Kuhn’s account of the development of science, together with its associated concepts of paradigm, normal science an scientific revolutions, has been the focus of intensive debate ever since its first publication in 1962. As a subsidiary issue in this debate the question has been raised as to the applicability of Kuhn’s theory or of Kuhn’s concepts to the history of individual scientific disciplines, and naturally the question has also been addressed by historians of mathematics concerning their own field of research. The debate concerning revolutions in the history of mathematics was opened by Michael Crowe (1975), who claimed that there have been no revolutions in the history of mathematics. Crowe was soon followed by Herbert Mehrtens (1976) who claimed the opposite. In a recently published volume (Gillies (ed.) 1992) an attempt has been made to assess the present state of that debate. This volume gathers contributions of twelve authors, including the original articles of Crowe and of Mehrtens, as well as both authors’ respective present views on the issue. Thus, about thirty years after Kuhn’s book on paradigms and scientific revolutions first appeared in print, and about twenty years after a debate on related issues was opened for the case of the history of mathematics, it makes sense to attempt to evaluate, from a critical perspective, the specific contribution of this debate to the historiography of mathematics.

Kuhn’s contribution to the professional discourse of historians of science has centered around the concepts of "scientific revolution" and "paradigm". Any critical evaluation of Kuhn's contribution must obviously consider these two concepts. The present article attempts to evaluate whether or not by adopting these, and other concepts introduced by Kuhn the historian and the philosopher of mathematics are bound to attain new insights that they would have otherwise overlooked. But before entering into a more detailed analysis, it is first necessary to begin with two general remarks concerning the use of Kuhnian concepts in interpreting the history of mathematics.

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1. The issues discussed here were already considered in two published, somewhat overlapping articles Corry 1993 & Corry 1996a. The present version combines selected sections taken from both of them, while avoiding repetitions. Eventual quotations should refer to one of the former articles.

2. See the extensive bibliography appearing in G. Gutting (ed.), 1980.
Firstly, when attempting to assess the actual influence of Kuhn’s concepts and theories on the actual writing of history of science one is confronted with a somewhat ambiguous situation. On the one hand, Kuhnian ideas have been adopted and are used (either explicitly or implicitly, either intendedly or unintendedly) in a considerable number of contemporary historical works on science. On the other hand, however, in spite of the intensive discussion aroused by Kuhn’s book among some philosophers and historians of science, no meaningful tradition of actual historical research and writing developed over the last thirty years, directly attempting to apply a Kuhnian approach. Remarkably enough, the very authors that contributed to the volume edited by Donald Gillies, to take but one, significant, example, have not in general adopted the Kuhnian approach in their respective academic work, beyond their specific articles appearing in the mentioned volume.

The pervasiveness of Kuhnian terminology in historical and philosophical debates and the simultaneous absence of an elaborate tradition of distinct Kuhnian historiography, a fact in itself seldom mentioned when discussing Kuhn’s contribution, should certainly be taken into account in any assessment of the value of Kuhn’s concepts, and of any related theories, for the working historian or philosopher of mathematics, or of science in general.

Secondly, it is also remarkable that most attempts to evaluate the applicability of Kuhn’s approach to the history of mathematics (and it is again illuminating to consider the examples appearing in the Gillies volume) are often preceded by a preliminary discussion concerning the question, what is the most appropriate definition of "scientific revolution”; having chosen one such definition, authors then proceed to prove (each according to his own view) either that there have been or that there have not been revolutions in mathematics. The reader thus often get the feeling that those authors painted the bull’s-eye after the arrow had been shot. Such preliminary discussions remain at the semantic level, rather than throwing new light upon the historical development of mathematics in general or upon particular episodes of it. Naturally the semantic freedom of authors to choose their own meaning for the term "scientific revolution", "paradigm" or "normal science", in order to find out a possible connection between their definition and the facts of history as they see them, should be respected. Nevertheless, one can still examine the various

3. This situation has been recently described as follows (Renn 1993, 312): "Whether the development of scientific concepts is a continuous or a discontinuous process is at present no longer truly controversial. Whatever historians and philosophers of science may think about the individual building blocks of Thomas Kuhn’s intellectual edifice, they more or less agree with him that the development of science is organized as a number of well-distinguished 'floors'”. It should be remarked, however, that Renn goes on here to point out the difficulties left unsolved by the adoption of this approach, and to advance his own proposal for overcoming them.
definitions critically, and evaluate their degree of interest and of fruitfulness as analytical tools for
the research of the history of science.

In (Corry 1993) I proposed a model for re-assessing the many possible interpretations of
Kuhn’s theory, and for evaluating their relative interest and fruitfulness in describing the historical
development of science. Based on that model I discussed the applicability and the expected con-
tribution of the relatively more interesting versions of Kuhn’s theory, and especially of the con-
cept of scientific revolution, to the particular case of the history of mathematics. The first two
sections of the present article repeat what was said in those of (Corry 1993). They are meant to
introduce again the model. There follows a discussion of the question, whether, and to what
extent, does the introduction of the concept of "paradigms" advance or distort our understanding
of the history of mathematics.

I. THE KUHNIAN AGENDA

Debates concerning Kuhn’s theory have often addressed two separate issues: the actual meaning
of Kuhn’s theory and its related concepts, on the one hand, and their applicability to the history of
science in general and of particular scientific disciplines, on the other. A typical difficulty
encountered while attempting to elucidate the meaning of the theory lies in the many changes that
affected Kuhn’s own views over his successive writings. The controversial character of Kuhn's
book stemmed from the fact that on most, if not on all issues involved, Kuhn originally adopted a
most stringent and uncompromising position. Later on, in the attempts to further explain his posi-
tion, and perhaps under the pressure of acute criticism, Kuhn appeared increasingly willing to
hold weakened versions of his initial views. His conceptions thus became more acceptable, but,
alas, much less bold and hence much less interesting.

4. This is particularly the case of Crowe and Mehrtens themselves. Cf. Crowe 1967; Mehrtens 1979. See
also Crowe 1990; although he uses the standard term “Copernican Revolution” in the title of his book, he does
not discuss what a scientific revolution in general is, and certainly he does not relate the term to, or otherwise
mention, any of the other Kuhnian terms: paradigms, normal science, etc. A more interesting example comes
to the fore in the writing of Jeremy Gray. Gray contributed to the Gillies volume with a discussion on the
history of algebraic number theory as a possible instance of a scientific revolution. Prior to that, Gray had writ-
ten a remarkable book on the rise of non-Euclidean geometry (Gray 1989). The latter, although dealing with
the most oft-quoted example of a putative revolution in the history of mathematics, neither discusses its sub-
ject-matter in terms of scientific revolutions nor adopts any of the Kuhnian concepts for that matter. Inciden-
tially, however, in a less historical and more speculative section of his book, Gray expresses his opinions
concerning methodological aspects of the scientific enquiry; there he discusses the differences between "com-
mon experiments", intended to measure some specific magnitudes in the framework of an accepted theory, and
"crucial experiments", intended to decide between two competing theories. In this short discussion Gray uses a
distinct Kuhnian wording to explain, that, whereas experiments of the former kind are not intended "even in
principle to refute the theory", the latter kind are "few and far between" (Gray 1989, 187-189). Thus, Gray's
remarks would seem to corroborate Jürgen Renn’s claim quoted in note 3 above.
Yet beyond one’s own evaluation of the merits of Kuhn’s actual views, there is at least one undeniable virtue that must be conceded to his work: that of having brought about the widespread adoption of a new agenda for debate in the history and philosophy of science (as well as in many other intellectual areas). Not that all the issues contemplated in this agenda were Kuhn’s direct creation; several of them had been separately discussed before Kuhn, by other authors trying to advance a non-positivistic view of science. Yet no other single book was so instrumental like *The Structure of Scientific Revolutions* in making this agenda of central concern for historians and philosophers of science. In what follows I will refer to this agenda as the "Kuhnian agenda". The latter should be clearly distinguished from (the various versions of) Kuhn’s own agenda, or Kuhn’s own theory.

The main issues considered in the Kuhnian agenda may be articulated around four axes:

1. Normal change vs. Revolutionary change.
2. Paradigms.
3. Rationality of Science.
4. The Scientific Community.

Kuhn himself, as well as his followers and critics, often addressed the issues belonging to the different axes without clearly separating them; this has been the source of a second typical difficulty in discussing Kuhn’s theory and its applicability. In order to overcome this difficulty, I will provide an exhaustive list of the issues that arise within each of the above mentioned axes; I will claim that these issues are logically independent, i.e., that a stand may be adopted on each single issue independently of the stand adopted on any of the others. Combining different possible stands on the various issues of the Kuhnian agenda yields several different Kuhnian accounts of the history of science.

The issues at stake in each of the above-mentioned axes admit extreme, uncompromising formulations. The different Kuhnian versions arise when those formulations are adopted with varying degrees of commitment. Kuhn’s own successive versions of the theory indicate how these varying degrees of commitment translate into varying degrees of boldness, on the one hand, and of acceptability, on the other hand, of the Kuhnian account of the history of science. It is not, however, a critical account of the development of Kuhn’s own ideas which interests here, but rather an appraisal of the perspectives opened by the Kuhnian agenda. Thus, the relative degree of interest of a particular version of the Kuhnian account will be determined by its degree of commitment to each of the proposed formulations; the more committed an account is, the farthest it is
from the positivistic conception of science and, therefore, the boldest and most interesting. Obviously, a completely different question is whether some version of Kuhn’s view is also correct.

I.1. Normal vs. Revolutionary Change

This is the central axis of the Kuhnian agenda. Kuhn’s original version of the theory was meant to ascertain the existence of revolutionary change, and to explain how and when it comes by. It connected revolutions to paradigms, it allowed non-rational factors into the account of the development of science, and assigned a cognitive role to the scientific community. At least, many understood Kuhn’s theory as doing so. One can, however, consider each of those issues separately.

Normal change is change by linear accumulation, with minimal, if any rejection of existing knowledge. The main activity of scientists engaged in normal research is "puzzle-solving", namely, attempts to connect a given problem with the existing corpus of accepted scientific knowledge. Difficulties encountered in those attempts challenge the scientist’s individual ingenuity; they do not question the validity of the theory as such.

In revolutionary change passage from old to new is not accomplished by mere addition to what was known. The corpus of accepted knowledge after revolutionary change is incompatible with that accepted before revolution; this means that not both of them may be held simultaneously as true. Therefore, essential components of previously accepted knowledge must be rejected when revolutionary change takes place. In his first version, Kuhn went so far as to claim that pre- and post-revolutionary theories are incommensurable.

The boldest position concerning normal and revolutionary change is formulated with the following claims:

1.1 There exist such things as purely normal and purely revolutionary change in science.
1.2 The history of science consists of long periods of normal science sporadically interrupted by revolutionary change.
1.3 The language used in a field of science changes so radically during revolutions in that field, that the old language and the new language are not intertranslatable.⁵
1.4 A scientific revolution is "a relatively sudden and unstructured event like a Gestalt switch."⁶ It is a holistic process, which cannot "be made piece-meal, one step at a time."⁷

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⁵ According to Philip Kitcher (1979, 520) this formulation describes Kuhn’s own view.
1.5 The distinction between revolutionary and normal change is not tantamount to that between "major breakthrough" and "minor discovery".

Claim (1.5) does not explicitly appear in Kuhn’s writings; I have introduced it here, however, to stress the fact that the term "revolutionary change" should not mean to imply a value judgement. If there was nothing in the Kuhnian agenda beyond labels for good and bad science, it would offer no new perspectives for historians and philosophers of science. In order for the definition of revolutionary science to be interesting, and in fact meaningful, it must allow for the existence of major breakthroughs as part of normal change.

I.2. Paradigms

Although often considered as the main contribution of Kuhn, enabling historians and philosophers of science to appreciate theretofore neglected aspects of their object of research, the concept of paradigm turned out in fact to be the most problematic one of the Kuhnian agenda. This vague term (and its several synonyms) was object of harsh criticisms and several reformulations. It has been used to denote whole theories, exemplary instances of scientific achievement in the past, models, metaphysical conceptions, choice of problems and techniques for analyzing them, and many other things.

But as already stated, we are interested, not in attempting to formulate a correct definition of this or of other central concepts of the Kuhnian agenda, but rather in assessing the possible contribution of those terms to the historian’s actual research. In discussing paradigms, one should stress, first of all, that if "paradigm" is taken as synonymous with "theory", then there is no need to discuss the former concept separately. Paradigms become meaningful only when considered as either single ideas or whole conceptual systems, different from particular scientific theories, which however must be referred to when explaining the development of science. Definitions of paradigms may allow either their conscious adoption by scientists, or only their discovery by the historian in hindsight. Yet in any case paradigms should have their own peculiar behavior in history, which is different than (and, of preference, have conceptual priority over) that of theories. Otherwise paradigms are redundant as an analytical tool for the historian and the philosopher of

8. The best known of which is, obviously, Masterman 1970.
science. Within this framework, several more elaborate definitions of paradigm may be formulated. Regarding any such definition the uncompromising Kuhnian version holds that:

2.1 Paradigms, not theories (and, of course, not individual discoveries), are the basic units of scientific achievement and change.

2.2 A scientist cannot, while under the sway of one paradigm, seriously entertain a rival paradigm. 9

Kuhn’s account has been often criticized as involving a vicious circle, since it allegedly defines a scientific community as any group sharing a paradigm, and a paradigm as that thing shared by a scientific community. Be that as it may in Kuhn’s writings, the two issues are separate in the Kuhnian agenda. One can claim a role for paradigm, more or less committed to (2.1) and (2.2), without yet accounting for how a paradigm is chosen. In particular, one can claim that the community either plays or does not play a role in determining that decision. 10

I.3. Rationality of Science

Positivistic accounts of science take the rationality of science for granted. Non-positivistic accounts face the issue of rationality. Popper addressed this issue by claiming that the rationality of science lies in its commitment to critical debate.

In Kuhn’s writings, the issue of rationality arises in connection with paradigms and with theory-choice; one of the sources of controversy aroused by Kuhn’s early version concerned the claim, apparently implied by it, that the acceptance of a new paradigm is not simply a matter of applying rules. But this issue, as the former two, is in fact a separate one that can be considered on its own. The issue of rationality manifests itself in the Kuhnian agenda as varying degrees of commitment to the following two claims:

3.1 Rational discussion is possible only after agreeing on fundamentals, and this agreement is just a matter of convention. 11

9. This formulation is taken from Watkins 1970, 34.

10. It is worth pointing out that in characterizing scientific revolutions in 1988, Kuhn (1988, fn. 4) did not even mention the word ‘paradigm’. In doing so, he finally dropped this problematic issue from his own version of the Kuhnian agenda.

11. This has been one of the central targets of Popper’s criticism of Kuhn. Cf. Popper 1970, 56: "The relativistic thesis that the framework cannot be critically discussed can be rationally discussed ... The Myth of Framework is, in our time, the central bulwark of irrationality. ... It simply exaggerates a difficulty into an impossibility."
3.2 Many among the scientist’s choices do not obey universally valid rules; rather, these choices admit only sociological and psychological explanations.

I.4. The Scientific Community

The traditional task of the sociology of science since Merton had been to account for the social conditions under which a community of scientists could effectively develop. Sociologists had implicitly been accepting a "division of labor" with historians and philosophers of science. Sociologists would study the non-logical behavior of the scientific community, while historians and philosophers would study the contents and internal logic of the scientists’ ideas; the former undertaking was considered as irrelevant to the latter. Kuhn made a sensible contribution to the sociology of science by suggesting that there may indeed be a relevance. As on other issues, Kuhn originally adopted an extreme view on this, thus contributing to make his book controversial. Later on, once more under the pressure of criticism, he weakened his position.

The uncompromising view of the scientific community’s structure and behavior as an instrumental factor in shaping the contents and logic of science is expressed in the following three claims:

4.1 A distinctive sociological characterization of scientific communities can be given, which is exclusive, i.e., it is valid for no other kind of professional community.

4.2 The sociological study of this kind of community is epistemologically relevant to the understanding of the cognitive contents of science. In particular: the ultimate locus of science’s rational authority is the scientific community.

4.3 The scientific community which is relevant for epistemological analysis is that of the practitioners of an entire discipline (physics, biology, etc.), rather than of sub-disciplines (molecular biology, evolution, marine zoology, etc.).

Claim (4.1) does not explicitly appear in Kuhn’s writings, yet it is necessary in order to confer a clearer meaning to (4.2). Notice also that one can claim that the community influences the con-

12. This is illuminatingly described King 1980, 97-98.
13. The transformation of Kuhn’s views on this issue is explained (and lamented) in King 1980, 107-115.
14. This last formulation is taken from Gutting (ed) 1980, 11. Gutting believes this phrase to encompass the "real significance of Kuhn’s work."
15. Kuhn’s work was actually criticized as containing a circular argument on this point. It allegedly defined a scientific community as a group of practitioners sharing a paradigm, and a paradigm as that thing shared by a scientific community. See Musgrave 1970, 40.
tents of science through paradigms, but there are also many other possible ways in which this influence may proceed; thus, paradigms and the scientific community are two separate issues in the Kuhnian agenda.

In responding to his critics Kuhn claimed that his definition of scientific communities equally allow for big and for very small communities, including even "fewer than twenty-five persons".\(^{16}\) Obviously this is a weakened form of his initial claim; thus, for instance, one of the features of science as a cognitive system, which a social account of science is bound to explain, is the relatively high degree of agreement found, as a rule, among the members of the community. Naturally the smaller the community covered by this explanation, the weaker the power of the claim. This is the reason for including (4.3).

The Kuhnian agenda, then, provides four axes of reference in describing the history of science. An extreme version of Kuhn’s theory is obtained by adopting the above-introduced formulations with full commitment. Analyzing the history of science in such terms, were it to prove itself adequate, might perhaps lead the historian to many new insights. In all likeliness, however, most of the above claims will prove historically inadequate unless one relinquishes the extreme formulation in favor of a more moderate one. The more one has to abandon the extreme formulations, in order to make them fit historical facts, the less the interest offered by the theory as an analytical tool. Given a particular account of scientific change, that touches upon the Kuhnian issues, we may now assess its fruitfulness; we may, that is, assess whether by looking at the history of science through this account, meaningful insights are to be expected that would not be otherwise attained, or whether, on the contrary, that account offers no more than a system for labelling - important or routinely - the various stages in the development of the discipline in question.

The above analysis of Kuhn’s theory, then, allows a clearer understanding of both its positive and negative assertions, and consequently, the possibility of a more balanced evaluation of its actual contribution to historians and philosophers of science.

\(^{16}\) Kuhn 1974, 464.
II. Arguing for Revolutions in Mathematics - A Case Study

An articulated attempt to analyze the history of mathematics in Kuhnian terms was advanced by Joseph Dauben. In an article of 1984 Dauben provided his own definition of revolution in science, in order to claim that, under this definition, both the discovery of incommensurable quantities in ancient Greece, and Cantor’s theory of sets were examples of revolutions. To be sure, Dauben’s definition is not intended to be a reformulation, or an adaption of Kuhn’s definition; it is indeed likely that Dauben would reject any connection between his analysis and Kuhn’s theory. Yet, as the preceding section has suggested, the specific contribution of any attempt to talk of revolutionary, as opposed to normal change in science may be assessed by referring to the Kuhnian agenda.

Dauben’s definition of revolution relies on ideas formerly developed by I. Bernard Cohen in his own work. In order to elucidate the meaning of "revolution", Dauben suggests to follow Cohen’s examination of the meaning that the term has been traditionally given (in the political context), since the eighteenth century. Such an examination leads Dauben to pin down the following characteristic features of revolutions:

a. A radical change or departure from traditional or acceptable modes of thought.

b. A series of discontinuities of such magnitude as to constitute definite breaks with the past. After such episodes, one might say that there is no returning to an older order.

c. Revolutions have been those episodes of history in which the authority of an older, accepted system has been undermined and a new, better authority appears in its stead. (p. 83)

Under this definition, Dauben claims, revolutions have occurred in mathematics. Of course, the peculiar nature of mathematics is not overlooked by Dauben, who sees no reason "to expect that a purely logico-deductive discipline like mathematics should undergo the same sort of transformations, or revolutions, as the natural sciences," and in particular, that it should comply to the framework of "Professor Kuhn’s model of anomaly-crisis-revolution (p. 82)." Therefore some qualifications must be added when talking of revolutions in mathematics:

d. [In mathematics] it is not always the case that an older order is refuted or turned down.

17. Dauben 1984. Dauben’s article was originally intended, among others, as a reply to Crowe 1975.

18. In a symposium on "Structures in Mathematical Theories" held in San Sebastián, Spain, in September 1990, Dauben addressed once more the issue and provided three additional examples of revolutions: Leibniz’s and Newton’s invention of the calculus, Cauchy’s calculus, and Abraham Robinson’s non-standard analysis.

e. It is often clear that new ideas would never have been permitted within a strictly construed interpretation of the old mathematics, even if the new mathematics finds it possible to accommodate the old discoveries in a compatible or consistent fashion.

f. Often, many of the theorems and discoveries of the older mathematics are relegated to a significantly lesser position as a result of a conceptual revolution that brings an entirely new theory or mathematical discipline to the fore. (p. 84)

As was claimed above, it is not so important whether a definition of scientific revolution does or does not fit "Kuhn's model" (if there is such a thing); what counts is whether a particular definition leads to new insights in studying the history of mathematics. Thus, the Kuhnian agenda described above provides useful parameters for evaluating the relative interest of Dauben's particular definition.

Notice first of all, that Dauben's explicit definition does not deal at all with questions related to issues I.2, I.3, and I.4 mentioned above. Dauben's description of revolutions and his claim that they do appear in the history of mathematics imply accepting some version of (1.1) and (1.2): Dauben claims that in the history of mathematics there have been certain events of special significance, which may be clearly separated from day-to-day progress in mathematical research. In addition, Dauben's statement (e) could perhaps be taken as a weakened version of (1.3). Of course, Dauben's (d) weakens his commitment both to (1.3) and to the differentiation between normal and revolutionary science.

Beyond Dauben's explicit claims, one finds implicit reference to other issues of the Kuhnian agenda in his analysis of the two examples chosen as representative of revolutions in mathematics. For instance, among the main revolutionary influences of the discovery of incommensurable Dauben mentions the eventual admission of the irrational numbers, in the following terms:

The transformation in conceptualization from irrational magnitudes to irrational numbers represented a revolution of its own in the number concept, although this was not a transformation accomplished by the Greeks. Nor was it an upheaval of a few years, as are most political revolutions, but a basic, fundamental change. Even if the evolution was relatively slow, this does not alter the ultimate effect of the transformation. The old concept of number, although the word was retained, was gone, and in its place, numbers included irrationals as well. (pp. 88-89)

This quotation, as well as other similar passages in the article, illustrates the problematic aspects of Dauben's revolutions as a category for historical analysis. Dauben's uncommitted stand on (1.4)
raises questions like: What is so peculiar about this long episode in the history of mathematics, that justify its separation from other no less important ones? Which of the basic concepts of mathematics changed less significantly during the last two thousand years of mathematical history?20

Of all the issues of the Kuhnian agenda, Dauben’s definition touches only upon the separation of revolutionary from normal change in mathematics, but he does not explicitly provide the means to identify, from among other contributions to mathematics, those which are truly revolutionary. Moreover, the particular examples chosen by Dauben do not clarify these issues. Dauben claims that after the discovery of incommensurables, two things became unacceptable: "(1) the Pythagorean interpretation of ratio, and (2) the proofs they had given concerning commensurable magnitudes came into play (sic)." Moreover, he adds, "new proofs replaced old ones. (p. 88)"

Dauben justifies his claim by referring to proofs that appear in Book V of the Euclidean Elements, and which generalize proofs of Books II and VII. Thus Dauben’s claim does not mean, as it might appear at first sight, that old proofs were rejected as erroneous; rather, it just means that improved, more general versions of older proofs were advanced, and the former "replaced" the latter. Dauben recognizes that this is case, but, he claims:

To say that mathematics grows by successive accumulation of knowledge, rather than by the displacement of discredited theory by new theory, is not the same as to deny revolutionary advance. (p. 93)

But what, then, characterizes revolutionary advances? The answer would seem to be found in Dauben’s second example of revolutionary change. In fact, Dauben claims that Cantor’s work:

... did not displace, but it did augment the capacity of previous theory in a way that was revolutionary - that otherwise would have been impossible. It was revolutionary in breaking the bonds and limitations of earlier analysis, just as imaginary and complex numbers carried mathematics to new levels of generality and made solutions possible that otherwise would have been impossible to formulate. (p. 93. Italics in the original)

If this is what characterizes revolutionary, as opposed to normal change, then one may ask: Does the formulation of any new theory enabling wider perspectives, providing more general formulations or leading to unexpected solutions of hitherto unsolved problems constitute revolutionary

20. Incidentally, I. B. Cohen himself stressed the need to answer this kind of questions clearly, when defining scientific revolutions when 1988, 23): "One of the problems in discussing any revolutionary set of changes in scientific thought or practice is the somewhat subjective decision whether the extent of the time scale implies that the process of change was a revolution or an evolution."
change? If this is the case, one is lead to conclude that, for Dauben, "revolutionary change" is tantamount to "major breakthrough". Though, undoubtedly, evaluating the relative prominence of various contributions to the development of the discipline is among the important tasks of the historian of mathematics, one wonders whether such an undertaking necessitates, or even justifies the introduction of an additional, superfluous concept of "revolutions". In any case, the concept of revolution advanced by Dauben, neither takes any of the risks implied by some degree of commitment to the Kuhnian agenda, nor envisages enjoying from any of its expected gains.

There is in fact one single issue that, although not explicitly mentioned by Dauben, may be read between the lines of his argument and confers some degree of interest upon his analysis. It is connected to (4.3) (although without avowing any epistemological role for the scientific community). Dauben seems to imply that the two examples considered in his article influenced the subsequent development of the whole discipline of mathematics, rather than of the more limited, specific context within which they arose. Thus, in his discussion on the discovery of incommensurability Dauben claims that:

Wholly apart from the slower, more subtle transformation of the number concept, however, was the dramatic, much quicker transformation of the character of Greek mathematics itself ... Greek mathematics was directly transformed into something more powerful, more general, more complete. (p. 88)

Although the last sentence implies, once more, that in Dauben’s account the term "revolutionary change" appears, above all, as a value judgment, his assessment also includes the independent claim of the overall influence of the discovery. Likewise, regarding Cantor’s theory, Dauben points out as its revolutionary symptoms:

... the extensive revision due to transfinite set theory of large parts of mathematics, involving the rewriting of textbooks and precipitating debates over foundations. (p. 93)

"Overall influence on the discipline" - although every development that fits this criterion is perhaps also a "major breakthrough", the opposite is certainly not the case. There are many major breakthroughs whose influence remain confined to a context more restricted than the whole of the discipline. This would seem to be the only acceptable and meaningful criterion, arising from

21. The three additional examples of revolutions in mathematics advanced by Dauben in his San Sebastián talk (note 18, above) give further credence to my conclusion here.
Dauben’s account, for considering an instance of change as revolutionary, without having to include under it every instance of "remarkable breakthroughs". Of course, the question remains open, whether Dauben’s two example conform to this criterion, but that is a different question which will not be discussed here. 22

III. BODY OF MATHEMATICS AND IMAGES OF MATHEMATICS

Can one nevertheless find a bold and meaningful definition of "revolution" (that is, one that takes in account as many issues of the Kuhnian agenda as possible), allowing for revolutions in mathematics, and leading to new, meaningful insights? In order to discuss this question it is first necessary to start with some general considerations on the history of mathematics.

Scientific disciplines give rise to two, more or less discernible sorts of questions. The first sort includes questions about the subject matter of the discipline. The second sort comprises questions about the discipline qua discipline, or meta-questions. To answer questions of the first sort is always among the aims of any given discipline and, obviously, practitioners of that discipline are usually engaged in such an activity. Concerning the questions of the second sort, however, whereas one may certainly find some scientists consciously attempting to answer them, one may also find some other scientist only implicitly or tacitly answering them, and still others ignoring the existence of those kinds of questions altogether. One may even find scientists deliberately avoiding to deal with them.

There are some statements which can easily be classified as being answers to either of the two above mentioned sort of questions. For other statements, however, it is harder to establish whether they are answers to questions about the subject matter, or about the discipline qua discipline. Each of Newton’s three laws, for instance, clearly belongs to the first category; all three are statements about how bodies move. The claim that Copernicus’s system is "simpler" than Ptolemy’s, clearly belongs to the second one: it is a claim about astronomical theories rather than a claim about the heavenly bodies. Gödel's theorems are deep results within a specific branch of mathematics, but they may also be taken as claims about mathematics, the discipline.

22. From the other three examples advanced by Dauben, non-standard analysis certainly does not fit the criterion.
Two layers can therefore be tentatively identified as relating to any scientific discipline; they can be described schematically as the "body of knowledge" and the "images of knowledge". The body of knowledge includes statements that are answers to questions related to the subject matter of the given discipline, while the images of knowledge include claims which express knowledge about the discipline qua discipline. This division is not always sharp and, to be sure, it is historically determined: the classification of a statement as belonging to one or to the other layer may change through time.

The body of knowledge includes theories, 'facts', methods and open problems. The images of knowledge serve as guiding principles, or selectors; they pose and resolve questions which arise from the body of knowledge, but which are not part of, and cannot be settled within, the body of knowledge itself. The images of knowledge help to resolve questions such as the following: Which of the open problems of the discipline most urgently demands attention? How should we decide between competing theories? What is to be considered a relevant experiment? What procedures, individuals or institutions have authority to adjudicate disagreements within the discipline? What is to be taken as the legitimate methodology?

All these are meta-questions; they consider diverse aspects of the discipline qua discipline. It is evident that the answers to these questions depend upon the contents of the body of knowledge at a given stage of development of the discipline and that, moreover, changes in the body of knowledge may alter these answers. And yet these answers are not exclusively determined by the body of knowledge; they may be influenced by other, external, factors as well. Thus, faced with one and the same body of knowledge, two different scientists could hold different images of knowledge. In turn, the images of knowledge play a decisive role in directing research and further determining the development of the body of knowledge.

The study of the interaction between these two layers may provide a framework of historical research within which to examine and understand the development of particular scientific discipline or scientific ideas. This framework allows taking into account how historically determined factors (institutional, philosophical, social, etc.) indirectly influence the realm of pure ideas (through the images of knowledge), but at the same time avoids the need to subscribe to dubious, "strong" explanations, which overemphasize the effects of these factors and necessarily imply a relativistic view of science.\footnote{The centrality of meta-issues for the history of science and the terminology "body of knowledge" and "images of knowledge" have been taken here from the work of Yehuda Elkana. These concepts arose in the framework of an ambitious program aimed at an anthropologic characterization of scientific knowledge as a cultural system (Cf. Elkana 1981; 1986). I have adopted them, however, with much more limited aims in mind.} This schematic separation of scientific knowledge into two layers
may provide, in conjunction with the analysis of their interaction, a useful perspective for the study of the history of mathematics as well, and in particular it may help us understand the issue of revolutions and paradigms in mathematics.

The distinction between body and images of knowledge should not be confused with a second distinction that has been often admitted, either explicitly or implicitly, by historians of mathematics, namely, the distinction between "mathematical content" and "mathematical form". Such a distinction has been criticized on grounds that a clear separation between mathematical form and content is impracticable, historically unilluminating, and indeed misleading. The separation of body and images of knowledge, however, is of a different kind. For one, the borderline between the two layers is admittedly blurred and historically conditioned. Moreover, both the body and the images of knowledge are equally important components of scientific knowledge. Where they differ is in the domain of their questions: while the former answers questions dealing with the subject matter of the discipline, the latter answers questions about the discipline qua discipline. Inasmuch as these two domains may be separated, so can the body and the images of knowledge be separated. Thus rather than a separation between form and content this is a separation between a text and its context.

Let us now elaborate further the way in which the body/images of mathematical knowledge may be used in more concrete historical analysis. The discovery of a new theorem, proof or concept represents the most basic form of change in mathematics, although not always the most historically significant one. It is the images of knowledge that determine, in mathematics as in the other sciences, the way in which a new item will be integrated into the existing picture of knowledge; whether it will be considered important or whether it will be ignored. Eventual changes in the images of knowledge may later transform the status of existing pieces of knowledge and produce a different overall picture of a particular scientific discipline. Changes in the images of mathematics may determine that a given theory or approach, formerly considered as irrelevant or uninteresting, will eventually turn into a lively field of research and into an expected source of important results. Thus one can focus on change which proceeds not only quantitatively, by addition of new results or concepts. These additions are, of course, fundamental to the growth of mathematics, but in studying the history of mathematics one should also stress the way that this

24. The implications of applying this scheme for mathematics have been elaborated in Corry 1989.
25. The distinction has been explicitly stated, for instance, by Michael Crowe (1975, 19) who claimed that "there are no revolutions in mathematics", thus separating "mathematics proper" from "incidental factors" such as "nomenclature, symbolism, metamathematics".
quantitative growth is accompanied, followed, or even sometimes influenced by a *qualitative*, new appreciation of the body of knowledge.

Qualitative change is usually change in the images of knowledge and it is essentially different from accumulation in the body of knowledge. For the latter there are quite clear criteria. Thus a theorem is added to the body of knowledge when a proof is found and, usually, though not invariably, a valid proof may be clearly distinguished from an invalid proof. What constitutes change in the images of mathematical knowledge, and how and when such change occurs, is more difficult to define. It is a task for the historian of mathematics to characterize the images of knowledge of a given period and to explain their interaction with the body of knowledge, on the one hand, and the subsequent transformations in both the body and images of mathematics, on the other.

Summarizing, then, one can say that mathematical knowledge comprises two interacting layers, namely, (i) the body of knowledge, a hard-core which grows through (quasi-)linear accumulation over time, and (ii) the images of knowledge which frame and direct the growth of the body of knowledge and affect the way mathematicians interpret and understand the contents of the body of mathematics. It may on occasions be hard to distinguish between the pure body of knowledge and pure images of knowledge. The hard-core mathematical knowledge cannot actually be separated from the way it is looked upon mathematically, as form cannot be separated from content (in mathematics or in any other field): they are conditioned by each other.

**IV. REVOLUTIONS AND IMAGES OF MATHEMATICS**

In the preceding section I claimed that there is a universally accepted standard to determine whether an item should or should not be added to the body of knowledge, namely, its endorsement by deductive proof. That this is so, is in itself an image of knowledge; in fact, this is the seminal, constitutive image of mathematical knowledge as we know it ever since the Greeks. But being an image of knowledge, it is the outcome of a particular, contingent historical process, rather than a necessary feature of knowledge as such. In fact, before the adoption of deductive proof as the touchstone of mathematical truth, various cultures became acquainted with considerable amounts of arithmetic and geometric 'facts'. It might be claimed that this should not be considered 'true mathematical knowledge' before its endorsement by deductive proof, but obviously this assertion would beg the question of why is it that mathematical knowledge is knowledge endorsed by deductive proof. The accumulation of 'known mathematical facts', the realization of
the very possibility to justify this knowledge by deductive proof, the discovery of particular proofs, the achievement of new mathematical knowledge through proof - all these are different faces of one and the same process of transformation. This was the first great transformation of the images of mathematical knowledge that affected the whole of the discipline: the “deductive transformation”. Why and how did this transformation come about? This is a question for historical research, and it might be illuminating to consider it with reference to the Kuhnian agenda.

Notice, first of all, that the acceptance of deductive proof as the decisive criterion of mathematical truth implies the rejection of other, formerly accepted criteria: Even the longest list of instances of a recurring mathematical situation will not suffice anymore to ascertain the truth of a particular mathematical claim; what is needed from now on is a deductive argument. In this sense, the deductive transformation was certainly a revolutionary change of images of knowledge; it was not the simple addition of a further criterion of truth to the existing ones, but it implied that a theretofore meaningful image of knowledge had to be rejected. Of course, this does not necessarily mean that under the new image, previously accepted items of the body of knowledge would have had to be rejected; the revolutionary aspect of this transformation concerns the images, not the body of knowledge. Moreover, a transformation of this magnitude and scope has occurred very seldom, if ever again in the history of mathematics. Its influence on the future course of events was certainly much more decisive than that of Dauben’s two examples. At the same time, the deductive transformation is indeed a good example of "revolutionary change" which is not a "major breakthrough" in the sense of the invention of the calculus, the discovery of incommensurables or any other particular innovation in the body of knowledge. Now, in this framework, there is still room for debate among historians, whether (1.3) and (1.4) hold for this particular example of transformation.

One can regard the ‘deductive transformation’ in terms of ‘paradigm-shift’ (in the sense discussed above). In fact, it was not in itself discovery of a new particular mathematical fact, argument or theory, yet it played an instrumental role in conditioning their subsequent discovery. Thus, there is room for historical debate regarding (2.1) and (2.2).

Interesting question arise when considering the issue of rationality (3.1 - 3.2) in reference to the deductive transformation. For instance, it is obvious that having chosen deductive proof as criterion for truth, criteria for rationality in the body of mathematics have also been established. But, can rationality criteria be articulated for choices in the images of knowledge? More specifically, can the historical act of adoption of deductive proof as truth-criterion be justified with rational arguments? Notice that this is not a question of principle, but rather an historical issue. The ques-
tion of principle could be answered, for instance, with a Popperian argument: if a choice is subject
to critical debate, it is this debate that confers its rational character upon that choice. The histori-
cal question, on the contrary, is whether the choice was actually done on rational, or rather on dif-
ferent (socially- or psychologically-conditioned, etc.) grounds. I feel inclined to answer that
choice was indeed actually justified on rational grounds, but at the same time I think that a case
may be made for the opposite claim, and the debate must be decided on the grounds of historical
evidence. As was argued in the former section, in this debate one can also ponder the role of the
mathematical, or scientific community in bringing about particular choices of images of knowl-
edge, and more specifically, the choice of deductive proof as truth-criterion.

In the next section we examine in greater detail the significance of each of the axes of the Kuhnian
agenda in terms of body and images of mathematics. At this stage, however, the question arises
whether one can mention additional changes in the images of mathematics of similar scope as the-
deductive transformation, throughout the history of the discipline? The answer is clearly in the
negative--so far. In fact, standards of proof have changed throughout history, and these changes
have often been noticed, and described in detail by historians of mathematics, as well as by
mathematicians themselves. Yet this changes have affected limited aspects of the images of
knowledge, and often only restricted domains of mathematical knowledge; the hegemony of
deductive proof itself as the exclusive criterion for truth for the whole of mathematics has been so
far undisputed. However, one would seem to perceive on the horizon the possibility of an even-
tual challenge to this hegemony. Potential contenders, perhaps not to succeed, but at least to share
a limited portion of the throne are probabilistic proofs and computer-aided proofs.

It is common knowledge that since the late nineteenth century probabilistic parlance
increasingly permeated many intellectual disciplines as to become a further legitimate way of justi-
fying either individual claims or whole theories in those domains. In particular, the adoption in
physics of probabilistic arguments and of whole theories that do not claim to more than a
probabilistic description of phenomena has been mentioned as a turning-point in the very concep-
tion of science. It has seldom been noticed, however, that mathematics is prominent among those
disciplines that have steadfastly denied any foothold to probabilistic arguments as a criterion of
truth. In fact, the development of the theory of probabilities since the seventeenth century until
its axiomatization in the twentieth century by Kolmogoroff was parallel to that of many other
mathematical theories; the increasing mathematical refinement in the treatment of probabilities
may have had some degree of influence on the gradual adoption of probabilistic arguments in the

27. It is remarkable that in L. Krüger et al. 1988, this fact is not even mentioned or analyzed.
other sciences, but it did in no sense transform the criteria of truth for mathematics, as it did for other disciplines -- so far. Probabilistic claims are proven as any other mathematical claim by deductive arguments, and the claim that a certain event has a probability $p$ of being true, is adopted with the same degree of certainty as that of any other mathematical claim. On the contrary, however, knowing that the probability that a certain mathematical statement $P$ be true is extremely close to 1 does not endow the statement with the status of "true".

But if the development of the theory of probabilities and the adoption of probabilistic arguments in other disciplines were not followed by a parallel adoption of similar arguments as a criterion of mathematical truth, developments in logic and algorithmics might in the future lead to such an eventuality. In fact, it has been recently been proven that certain decidable statements exist, whose proofs are much longer than a human or a machine can actually write down. It has been proposed that the truth of such statements be proven up to a very high degree of probability. The classic example of this is Michael Rabin's algorithm for proving primeness.\textsuperscript{28} Rabin devised a test that determines whether a given number is prime, in such a way that the probability that the opposite is the case is one in a billion; he said that a number that passed his test is "prime for all practical purposes".\textsuperscript{29} A claim of this kind in a mathematical text is, on the face of it, bound to raise much controversy; in practice, however, although some colleagues replied to it, Rabin's result has not brought about much debate. I think this is due to the fact that it concerns a rather marginal result. It would be interesting to speculate what would be the case if a much central result would turn out to be provable only by means of probabilistic arguments.

Computer-assisted proofs became the focus of philosophical discussion after the remarkable proof, by Apple and Haken, of a result that had been for long an open question: the four-colors theorem. This is not the place to discuss their proof and the reactions it aroused in detail.\textsuperscript{30} Let it only be remarked that, although the acceptance of such a proof as legitimate implies the transgression of long-existing mathematical tabus, a sensible portion of the mathematical community seems to have come to terms with it. Obviously, everyone would be more satisfied to have a normal, deductive proof of the theorem, yet the theorem is considered, by and large to have actually been proven.

Probabilistic and computer-assisted proofs are still marginal in mathematics, and I believe that, even if they become more common than they are now, deductive arguments will alone

\begin{itemize}
\item \textsuperscript{28} Rabin 1976.
\item \textsuperscript{29} Rabin's article and its implications are discussed in Kolata (1976).
\item \textsuperscript{30} See Tymoczko 1979; Detlefsen and Lucker 1980.
\end{itemize}
remain in the foreseeable future the preferred criterion for mathematical truth. Yet the very existence of alternative criteria, and the very fact that mathematicians have seen the need to pronounce themselves on the issue imply that the wall of mathematical consensus is not absolutely monolithic, even on its foremost principle, and that further debates and changes in the central images of mathematical knowledge are likely to appear in the future.

### V. PARADIGMS AND IMAGES OF MATHEMATICS

We proceed to connect all the elements introduced in the preceding sections, and to evaluate the relevance of each of the issues involved in the Kuhnian agenda for actual research in the history of mathematics, by discussing its possible application separately for the body and for the images of knowledge.

Section IV. discussed the adequacy of analyzing a specific event in the history of mathematics in terms of revolutionary change. The present section focuses especially on paradigms. It may thus be convenient to discuss in a somewhat more cursorily way the other axes of the Kuhnian agenda. Consider first the issue of rationality. It can always be claimed that logic is a matter of convention, and that therefore items are added to the body of knowledge on the basis, not of universally valid criteria, but rather of convention alone. Anyone willing to admit the validity of this claim will presumably therefore admit, that, the contents of the body of mathematics, is determined by pure convention (3.1). Now, the nature of logic and the basis for its acceptance is, of course, a very heavy issue to be dealt with in the present context, but, in short, one can reply to the claim that it is only a matter of convention with Lakatos’ argument (previously formulated by Popper, for the case of science in general), that the rationality of mathematics lies in the possibility of critically discussing existing proofs. Unless one admits that logic is a matter of convention alone, from what was said above it also follows that (3.2) hardly, if at all, applies to the body of mathematics: the choice whether or not to include an item in the body of knowledge does not necessitate psychological or sociological explanations. There are, to be sure, certain choices of mathematicians which may necessitate such explanations, but these belong to the images of knowledge: Choice of one’s own specialization discipline and of the specific problems to be addressed within it, choice of legitimate tools to solve the chosen problems, determination of typical arguments as standard proofs of a given mathematical discipline, etc. Thus, market consideration may explain, say, the flourishing of disciplines relevant to computer science or statistics, and the decline of other sub-disciplines during the last decade; Sociological or psychological
arguments may explain why a certain question was addressed and why the proof of a theorem was sought after. Such arguments are not needed, and in fact they cannot be used, to explain why after a proof was found, the respective theorem was added to the body of knowledge in those particular branches.

So much for the issue of rationality. We return now briefly to the issue of revolutions. In revolutionary change, it was said above, there is more than mere addition; standards of rationality, metaphysical beliefs and even language are put into question. Moreover, something essential must be rejected from the existing corpus of science in revolutionary change. Since the body of mathematical knowledge is only locally amended, and it changes, essentially, by addition, then obviously there can be nothing like pure revolutionary change in the body of mathematics. To assert the existence of pure revolutionary change (1.1)-(1.2), or of different, incommensurable, languages at different periods of history (1.3), seem therefore meaningless regarding the body of mathematics. Existing images of mathematics, on the other hand, may indeed be rejected when adopting new ones. Thus, a problem which is considered the most urgent open problem of a particular mathematical branch during a certain period of time, and which influences the whole of research in that branch to be directed towards its solution, may be totally forgotten after a while, either because it was solved or because new, more attractive and challenging problems appeared in its stead, thus creating a new focus of attention for the relevant community. The example of Fermat’s last theorem comes soon to mind.

The very fact, that change in the images of knowledge includes, among other aspects, processes of rejection, clears the way, in principle, for the existence of revolutionary change therein; yet, it is still an open question whether or not such revolutionary processes have indeed happened in the history of mathematics. This is an issue that historians of mathematics have to decide upon, by focusing on specific case-studies. As the example discussed in the former section indicates the case can hardly be made for an extreme version of (1.1), (1.2) and (1.3), regarding the images of mathematical knowledge, namely, that images of mathematical knowledge are changed through revolutionary processes, at the end of which one obtains a system of images which is incommensurable with the formerly existing one. The important point to be stressed, however, is that this is a question of historical fact, and not of logical possibility. As for (1.5), since all change in the body of knowledge is normal change, it follows, obviously, that there is significant change which is not revolutionary. If one admits the possibility of revolutionary change in the images of knowledge, then it is likely that some instances of it do not constitute major breakthroughs. Finally, if one admits the possibility of revolutionary change in the images of knowledge, one may defend, based on historical evidence, (1.4) with varying degrees of commitment.
We consider now paradigms. It should already be clear that only at the level of the images of knowledge there appears to be some interest in talking about paradigms in mathematics.

As was already stated, paradigm is a rather indefinite term. This may perhaps explain the interest the concept raised, but it explains its limitations as well. In spite of its indefiniteness one can nevertheless assert that any useful definition of paradigm must refer to something different from an individual theory or scientific discovery, but at the same time it must be able to influence the development of particular theories. Since it is also characteristic of the images of knowledge, as was mentioned above, that they may influence the course of development of the body of knowledge, then one readily sees the possibility of trying to discover paradigms and paradigmatic change in the images of knowledge. A paradigm (be it an exemplary instance of a theorem, a particular textbook, a successful technique applied in a particular branch, a type of problems that has proven to be fruitful, etc.) may led to the adoption (or modification, or rejection) of a certain system of images of knowledge in a given branch of mathematics, or in mathematics as a whole. This choice directs the interest of researches, and thus a particular theory, theorem, open problem or technique may get to be considered worth of attention or, on the contrary, attention may be driven away from it. But again, if a particular theorem comes to the focus of attention of a particular community of mathematicians, it will only be accepted as true after a proof of it is provided. Thus, paradigms directly affect the images, and only indirectly (through the images) the body of knowledge: the existence of a successful method for problem-solving in a given branch of mathematics may explain the attempt to apply it in other branches, but it does not account for the very success of the method. Therefore, the validity of any version of the claim, that it is paradigms and not theories (or individual theorems) that constitute the basic units of scientific change (2.1), or that a scientist cannot, while under the sway of one paradigm, seriously entertain a rival paradigm (2.2), is limited in the first place to the conceptual precedence and influence of paradigms over the images of knowledge alone, and never over the body of mathematical knowledge.

To complete this section, it must be added that similar considerations to those applied here to paradigms in mathematics can be repeated, mutatis mutandis, for the epistemological relevance of the mathematical community, and of the related claims (4.1), (4.2) and (4.3) stated above, claims that assign a specifically defined, epistemological function to that community on the contents of the body and of the images of mathematics.
VI. THE STRUCTURAL PARADIGM IN ALGEBRA

To properly summarize the present discussion, the degree of interest that the application of the concept of paradigm may offer to the historian of mathematics can be now best assessed by examining a concrete example. In the present section we examine the possibility of discussing the rise of modern, structural, algebra in terms of paradigm and paradigm-change.

It would be well beyond the scope of the present article to give a detailed account of the rise of modern, structural algebra. For our present purposes it will suffice to state, that the essence of the structural approach to algebra lays in the recognition, that several mathematical concepts, which had aroused within separate, though somewhat related mathematical contexts, are, in fact, individual varieties of one and the same species of mathematical entity, namely, that of "algebraic structures".31 This recognition was the result of a convoluted historical development, as a consequence of which, mathematicians came to realize, that a fruitful perspective emerged when all these concepts were approached from a common perspective; that is, when these concepts began to be defined in similar terms (i.e., through an abstract axiomatic formulation), began to be investigated by posing similarly formulated questions, and by using similar conceptual tools to solve those questions (isomorphisms, quotient structures, extensions, etc.), and when the theories that comprised the new discipline of "modern algebra" began to be developed by expecting similar kinds of answers to be given as the legitimate ones.

The rise of the structural approach to algebra implied the reformulation of many existing results, and a redefinition of the basic aims and legitimate methodology of algebra. Thus this process can be best described as the adoption of a new image of mathematical knowledge, rather than as a major breakthrough, or in fact, any other kind of change, in the body of knowledge. Of course, this image change took place after a considerable amount of new concepts and results had been added to the body of algebraic knowledge over the preceding decades, yet it cannot be argued that this growth necessarily implied a change of perspective like the particular one that actually took place. On the other hand, the adoption of this particular image of mathematical knowledge provided a conceptual framework within which an impressive quantity of extremely significant contributions to the body of knowledge were to be produced in the following decades.

That the rise of modern, structural algebra was in essence a change in images of knowledge is highlighted by the fact, that the first full-fledged manifestation of the idea that algebra is

the discipline dealing with "algebraic structures" is connected to the publication of a single textbook, namely, *Moderne Algebra* (1930), by the Dutch mathematician B.L. van der Waerden. As Van der Waerden explicitly wrote, his book was an elaboration of material he had studied in the previous years, in lectures given by Emmy Noether in Göttingen and by Emil Artin in Hamburg. But van der Waerden did not explicitly write, that the discipline presented in his book differed essentially from that appearing in any existing textbook of algebra. It presented algebra as the mathematical discipline whose aim is the definition of the various algebraic systems, and the elucidation of their respective structures. Although van der Waerden neither explicitly claimed, that this was in fact the aim of his book, nor explained what is in fact meant by elucidating the structure of an algebraic system, the various chapters of his book actually realize this aim, by showing how the structure of the hierarchically defined diversity of algebraic systems (groups, rings, fields, etc.) is elucidated. This practice was in glaring contrast to the classical image of algebra, as presented, for instance in Heinrich Weber's *Lehrbuch der Algebra* (1895). In this latter textbook, the main disciplinary concern was the theory of algebraic forms and polynomial equations. It discussed at length, and in all of its manifestations, the problem of equation-solvability.  

Seen in this terms, then, the rise of modern, structural algebra seems to offer an ideal example to assess the possible value of the Kuhnian agenda, and in particular the concept of paradigm, as a helpful tool for assessing the historical significance of a significative mathematical event.

First of all the structural approach may indeed be taken as an illuminating example of paradigmatic achievement at the level of the images of mathematics. It is not in itself a theory, or a specific theorem, yet it was instrumental in shaping the future course of development of the disciplines which embraced it, and not only of algebra. Moreover its process of development and consolidation was different from that of individual mathematical theories; it was not shaped by the discovery of particular theorems, or of mathematical concepts that lay at the base of it, nor was there a particular problem whose solution was its *raison d'être*. Yet, even if one is willing to see this episode in the history of mathematics as an example of paradigm-shift, it is perhaps much more difficult to defend the more general claim, that "Paradigms, not theories (and, of course, not individual discoveries), are the basic units of scientific achievement and change" (2.1) in mathematics. As for (2.2), there seems to be some room for debate concerning it in this regard: can an algebraist work simultaneously under the sway of both the structural and the classical, nineteenth-century image of algebra? In principle, there is no logical reason why this could not be the case,

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32. This is described in greater detail in Corry 1991, 23-28.
yet perhaps more interesting is the question, what has actually been the case in history, namely, whether or not algebraists, after the publication of *Moderne Algebra*, were conducting their own research, while *simultaneously* following the orientation implied by both images of algebra. This question is, in fact, open to historical research. Once again, any historian claiming that paradigms in the images of mathematical knowledge constitute the basic unit of historical change, should also address, by means of historical research, the more general question, whether a mathematician can conduct research, while working simultaneously under the sway of two different paradigmatic images.

Following the same line of enquiry, one may now also ask, whether the rise of the structural approach and the publication of *Moderne Algebra* were instances of *revolutionary change* in the images of algebraic knowledge. The dominant nineteenth-century image of algebra, as said above, saw it as the discipline dealing with the theory of algebraic forms and polynomial equations. Under the new image of algebra, the issue of solvability of polynomial equations was relegated to a very specific question arising within a particular sub-discipline, Galois theory, which itself was subsidiary to other, broader sub-domains of algebra, like group theory and field theory. Thus, the rise of modern algebra provides an example of significant change, which was however not followed by any rejection of existing items at the level of the body of algebra. Rather, under this new image of knowledge, the relative importance of existing items was reassessed. The change in images of knowledge consisted, in the case at issue, not in having completely solved the problem of solvability of polynomial equations within the framework of Galois theory, but in having come to consider this latter theory as a theory dealing with the problem of extensions of abstract fields and using the tools provided by group theory.\(^{33}\)

But, obviously, there still seems to be room for historical debate concerning issues (1.1) to (1.5) of the Kuhnian agenda. Thus regarding (1.1), the question arises whether the rise of modern, structural algebra was, or was not a case of pure revolutionary change in the images of knowledge. My personal opinion is, that this was not the case, and that one can also find in this process, many elements of continuity. Again, detailed historical research must be the only legitimate basis for any such answer. Nevertheless, even if one answers the former question in the negative, one can still argue for some version of (1.3) regarding this event, namely, that the basic language of algebra, before and after van der Waerden hanged so radically, as to make the questions addressed in the former, if not intelligible, at least irrelevant to the latter, and the other way round. Further,

\(^{33}\) For a detailed account of the development of Galois theory and its influence on the rise of the structural image of algebra see Toti Rigatelli 1989, 125-148, and Kiernan 1971, 135-144.
one can still ask, whether the kind of change represented by the rise of the structural approach to algebra, occurs very often in the history of mathematics, or whether, on the contrary, (1.2) it is a rather sporadic event. Statement (1.4) poses an interesting question to be investigated by the historian of mathematics: In my opinion, the historical process that led to the conception behind *Moderne Algebra* will appear, under closer inspection by historians, a piece-meal one; at the same time, however, one finds testimonies of van der Waerden himself and of several other mathematicians as having experienced a Gestalt-switch experience when first meeting that conception - their individual adoption of the new images of algebra was indeed "a sudden and unstructured event". Finally, the rise of the structural trend was not a "major breakthrough" (1.5), at least not in the same sense, that major events in the history of mathematics, such as the discovery of incommensurability or the development of the calculus, were. In particular, van der Waerden’s book contained no new important theorems or proofs, that had not been published before; still its influence was to be felt in many domains of mathematics beyond algebra over the decades to come. Certainly algebra was not the same discipline before and after the publication of *Moderne Algebra*.

Finally, the issue of rationality raises here further questions for historical research. Were there rational grounds for the widespread adoption of the approach? If so, can one deduce universally valid rules that apply to all choices of images of knowledge in the history of mathematics? Or must one rather explain this change in sociological or psychological terms? Answering these questions will lead us to decide whether or not issues (4.1) - (4.3) are relevant to the present discussion. Once more, this must be answered by detailed historical research.

VII. Conclusion

Paradigms, like other concepts connected with the Kuhnian agenda, may be defined and characterized in many different ways. These concepts become the more meaningful as a category of historical research, the bolder they are in their implicit assumptions concerning the development of scientific knowledge. The above discussed account of the issues contemplated in the Kuhnian agenda allows a relative evaluation of the merits of the alternative definitions of those concepts.

The above proposed account of the rise of the structural approach to algebra, in terms of images and body of mathematical knowledge, illustrates the potential contribution of studying specific events in the history of mathematics from the perspective offered by the Kuhnian agenda, and in particular by the concept of paradigm.
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