Kuhnian Issues, Scientific Revolutions and the History of Mathematics

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KUHN'S ACCOUNT of the development of science, together with its associated concepts of paradigm, normal science and scientific revolutions, has been the focus of intensive debate ever since its first publication in 1962.[1] Subsidiary issues in this debate are the applicability of Kuhn's theory to the history of individual scientific disciplines; in particular, the question has been discussed whether there are scientific revolutions in those disciplines. Such discussions are often preceded by preliminary considerations about the appropriate definition of 'scientific revolution'; having chosen one such definition, one may prove either that there have been or that there have not been revolutions in a given field. Thus discussions have often remained at the semantic level. Although the semantic freedom of authors to choose their own meaning for the term 'scientific revolution' should be respected, one can still approach the various definitions critically and evaluate their degree of interest and fruitfulness as analytical tools for research in the history of science.

In the present article I pursue a two-fold task. First I propose a model for reassessing the many interpretations of Kuhn's theory and for evaluating their relative interest and fruitfulness in describing the historical development of science. Then I discuss the applicability of the relatively more interesting versions of Kuhn to the particular case of the history of mathematics. Thus, rather than discussing whether a definition of scientific revolutions can be advanced such that revolutions are found to have occurred in mathematics, it is asked whether by analysing the history of mathematics in Kuhnian terms new insights are attained which would otherwise have been overlooked by historians and philosophers of mathematics.

1. 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

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[1]See for instance, the extensive bibliography appearing in G. Gutting (ed.), *Paradigms and Revolutions* (Notre Dame: Notre Dame University Press, 1980).

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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 all issues involved Kuhn originally adopted a most stringent and uncompromising position. Later on, in the attempts to further explain his position, 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.

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 as instrumental as *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 main issues considered in the Kuhnian agenda may be articulated around four axes:

1. Normal change versus 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 regardless 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 on each of the above-mentioned axes admit of 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 is of interest 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 farther it is from the positivistic conception of science and, therefore, the bolder and most interesting. Obviously, whether some version of Kuhn's view is also correct is a completely different question.

1. Normal versus Revolutionary Change

This is the central axis of the Kuhnian agenda. Kuhn's original version of the theory was meant to establish the existence of revolutionary change, and to explain how and when it comes about. It connected revolutions to paradigms, it allowed irrational factors into the account of the development of science, and it 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 the revolution; this means that both of them may not be held as true simultaneously. 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.[2]

[2]According to Philip Kitcher, this formulation describes Kuhn's own view. Cf. P. Kitcher, 'Theories, Theorists, and Theoretical Change', *Philosophical Review* **8** (1979), 519–547, see p. 520.

1.4. A scientific revolution is 'a relatively sudden and unstructured event like a Gestalt switch'.[3] It is a holistic process, which cannot 'be made piece-meal, one step at a time'.[4]
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 be taken 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.

2. Paradigms

This vague term (and its several synonyms) has been used to denote whole theories, exemplary instances of scientific achievement in the past, models, metaphysical conceptions, choice of problems and techniques for analysing them, and many other things. If 'paradigm' is synonymous with 'theory', then there is no need to discuss the former concept separately. Paradigms become meaningful 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 for 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 from (and, by preference, has conceptual priority over) that of theories. Otherwise paradigms are redundant as an analytical tool for the historian and the philosopher of 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.[5]

[3]T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962), p. 121.

[4]T. S. Kuhn, 'What Are Scientific Revolutions?', in L. Krüger *et al.* (eds), *The Probabilistic Revolution* (Cambridge, Mass.: MIT Press, 1988), pp. 7–22, see p. 19.

[5]This formulation is taken from J. W. N. Watkins, 'Against Normal Science', in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), pp. 25–38, see p. 34.

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. Whether or not that is the case in Kuhn's writings, the two issues are separate in the Kuhnian agenda. One can claim a role for paradigms, more or less committed to (2.1) and (2.2), without 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.[6]

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 about Kuhn's early version was its implication that the acceptance of a new paradigm is not simply a matter of applying rules. But this issue, like 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.[7]

3.2. Many among the scientist's choices do not obey universally valid rules; rather, these choices admit only sociological and psychological explanations.

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 content and internal logic of the scientists' ideas;

[6]It is worth pointing out that in characterizing scientific revolutions in 1988 (cf. Kuhn, *op. cit.*, note 4), Kuhn did not even mention the word 'paradigm'. In doing so, he finally dropped this problematic issue from his own version of the Kuhnian agenda.

[7]This has been one of the central targets of Popper's criticism of Kuhn, Cf. K. Popper, 'Normal Science and its Dangers', in Lakatos and Musgrave, *op. cit.*, note 5, pp. 51–58, see p. 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.'

the former undertaking was considered to be irrelevant to the latter.[8] 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, so contributing to make his book controversial. Later on, once more under the pressure of criticism, he weakened his position.[9]

The uncompromising view of the scientific community's structure and behavior as an instrumental factor in shaping the content 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.[10]
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 writing, yet it is necessary in order to confer a clearer meaning on (4.2).[11] Notice also that one can claim that the community influences the contents of science through paradigms, but there are also many other possible claims; 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' allowed equally for big and for very small communities, including even 'fewer than twenty-five persons'.[12] 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

[8]This is illuminatingly described in M. D. King, 'Reason, Tradition, and the Progressiveness of Science', in Gutting, *op. cit.*, note 1, pp. 97–98.

[9]The transformation of Kuhn's views on this issue is explained (and lamented) in King, *op. cit.*, note 8, pp. 107–115.

[10]This last formulation is taken from Gutting, *op. cit.*, note 1, p. 11. Gutting believes this phrase to encompass the 'real significance of Kuhn's work'.

[11]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 A. Musgrave, 'Kuhn's Second Thoughts', in Gutting, *op. cit.*, note 1, pp. 39–53, see p. 40.

[12]T. S. Kuhn, 'Second Thoughts on Paradigms', in F. Suppe (ed.), *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1974), pp. 459–482, see p. 464.

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. A radical version of Kuhn's theory is obtained by adopting the above-introduced formulations with full commitment. Analysing the history of science in such terms, were it to prove itself adequate, would be bound to provide many new insights to the historian. In all likelihood, 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 — as 'important' or 'routine' — 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.

2. Arguing for Revolutions in Mathematics: A Case Study

An articulated attempt to analyse the history of mathematics in Kuhnian terms was advanced by Joseph Dauben.[13] In an article of 1984 he 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

[13]See J. Dauben, 'Conceptual Revolutions and the History of Mathematics', in E. Mendelsohn (ed.), *Transformation and Tradition in the Sciences* (Cambridge: Cambridge University Press, 1984), pp. 81–103. Dauben's article was intended, amongst other things, to contest M. J. Crowe's denial of the existence of revolutions in mathematics. See M. J. Crowe, 'Ten "laws" Concerning Patterns of Change in the History of Mathematics', *Historia Mathematica* **2** (1975), 161–166, see p. 165.

Additional attempts at a Kuhnian analysis appear in P. Kitcher, *The Nature of Mathematical Knowledge* (New York: Oxford University Press, 1983), pp. 149 ff., and H. Mehrtens, 'T. S. Kuhn's Theories and Mathematics: A Discussion Paper on the "New Historiography of Mathematics" ', *Historia Mathematica* **3** (1976), 297–320. For a critique (slightly different from the one intended here) of their points of view, see L. Corry, 'Linearity and Reflexivity in the Growth of Mathematical Knowledge', *Science in Context* **3** (1989), 409–440, see pp. 427–429.

and Cantor's theory of sets were examples of revolutions.[14] To be sure, Dauben's definition is not intended to be a reformulation, or an adaptation of Kuhn's definition; indeed it is 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.[15] In order to elucidate the meaning of 'revolution', Dauben suggests following 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.[16]

According to 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 science', and in particular, that it should comply to the framework of 'Professor Kuhn's model of anomaly–crisis–revolution'.[17] 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.

[14]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 ε–δ calculus, and Abraham Robinson's non-standard analysis.

[15]Dauben gives reference to I. B. Cohen, 'The Eighteenth-Century Origins of the Concept of Scientific Revolutions', *Journal of the History of Ideas* **37** (1976), 257–288, and I. B. Cohen, *The Newtonian Revolution, with Illustrations of the Transformations of Scientific Ideas* (Cambridge: Cambridge University Press, 1976). For a recent reformulation of these ideas, see I. B. Cohen, 'Scientific Revolutions, Revolutions in Science, and a Probabilistic Revolution 1800–1930', in Krüger *et al.*, *op. cit.*, note 4, pp. 23–44.

[16]Cf. Dauben, *op. cit.*, note 13, p. 83.

[17]*Ibid.*, p. 82.

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.[18]

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 matters 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 (2), (3) and (4) 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): he 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 quantities, 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.[19]

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

[18] *Ibid.*, p. 84.
[19] *Ibid.*, pp. 88–89.

peculiar about this long episode in the history of mathematics, that justifies 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, one can only find in Dauben's definition the separation of revolutionary from normal change in mathematics, and even this is not clearly formulated. Dauben's characterization, moreover, does not explicitly provide the means to identify, from among other contributions to mathematics, those which are truly revolutionary; the particular examples chosen by Dauben make things even more confused. 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'.[21] Dauben justifies his claim by referring to proofs that appear in Book V of Euclid's *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 the 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.[22]

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.[23]

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

[20]Incidentally, I. B. Cohen himself stressed the need to answer these kinds of questions clearly, when defining scientific revolutions. Cf. 'Scientific Revolutions, Revolutions in Science, and a Probabilistic Revolution 1800–1930', in Krüger *et al.*, *op. cit.*, note 4, pp. 23–44, see p. 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.'

[21]Dauben, *op. cit.*, p. 88.

[22]*Ibid.*, p. 93.

[23]*Ibid.*, p. 93. Italics in the original.

of hitherto unsolved problems constitute revolutionary change? If this is the case, one is led to conclude that, for Dauben, 'revolutionary change' is tantamount to 'major breakthrough'.[24] Though, undoubtedly, evaluating the relative significance 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 'revolution'. 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 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.[25]

Although the last sentence implies, once more, that for Dauben the term 'revolutionary change' implies, above all, 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.[26]

'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 Dauben's acount, for considering an instance of change as revolutionary,

[24]The three additional examples of revolutions in mathematics advanced by Dauben in his San Sebastián talk (note 14, above) give further credence to my conclusion here.

[25]Dauben, *op. cit.*, note 13, p. 88.

[26]*Ibid.*, p. 93

without having to include under it every instance of 'remarkable break-throughs'. Of course, the question remains open whether Dauben's two examples conform to this criterion, but that is a different question which will not be discussed here.[27]

3. The Body of Mathematics and Images of Mathematics

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

Any scientific discipline raises two sorts of questions: (1) substantive questions of the discipline, and (2) questions about the discipline *qua* discipline, or meta-questions. One can accordingly distinguish two layers related to any scientific field: the 'body' of knowledge (answers to the first kind of questions) and 'images' of knowledge (answers to the second kind of questions). The body of knowledge includes theories, 'facts', methods and open problems. The images of knowledge play the role of 'selectors' for the body of knowledge, by answering meta-questions such as: which of the open problems of the discipline most urgently demands attention? How should one 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? etc. It is clear that answers to this kind of question are dictated not only by the substantive content of the body of knowledge alone, but also by additional, external factors.

It may sometimes be hard to distinguish between pure body of knowledge and pure images of knowledge. Newton's theory of motion clearly belongs to the body of physics; it is a statement about how bodies move. The claim that Einstein's theory is 'simpler' than Newton's clearly belongs to the images of scientific knowledge; it is a claim about physical theories rather than a claim about the physical objects. Gödel's theorems, on the other hand, belong to the body of mathematics; they are results otained within a specific branch of the discipline. They may also be taken to be, however, a claim *about* (an intrinsic limitation, in this case) of mathematics, the discipline.

Science as a system of knowledge is composed of the two layers, body and images of science, which organically interact and do not have separate existence. The separation mentioned here is an analytical one, which the historian of science may identify in hindsight. The study of the interaction between these

[27]From the other three examples advanced by Dauben, non-standard analysis certainly does not fit the criterion.

two layers may provide significant insights for understanding the growth of scientific knowledge in general; in particular, the peculiar fashion in which the body and the images of mathematical knowledge interact may shed new light upon historical and philosophical inquiries of mathematics, and upon the differences between mathematics and other scientific disciplines.[28] As will be seen below, the body/images scheme may help us understand the issue of revolutions in mathematics, and it is therefore convenient to briefly elaborate on this point here.

Facts and theories are continually added to and deleted from the body of scientific knowledge. However, in contrast to other scientific disciplines, claims that entered the body of mathematics through proof are seldom, if ever, rejected. New theorems and new proofs of old theorems do not falsify old theorems and proofs. Still, the process of mathematical change is not one of plain linear accumulation. Discovering a new theorem, proof or concept is but one of the ways in which mathematical knowledge changes. Yet in the event of one such discovery, it is the images of knowledge (which are determined by the content of the body of knowledge, but also by social and philosophical considerations, by the interaction of mathematics with other sciences, and so on) that determines the way in which the new item will be integrated into the existing picture of knowledge, whether it will be considered an important contribution or whether it will be ignored. Eventual changes in the images of knowledge may later transform the relative status of existing pieces of accepted knowledge; theories, techniques, or sets of problems, that the dominant images of knowledge render uninteresting or devoid of scientific relevance, may eventually, under a different system of images of mathematics, come to be the focus of attention of the mathematical community, or at least a specific part of that community.[29] Thus, change proceeds not only *quantitatively*, by addition of new results or concepts. These additions are, of course, fundamental to the growth of mathematics, but real change occurs only insofar as the quantitative growth is accompanied by a *qualitative* new appreciation of the body of knowledge.

I said above that claims that entered the body of mathematics through proof are seldom if ever rejected. *Seldom*, rather than *never*, since there have been several cases in which proofs that had been accepted as correct were later

[28]A comprehensive discussion of the body/images scheme for scientific knowledge in general appears in Y. Elkana, 'A Programmatic Attempt at an Anthropology of Knowledge', in E. Mendelsohn and Y. Elkana (eds), *Sciences and Cultures*, Sociology of the Sciences, vol. 5 (Dordrecht, Boston and London: Reidel, 1981), pp. 1–170. For a detailed discussion of the scheme as a useful tool for studying the history of mathematics, see Corry *op. cit.*, note 13.

[29]For an illuminating example of how changes in the images of knowledge may bring about a reassessment of an existing body of knowledge see H. M. Edwards, 'An Appreciation of Kronecker', *The Mathematical Intelligencer* **9** (1987), 28–35, pp. 33–35. Of course, Edwards' account is not explicitly couched in terms of the body/images scheme.

then obviously there can be nothing like pure revolutionary science in mathematics. (1.1), (1.2) and (1.3) are 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 springs to mind.

The very fact that change in the images of knowledge includes processes of rejection clears the way, in principle, for the existence of revolutionary change therein; yet one may still argue whether or not such revolutionary processes do indeed happen in the history of mathematics. I will claim below that certain changes in the images of mathematics have been, to a considerable degree, revolutionary; nevertheless, I doubt whether the case may be made for an extreme version of (1.1), (1.2) and (1.3), regarding the images of mathematical knowledge. Be that as it may, this is a point 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.

4. Revolutions in the 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 the case, 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 ancient 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 arithmetical and geometrical '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 of justifying 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 any more 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 hitherto meaningful image of knowledge had to be rejected. At the same time, however, this revolutionary change in images of knowledge did not necessarily bring about rejection of the existing body of knowledge. The revolution concerned the criteria of true mathematical knowledge, and not the specific content of that knowledge.

The influence of this particular change of images of knowledge on the future course of events was all-pervasive; one cannot now think of mathematics without thinking of proof as the one criterion of truth. Clearly the deductive transformation was much more decisive in the history of mathematics than any of Dauben's examples, or any other major breakthrough in the body of knowledge than one can think of. At the same time, however, the deductive transformation is indeed a good exmple of a 'revolutionary change' which is not itself 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. Of course, the adoption of this new image of mathematical knowledge did in the long run bring about many innovations in the body of knowledge, but it in no sense implied the rejection of previously accepted items of that body of knowledge. Yet, within this framework of ideas, 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, in the case of the deductive transformation regarding (2.1) and (2.2).

Interesting questions 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 the criterion of 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 in general? More specifically, can the historical act of adopting deductive proof as the criterion of truth be justified with rational arguments? Notice that this is not a question of principle but rather an historical issue. The question 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 historical question, by contrast, is whether the choice was actually done on rational, or rather on different (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 consider the role of the mathematical or scientific community in bringing about particular choices of images of knowledge, and more specifically, the choice of deductive proof as the criterion of truth.

Can further cases of change in the images of mathematics be pointed out throughout the history of the discipline, of similar scope and significance as the deductive transformation? The answer seems to be 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 such 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. Nevertheless, one would seem to perceive on the horizon the possibility of an eventual 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 so as to become a further legitimate way of justifying 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 conception 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.[33] 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 other sciences, but it in no sense transformed the criteria of truth for mathematics, as it did for other disciplines — so far. Mathematical claims are proven in the theory of probability as in any other mathematical branch, by deductive arguments, and the mathematical claim '$P(X) = p$' (i.e. that a certain event X has a probability p) is adopted with the same degree of certainty as that of any mathematical claim. On the contrary, however, knowing that $P(A) = p$, where A is any mathematical claim and p is extremely close to 1, does not endow A 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 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.[34] Rabin's algorithm takes a number p, and very effectively calculates the probability P that 'p is not prime'. Rabin applied his algorithm to big numbers and, in case that certain number p yielded P less than one in a billion, he said that p is 'prime for all practical purposes'.[35] 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 relatively isolated 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. Be that as it may, the existence of the proof indicates the actual possibility of advancing an alternative criterion for truth in mathematics.

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

[33]It is remarkable that in the recently published Krüger, *op. cit.*, note 4, this fact is not even mentioned or analysed.

[34]M. Rabin, 'Probabilistic Algorithms', in J. F. Traub (ed.), *Algorithms and Complexity: New Directions and Recent Results* (New York: Academic Press, 1976, pp. 21–40.

[35]Rabin's article and its implications are discussed in G. B. Kolata, 'Mathematical Proofs: The Genesis of Reasonable Doubt', *Science* **192** (1976), 989–990.

an open question: the four-colors theorem. This is not the place to discuss their proof and the reactions it aroused in detail.[36] 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 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 on the issue, imply that the wall of mathematical consensus is not absolutely solid, 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. Of course the question remains open whether in the future we shall witness a second *revolutionary* change in the images of knowledge, of the scope and significance of the deductive transformation.

5. The Structural Revolution in Algebra

Finally, I want to briefly consider the rise of the structural trend in algebra, in terms of the above-discussed concepts. This will illuminate, I believe, the kind of insights we are bound to gain from an analysis of particular episodes in the history of mathematics in terms of the Kuhnian agenda and the scheme body/images of knowledge.

The essence of the structural approach to algebra is the recognition that several concepts that had appeared in separate, though related mathematical contexts are, in fact, individual varieties of one and the same species of mathematical entity, namely, that of the 'algebraic structures'. Mathematicians found that a fruitful perspective emerged when they studied all these concepts from a common perspective; that is, when they defined them in similar terms (i.e. through an abstract axiomatic formulation), and then investigated them by addressing similar questions, by using similar conceptual tools to solve those questions (isomorphisms, quotient structures, extensions, etc.), and by expecting similar answers to be given as the legitimate ones of the discipline.

The rise of the structural approach to algebra can thus be seen as the adoption of a new image of mathematical knowledge, rather than as a major

[36]See T. Tymoczko, 'The Four-Color Problem and its Philosophical Significance', *Journal of Philosophy* **76** (1979), 57–83, and M. Detlefsen and M. Lucker, 'The Four-Color Problem and Mathematical Proof', *Journal of Philosophy* **77** (1980), 803–824.

breakthrough in the body of knowledge. Of course, this change took place after a considerable amount of new concepts and results had been added to the body of algebraic knowledge over the last few decades, yet it cannot be argued that this growth *necessarily* implied a change of perspective like the particular one that actually took place.

Traditional accounts of the rise of the structural trend in algebra have been affected by two main characteristic forms of confusion. The first is having identified this new trend exclusively with the widespread adoption of the abstract formulation of concepts. On closer inspection of the historical facts, it turns out that the mere technical ability to formulate such definitions did not bring real change in the images of algebra until it was combined with the identification of the various concepts (groups, fields, rings, etc.) as varieties of the same species.[37] The second form of confusion concerns the fact that several formal definitions of 'mathematical structure' were advanced, and that the development of the structural trend in algebra has often been told as the development of one of the particular formal definitions.[38] For lack of space, we can not discuss these issues here; I thus limit myself to summarizing the above claims by stressing the fact that the rise of the structural trend in algebra implied, above all, a change in the images of mathematical knowledge.

A fully-fledged realization of the structural image of algebra first appeared in print in van der Waerden's *Moderne Algebra*.[39] Van der Waerden did not explicitly say so, but his book represented a clear departure from any previous image 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. By contrast, the classical image of algebra saw the discipline as concerned with polynomial equations and the problems of their solvability.[40] We can now see how the Kuhnian agenda is helpful in assessing the historical significance of the publication of van der Waerden's book and, more generally, of the rise of the structural approach to algebra.

First one must 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 resolution of polynomial equations, and more generally, with the problem of their solvability. Under the new image of algebra this problem was relegated to a particular question within the particular sub-discipline of Galois theory, which itself was subsidiary to other, broader sub-domains of algebra, such as group theory and field theory. Thus, at the level of the body of algebra, there was no rejection of

[37]See L. Corry, 'Libros de Texto e Imágenes del Algebra en el Siglo XIX', *Llull* **14** (1991), 7–30.
[38]See Corry, *op. cit.*, note 13.
[39]B. L. van der Waerden, *Moderne Algebra* (Berlin: Springer Verlag, 1930).
[40]This is described in greater detail in Corry, *op. cit.*, note 13, pp. 23–28.

existing items, but under the new image the relative importance of those 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.[41]

There still seems to be room for historical debate concerning (1.1)–(1.5). Regarding (1.1), I feel strongly inclined to claim that this was not a case of pure revolutionary change in the images of knowledge, and that there are also many elements of continuity; yet one can still argue for some version of (1.3) regarding this event. One can still ask [(1.2)] 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 it is a rather sporadic event. (1.4) poses an interesting question to be investigated by the historian of mathematics. I think that the historical process that led to the conception behind *Moderne Algebra* will appear, under closer inspection by historians, a piecemeal 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'.[42] Finally, the rise of the structural trend was not a 'major breakthrough', in the sense that the discovery of incommensurability or the development of the calculus were. In particular, van der Waerden's book contained no new important theorem or proof 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*.

As for the issue of paradigms, 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 mathematical concepts that lay at the base of it, nor was there a particular problem

[41]For a detailed account of the development of Galois theory and its influence on the rise of the structural image of algebra, see L. Toti Rigatelli, *La Mente Algebrica — Storia dello sviluppo della Teoria di Galois nel XIX secolo* (Varese: Bramante Editrice, 1989), pp. 125–148, and B. M. Kiernan, 'The Development of Galois Theory from Lagrange to Artin', *Archive for the History of Exact Sciences* **8** (1971), 40–154, see esp. pp. 135–144.

[42]Cf., e.g., G. Birkhoff, 'Current Trends in Algebra', *American Mathematical Monthly* **80** (1973), 760–782, see p. 771, and J. Dieudonné, 'The Work of Nicolas Bourbaki', *American Mathematical Monthly* **77** (1970), 134–145, see pp. 136–137.

whose solution was its *raison d'être*. Still, there is a room for debate concerning (2.2) in this regard: can an algebraist work simultaneously under the sway of both the structural and the classical image of algebra? In principle there seems to be no logical reason why this could not be the case, yet it is also important to consider what has actually been the case in history. This issue is open to historical research.

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? This will also 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 further historical research.

6. Conclusion

Scientific revolution may be defined and characterized in many different ways. The concept becomes the more meaningful as a category of historic research, the bolder it is in its implicit assumptions concerning the development of scientific knowledge. The above account of the issues involved in the Kuhnian agenda allows a relative evaluation of the merits of alternative definitions of 'scientific revolutions'.

Considered in those terms, one concludes that any meaningful definition of 'scientific revolutions' which allows separating 'revolutions' from 'major breakthroughs' excludes the possibility that such revolutions take place in the body of mathematical knowledge. On the other hand, however, revolutions may indeed take place in the images of mathematics. The above accounts of the role of proof in mathematics, and of the rise of the structural approach to algebra, illustrate the potential contribution of studying specific events in the history of mathematics from this perspective.