" to mathematics (law 5). This has been discussed and specified at due length in the sections *2.2.8.* and *2.2.7.* on the disciplinary matrix and its elements.

A group of "laws" may be treated in connection with Crowe's own main example, the history of vector analysis. "New mathematical concepts frequently come forth ... against the efforts of the mathematicians who create them" (law 1). This formulation might be elegant, but it is misleading. Take Hamilton's invention of quaternions. Hamilton certainly did not struggle against quaternions and quaternions did not come forth by themselves. Hamilton's work was guided by the exemplar of complex numbers and one of the elements of the disciplinary matrix of his time said that multiplication was a commutative operation. Only after a long time of strenuous work did Hamilton abandon commutativity and find quaternions [Crowe 1967]. The problem Hamilton attacked was normal mathematics [*ibid.*, 12]. But it could not be solved in a normal way, it grew into an anomaly. For a man like Hamilton who stubbornly stuck to the problem for many years it is highly probable that in course of time he would try lines of thought that deviate more and more from the usual ways. Thus to find a solution like quaternions presupposes much time and effort, necessary for dissociating from the restrictions of the accepted beliefs and concepts. This, I think, is what Crowe calls the struggle against new concepts.

"Many new mathematical concepts ... meet forceful resistance after their appearance and achieve acceptance only after an extended period of time" (law 2). The mathematical community, like the creator of a new concept, only reluctantly abandons some of its accepted beliefs and concepts. This could be seen functionally. A light-handed use of new concepts that break with many implicit restrictions and beliefs would endanger the very basis of the communication of the community. Furthermore most mathematicians are concerned solely with normal mathematics and take no pains to understand and appreciate new and peculiar concepts and theories. Thus it takes a long time until an unusual concept like that of quaternions is accepted, and sometimes inventions are overlooked completely.

"The fame of the creator of a new mathematical concept has a powerful ... role in the acceptance of that ... concept, at least if the new concept breaks with traditions" (law 6). Reputation has been considered as functional in many ways by sociologists of science. The main point is, that reputation ensures disciplinary competence of a member of the mathematical community. As to the cited "law", this plainly means that for simple reasons of economy the members of the mathematical community are more willing to spend time on the non-normal work of a famous mathematician than on that of an outsider. This is even more so when, as in the case of Hamilton and Grassman, the work of the outsider even looks strange and outsiderish.

"New mathematical creations frequently arise within ... contexts far larger than the preserved contents of these creations; ..." (law 7). A new concept or theory as it is worked out by an outsider or a mathematician who has in a long course of work drifted away from the normal paths and has connected different ideas in unusual ways will very probably be framed in a peculiar, personal way. Furthermore, the mathematician who breaks with traditions and violates accepted beliefs tends to put something else in its place. He will either interpret his creations in a way to make them as compatible as possible with the contemporary disciplinary matrix, or he will draw up--as justification--a philosophy of his own, which helps him to keep his professional identity, and which will be closely connected with the new concept or theory. The same process will--in other forms--take place collectively in the process of acceptance by the mathematical community. Many of the peculiarities will be done away with, but some piece of what is not the "content" (again, what is it?) may become a common possession.

"Multiple independent discoveries of mathematical concepts are the rule, not the exception." (law 8). Crowe points to his "laws" 2 and 7 for a partial explanation. I can understand this only in the sense that the extended period of acceptance of new concepts gives room for independent discoveries, but this is a minor point as to the explanation of multiples. I do not pretend to be able to come forth with a general explanation for multiples in the history of mathematics; each one is different and each is multifactored. But the fact that multiple discoveries are frequent is a strong point in arguing that the interaction of the mathematicians in their community is the vital basis of the development of mathematics. The fact that discoveries are "in the air" can only be rationally explained by the contemporary disciplinary matrix, the combination of certain elements of which, like exemplars, concepts, problems, values, plus the existence of anomalies and, maybe, extramathematical influences make possible the rise of certain new concepts. Again the history of vector analysis is an example. Crowe speaks of a "trend" or a "movement" [1967, 48, 248] which evolved from different traditions. A comparative study considering the relation of each attempt to the contemporary background ought to throw more light on the causes of this "trend". To end this discussion of the "laws" connected with the history of vector analysis I should like to add that Crowe's book is--in the light of the viewpoints held in this paper--an excellent piece of history of mathematics.

"Although the demands of logic, consistency, and rigor have at times urged the rejection of some concepts now accepted, the usefulness of these concepts has repeatedly forced mathematicians to accept and to tolerate them, ..." (law 3). This regularity in the history of mathematics should be explained in terms of the basic beliefs and values of the mathematical community. I have argued above that fruitfulness is a value of higher priority than rigor. Furthermore, there is the generally implicit belief that mathematics is concerned with the solution of problems. Even if at times it might seem that the mathematical community, or part of it, is aiming at the construction of nice theories, in the case of Crowe's example, the imaginary numbers, mathematics was aimed at problem-solutions and those impossible numbers were an important solution. Questions of foundations and of ontology are left to the philosphers or to philosophically minded mathematicians, who are tolerated as the adressee for uncomfortable questions that do not concern the true mathematics. What the true mathematics is varies through history and can be seen only as a common consent of the mathematical community that cannot be sharply delimited.

I have in section 3 given an interpretative idea about the emergence of rigor, to which Crowe's "law" 4 alludes. Crowe states that the rigor of textbook presentation is frequently a late acquisition, rather forced upon than sought by the pio-, neers of the field. It may be added in explanation that before the textbook presentation of a subject there frequently is a period of normal mathematics in the field. In this period attention is given to the details and especially to those points which do not accord with the standards of the mathematical community, and thus rigor is forced upon the presentation of the subject.

NOTES

1. Foundation crises have been treated by J. Thiel [1972]. He starts with a basically social definition of "crisis", seems to forget about it, and ends up in a systematic, philosophical discussion. The book is historically unsatisfying. S. Bochner, in a paper [1963] on Kuhn's book, even holds the mathematical foundation crises to be revolutions, without giving much evidence to the point. The considerable main point of his paper is the role of the mathematical paradigm in physics.

2. J. Höppner and myself have in a course on the foundation crises of mathematics (Hamburg, 1973) tried to apply the concepts of Kuhn and Thiel. The discussion showed both to be inadequate as generalization of the historical facts as well as a guideline for historical inquiry.

3. In an exploratory study of the social features of mathematical problem solving Ch. S. Fisher [1972/3] has given an

interesting description of the contemporary mathematical community which he describes as quite diffuse.

4. This example comes from my research on the prehistory of lattice theory.

5. H. J. M. Bos remarked on this statement that it shows the intrinsic difference between science and mathematics: science uses symbolic generalizations *of something*, mathematics studies symbolic generalizations themselves--they are the concepts.

6. Possible doubts as to the correctness of this story in the case of Kummer do not weaken the argument. Cf. Edwards, H. M "The Background of Kummer's Proof of Fermat's Last Theorem for Regular Primes." *Archive for History of Exact Sciences* 14(1975), 219-236.

REFERENCES

Ben-David, Joseph 1971 *The Scientist's Role in Society* Englewood Cliffs (Prentice-Hall)

Bochner, Salomon 1963 Revolutions in Physics and Crises in Mathematics *Science* 141, 408-411

Crowe, Michael J. 1967 *A History of Vector Analysis* (University of Notre Dame Press)

______ 1975 Ten "Laws" Concerning Patterns of Change in the History of Mathematics *HM* 2, 161-166

Dedekind, Richard 1930/32 *Gesammelte mathematische Werke* 3 vols Braunschweig (Vieweg)

Duveen, Denis J. & Roger Hahn 1957 Laplace's Succession to Bézout's Post as *Examinateur des Elèves de L'artillery* *Isis* 48, 416-427

Fisher, Charles S. 1966 The Death of a Mathematical Theory: A Study in the Sociology of Knowledge *Archive for History of Exact Sciences* 3, 137-159

______ 1967 The Last Invariant Theorists *Archives Européennes de sociologie* 8, 216-244

______ 1972/3 Some Social Characteristics of Mathematicians and their Work *American Journal of Sociology* 78, 1094-1118

Kuhn, Thomas S. 1970a *The Structure of Scientific Revolutions* 2nd ed., enlarged (University of Chicago Press)

______ 1970b Logic of Discovery or Psychology of Research in Lakatos/Musgrave 1970, 1-23

______ 1970c Reflections on my Critics *ibid.*, 231-278

Lakatos, Imre 1963/64 Proofs and Refutations *British Journal for the Philosophy of Science* 14, 1-25, 120-139, 221-243, 296-342

______ 1973 History of Science and its Rational Reconstructions *Boston Studies in the Philosophy of Science* 8, 91-136

Lakatos, Imre & Alan Musgrave 1970 *Criticism and the Growth of Knowledge* (Cambridge University Press)

Lorey, Wilhelm 1916 *Das Studium der Mathematik an den deutschen Universitäten seit Anfang des 19. Jahrhunderts* Leipzig u. Berlin (Teubner)
Lewis, Judith O. 1966 *The Evolution of the Logistic Thesis in Mathematical Logic* Ph.D. Dissertation (Univ. of Michigan)
Merton, Robert K. 1968 Science and democratic social structure in *Social Theory and Social Structure* enlarged ed. New York (The Free Press) 604-615
Meyer, Karl 1974 Das Kuhnsche Modell wissenschaftlicher Revolutionen und die Planetentheorie des Copernicus *Sudhoffs Archiv* 58, 25-45
Needham, Joseph 1956 Mathematics and Science in China and the West *Science and Society* 20, 320-343
______ 1964 Science and Society in East and West *Centaurus* 10, 174-197
Nový, Luboš 1973 *Origins of Modern Algebra* Prague (Academia)
Schröder, Ernst 1877 *Der Operationskreis des Logikkalkuls* Leipzig (Teubner)
______ 1890 *Vorlesungen über die Algebra der Logik* vol. I Leipzig (Teubner)
Struik, Dirk J. 1948 *A Concise History of Mathematics* 2nd ed. New York (Dover)
Toulmin, Stephen 1970 Does the Distinction between Normal and Revolutionary Science Hold Water? in Lakatos/Musgrave 1970 39-47
Thiel, Christian 1972 *Grundlagenkrise und Grundlagenstreit* Meisenheim (Anton Hain)
Wilder, Raymond L. 1968 *Evolution of Mathematical Concepts* New York (Wiley)
______ 1974 Hereditary Stress as a Cultural Force in Mathematics *HM* 1, 29-46
Wussing, Hans 1970 Zur Entwicklungsgeschichte naturwissenschaftlicher Begriffe *NTM* 7, Heft 2, 15-29

* * * * * * * * * * * * *