Revolutions in Mathematics

Edited by

DONALD GILLIES

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'I have been ever of opinion that revolutions are not to be evaded.'
Benjamin Disraeli, from *Coningsby, or the new generation* (1844).

Preface

The idea of the present collection arose gradually from discussions with quite a number of people. My attention was first drawn to the debate between Crowe and Dauben on whether there were revolutions in mathematics by Caroline Dunmore, whose thesis on the development of mathematics I was supervising. The controversy seemed to me an intriguing one. Here were two distinguished historians of mathematics, one of whom (Crowe) proposed as a law that revolutions never occur in mathematics, while the other (Dauben) maintained that such revolutions do occur and gave examples. Moreover, both sides of the debate were very well argued.

Caroline Dunmore developed her own ideas on this subject during the academic year 1986–7, and read a paper entitled: 'Are there revolutions in mathematics?' to our departmental seminar at King's College, London in June 1987. Her theory, which in a certain sense develops the ideas of Crowe, formed part of her Ph.D. thesis, and is now presented as Chapter 11 of this book. My own opinion on the question inclined rather more to Dauben than to Crowe.

I was fortunate at this juncture to have a chance to meet both Michael Crowe and Joseph Dauben, and to discuss the whole problem with them. Michael Crowe was on sabbatical leave in England in 1986–7, and indeed gave a paper to our departmental seminar in October 1986. I met Joseph Dauben in the summer of 1987, and then again in January 1988 when he gave a paper on revolutions in mathematics in Oxford. Yuxin Zheng from Nanjing University spent the academic year 1987–8 in London, and, in the course of our many agreeable discussions on the philosophy of mathematics, the topic of revolutions in mathematics kept recurring. In September 1988 I met Giulio Giorello at a conference, and we had a long discussion about the whole question. As a result of all these meetings, the plan for the present book took shape.

The idea was to have a collection on revolutions in mathematics which would reprint the original papers that started the debate, and include also a series of specially commissioned papers discussing the question from different points of view and describing different historical examples of what might be considered revolutions in mathematics. As editor I must take this opportunity to thank all the contributors for their efforts. Nearly everyone was busy with other work, but everyone was intrigued by the problem, and the papers came in with surprising alacrity. My colleague, Dr John Milton, was of great assistance on some points of erudition. I should also like to mention the invaluable secretarial help I received from Phyllis Devitt in preparing the papers for the publisher.

More details about the contents of the volume will be found in the introduction. It remains only for me to conclude this preface with some further words of thanks and acknowledgement. First of all, I should like to mention that two of the younger contributors (Caroline Dunmore and Paolo Mancosu) did the research for their papers while holding fellowships at Wolfson College, Oxford. I would like, therefore, to thank Wolfson for their support for the history and philosophy of science and mathematics, particularly at a time when it was very difficult indeed in the UK to obtain funding for this important intellectual area. Secondly, I would like to make an acknowledgement of rather a different character to my former Ph.D. supervisor, Imre Lakatos. While I was completing my thesis, Imre Lakatos was editing with Alan Musgrave the justly famous collection *Criticism and the growth of knowledge*, which appeared in 1970. This volume discusses the ideas of Kuhn, and the alternative approaches of Popper, Lakatos, Feyerabend, and others in the context of science. Imre Lakatos, however, was planning at the time of his early death on 2 February 1974 to reconsider some of these questions in the context of mathematics rather than science. I had several discussions with him about these plans, and this was certainly one of the sources of inspiration for the present collection.

King's College London D.G.
July 1991

Contents

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Introduction

DONALD GILLIES

ARE THERE REVOLUTIONS IN MATHEMATICS?

In modern English, 'revolution' is most widely used in a political context, and talk of revolutions in science or mathematics can, with justification, be considered as the application of a political metaphor to the development of science or mathematics. It will help us to evaluate this metaphor if we begin with a brief consideration of some political revolutions.

European history of the last few hundred years affords three classic examples of political revolutions. First, chronologically, comes the seventeenth-century revolution in Britain. This began in 1640 when Charles I was forced to summon the Long Parliament because of the expense of his war in Scotland. Conflicts between King and Parliament led to a civil war, and a phase was ended with the execution of Charles I in 1649. His son Charles II was, however, restored in 1660, but this led in turn to a second minor revolution, the so-called Glorious Revolution of 1688 in which James II was expelled and replaced by William and Mary. Next we have the French Revolution, which is normally taken as having begun in 1789. Its course of events has many points in common with the earlier British revolution. Here again, the monarchy was overthrown and the king executed. Here again a dictatorship was established, only to give way to a restoration of the monarchy in 1815. And here again, as a sort of epilogue, an unsuitable king (Charles X) was replaced by a more suitable one (Louis Philippe) in 1830. Thirdly, there is the Russian Revolution of 1917. One point of difference from the earlier revolutions is worth noting: the Tsar, like Charles I and Louis XVI, was killed, but since his death there has been no question of restoring the monarchy.

Let us now turn from politics to science. The most important text here is of course Kuhn's *The structure of scientific revolutions* (1962). Though most historians and philosophers of science (including the later Kuhn!) would disagree with some of the details of Kuhn's 1962 analysis, it is, I think, fair to say that Kuhn's overall picture of the growth of science as consisting of non-revolutionary periods interrupted by the occasional revolution has become generally accepted. A scientific revolution, according to Kuhn, consists in the overthrow of a previously dominant paradigm and its replacement by a new paradigm. Three standard examples of scientific revolutions will illustrate this process.

In the Copernican revolution, the Aristotelian–Ptolemaic paradigm was overthrown and, after a rather involved series of intermediate steps, replaced by the Newtonian paradigm. In the chemical revolution, a paradigm in which combustion was considered as the loss of phlogiston was replaced by a new one in which combustion was considered as the addition of oxygen. In the Einsteinian revolution, the paradigm of Newtonian mechanics was replaced by the theory of relativity.

One interesting point to notice is that these three scientific revolutions occurred at more or less the same times as the three political revolutions mentioned earlier. The Copernican revolution was brought to a close by the publication of Newton's *Principia* in 1687, while only a year later Britain's political upheavals were settled by the Glorious Revolution of 1688. The chemical revolution overlapped with the beginning of the French revolution. Lavoisier's great *Traité élémentaire de la chimie* was published in 1789, and he hmself was guillotined in 1794. Finally, Einstein's two papers introducing the general theory of relativity ('Zur allgemein Relativitätstheorie') appeared in 1915, two years before the Bolshevik Revolution.

Of course, these overlaps in time could be just coincidental, but they do suggest that there might be some connections between scientific and political revolutions. I will not, however, pursue this question further here, but turn instead to the central question of this book. Let us grant, with the majority, that the concept of revolution can be usefully applied to the growth of science. Our problem is whether it can be extended further to cover episodes in the development of mathematics.

Given the interest in Kuhn's work in the 1960s and early 1970s, it is understandable that this question should have suggested itself to historians of mathematics working at that time. One historian in particular, Michael Crowe, started to consider the problem. His reflections were further stimulated by his reading of Lakatos's *Proofs and refutations* (1963–4), and by discussions with Kuhn in 1973. At a colloquium on the history of modern mathematics sponsored by the American Academy of Arts and Sciences held in Boston on 7–9 August 1974, Crowe read the first version of his paper, 'Ten "laws" concerning patterns of change in the history of mathematics'. It aroused little interest at the conference, but has subsequently stimulated an enormous amount of discussion. In this paper Crowe puts forward his famous Law 10, that 'Revolutions never occur in mathematics'. The paper was published the next year (1975) in *Historia Mathematica*, and is here reprinted as Chapter 1.*

The same question of whether Kuhn's view of science might apply to mathematics was independently exercising the mind of another American

* Subsequent references to this paper will cite it using the date of first publication, as in '(Crowe 1975)', but any page numbers given will be those of the reprint in this volume. The same system will be used for the papers by Mehrtens (1976), reprinted here as Chapter 2, and Dauben (1984), reprinted here as Chapter 4. This is in accordance with the practice followed throughout this volume (see the note at the beginning of the Bibliography).

historian of mathematics—Joseph Dauben. In the early 1970s, Dauben was carrying out some of the research that would lead to his life of Cantor (Dauben 1979). He reached the conclusion that revolutions do occur in mathematics, and indeed that Cantor's work in mathematics was revolutionary in character. At the fiftieth-anniversary meeting of the History of Science Society held on 27 October 1974 at Norwalk, Connecticut, an entire session was devoted to the history of mathematics and recent philosophies of science. Here the growth of mathematics was discussed in the light of Kuhn's work, and Kuhn himself acted as a commentator. Dauben read a paper in which he put forward his view of Cantor as a mathematical revolutionary. Dauben later added a second example of a revolution in mathematics, namely the Pythagorean discovery of incommensurables, and published his defence of revolutions in mathematics (Dauben 1984); this paper is reprinted as Chapter 4 of the present volume.

Discussions of Kuhn and mathematics took place in Europe as well, and Herbert Mehrtens from Germany published a paper on the subject in 1976, which is here reprinted as Chapter 2. I give a brief account of Mehrtens' ideas later in this introduction, but it will be convenient to start by considering the debate in the USA between Crowe and Dauben.

THE CROWE–DAUBEN DEBATE

Crowe (1975) gives as his Law 10 'Revolutions never occur in mathematics', and goes on to justify this claim as follows (p. 19):

> . . . this law depends upon at least the minimal stipulation that a necessary characteristic of a revolution is that some previously existing entity (be it king, constitution, or theory) must be overthrown and irrevocably discarded.

Here Crowe in effect proposes a necessary condition for something to count as a revolution, namely 'that some previously existing entity . . . be overthrown and irrevocably discarded'. He goes on to point out that the Copernican revolution did indeed satisfy this condition, because the previously existing theories of Ptolemy and Aristotle were certainly 'overthrown and irrevocably discarded'. On the other hand, this condition rules out the possibility of revolutions in mathematics, since the development of new mathematical theories does not lead to older theories being 'irrevocably discarded'. For example, the discovery and development of non-Euclidean geometry is sometimes claimed to be a revolution in mathematics. However, according to Crowe's condition, it cannot be, since the discovery of non-Euclidean geometry did not lead to Euclidean geometry being 'irrevocably discarded'. Indeed, as Crowe (1975) himself says, '. . . Euclid was not deposed by, but reigns along with, the various non-Euclidean geometries' (Crowe 1975, p. 19).

Let us next consider what Dauben has to say in reply to these very persuasive arguments of Crowe's. Dauben agrees with Crowe that older theories in mathematics, such as Euclidean geometry, are not discarded in the way that has happened to some scientific theories such as Aristotelian physics, or the phlogiston theory of combustion. On the other hand, he thinks that there have occurred radical innovations which have fundamentally altered mathematics, and so are justifiably referred to as revolutions, even though they have not led to any earlier mathematics being 'irrevocably discarded'.

Dauben (1984) explains his sense of revolution as follows:

> But following the French Revolution . . . revolution commonly came to imply a radical change or departure from traditional or acceptable modes of thought. Revolutions, then, may be visualized as a series of discontinuities of such màgnitudes as to constitute definite breaks with the past. After such episodes, one might say that there is no returning to an older order. (Dauben 1984, p. 51)

Dauben then goes on to cite with approval Fontenelle's description of the development of the infinitesimal calculus as a revolution in mathematics. Fontenelle dates the onset of this revolution to around 1696, when the first edition of the Marquis de l'Hôpital's *Analyse des infiniment petits* was published. Dauben comments:

> It was a revolution that Fontenelle perceived in terms of character and magnitude, without invoking any displacement principle—any rejection of earlier mathematics—before the revolutionary nature of the new geometry of the infinite could be proclaimed. For Fontenelle, Euclid's geometry had been surpassed in a radical way by the new geometry in the form of the calculus, and this was undeniably revolutionary. (Dauben 1984, p. 52)

Dauben next supports his conception of revolutions in mathematics by a very interesting political analogy (which will be developed in a moment). He says:

> . . . the Glorious Revolution . . . marked England's political revolution from the Stuart monarchy. The monarchy, we know, persisted but under very different terms.
>
> In much the same sense, revolutions have occurred in mathematics. However, because of the special nature of mathematics, it is not always the case that an older order is refuted or turned out. Although it may persist, the old order nevertheless does so under different terms, in radically altered or expanded contexts . . . 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.' (Dauben 1984, p. 52)

Here Dauben is making the important point that, although an older mathematical theory may persist rather than being 'irrevocably discarded' after some striking change, it may none the less be 'relegated to a significantly lesser position', just as the British monarchy persisted after the Glorious

Revolution, but 'was relegated to a significantly lesser position'. Later on Dauben describes such a relegation in the following terms:

> . . . the old mathematics is no longer what it seemed to be, perhaps no longer of much interest when compared with the new and revolutionary ideas that supplant it. (Dauben 1984, p. 64)

Both sides of this debate are very well argued. Some readers will no doubt have more sympathy with Crowe, while others will incline to Dauben's position. Among the contributors to this volume there is a variety of opinions on this, as on other problems concerned with revolutions, as I shall show later in this introduction. Here I consider a little further the question of the different types of political revolution mentioned earlier, and whether these distinctions can be applied to science and mathematics. This leads to a development which is perhaps more favourable to Dauben than to Crowe. For an alternative analysis which leads to a position closer to Crowe's, the reader is referred to Dunmore's contribution in Chapter 11.

The three political revolutions described on p. 1 were all revolutions against a form of monarchy, but it is not true to say that monarchy was 'irrevocably discarded' in all three cases. It was indeed irrevocably discarded in the Russian Revolution, but in the British and French cases it was, when all the upheavals had come to an end, only 'relegated to a significantly lesser position'.

This suggests that we may distinguish two types of revolution. In the first type, which could be called *Russian*, the strong Crowe condition is satisfied, and 'some previously existing entity . . . is overthrown and irrevocably discarded'. In the second type, which could be called *Franco-British*, the 'previously existing entity' persists, but experiences a considerable loss of importance. If we now apply this distinction to the three scientific revolutions mentioned earlier, it is at once clear that the Copernican and the chemical revolution were Russian revolutions, while the Einsteinian revolution was Franco-British. After the triumph of Newton, Aristotelian mechanics was indeed 'irrevocably discarded'. It was no longer taught to budding scientists, and appeared in the university curriculum, if at all, only in history of science courses. The situation is quite different for Newtonian mechanics, for, after the triumph of Einstein, Newtonian mechanics is still being taught, and is still applied in a wide class of cases. On the other hand, after the success of relativistic mechanics, Newtonian mechanics has undoubtedly suffered a considerable loss of importance.

The phrase 'a considerable loss of importance' has been chosen so as to apply to both political and scientific revolutions. However, it obviously has a rather different meaning in the two cases. In the British and French revolutions, the monarchy's 'considerable loss of importance' consisted in a loss of power. After 1688, Britain still has a king and the king still had some power, but his importance was not what it had been in the 1630s. The real power was now in the hands of Parliament rather than the Monarchy. In the Einsteinian revolution, Newtonian mechanics certainly lost some of its former importance. It was no longer the fundamental theory of physics, but merely a special case of a deeper theory. Limits, which had not existed before, were set on the domain of its applicability, and it was recognized that, outside these limits, Newtonian theory gave wrong (or at least inaccurate) results.

These considerations suggest the following approach to revolutions in mathematics. In science, both Russian and Franco-British revolutions occur. In mathematics, revolutions do occur but they are always of Franco-British type. An innovation in mathematics (or a branch of mathematics) may be said to be a revolution if two conditions are satisfied. First of all, the innovation should change mathematics (or the branch of mathematics) in a profound and far-reaching way. Secondly, the relevant older parts of mathematics, while persisting, should undergo a considerable loss of importance.

My aim here, however, is not to resolve the Crowe–Dauben debate, but to show that the debate is an interesting and important one, and that it is therefore worth examining in greater detail the question of whether there are revolutions in mathematics. This question is indeed central to this book, whose general plan is described in the next section.

PLAN OF THE BOOK

The book begins with the three papers which started the modern debate on whether there are revolutions in mathematics. Crowe (1975) is Chapter 1, Mehrtens (1976) is Chapter 2, and Dauben (1984) Chapter 4. The three original participants have each written a further chapter describing developments and changes in their views. The original papers by Mehrtens and Dauben are each followed by an 'Appendix (1992)'. In his appendix, Dauben provides two further examples of revolutions in mathematics, namely Cauchy's revolution in rigour and Robinson's non-standard analysis. Crowe's new chapter is the final one, and so constitutes an afterword to the whole book. Since his original short paper has provoked so much discussion, it is only fair that he should be allowed the last word!

In the rest of the book, contributors who have an expert knowledge of particular episodes in the history of mathematics have been asked to give an account of these episodes, in particular to discuss whether they should be considered as revolutions in mathematics. I have arranged these specially commissioned papers, which constitute Chapters 6 to 14, in roughly chronological order of subject. This sequence begins with Paolo Mancosu's detailed analysis of Descartes's *Géométrie*. The question is whether Descartes's introduction of analytic geometry constituted a revolution in mathematics.

Mancosu has some doubts as to whether this was the case. The next two contributors (Emily Grosholz ad Giulio Giorello) deal with the development of the infinitesimal calculus; both affirm that this was indeed a revolution in mathematics. Grosholz in Chapter 7 discusses Leibniz's contribution, while Giorello in Chapter 8 focuses mainly on Newton and some of his British successors.

After the infinitesimal calculus, the next major candidate for a revolution in mathematics is the discovery of non-Euclidean geometry, and this is considered in Chapters 9 and 10. In Chapter 9, Yuxin Zheng deals with the earlier period of Gauss, Bolyai, and Lobachevsky, while Riemann is considered by Luciano Boi in Chapter 10. Boi does not deal exclusively with non-Euclidean geometry, but widens the discussion into a consideration of the change in the geometrical vision of space in the nineteenth century.

In Chapter 11, Caroline Dunmore expounds her concept of meta-level revolutions in mathematics, and illustrates it by a number of historical examples. One of these is Hamilton's invention of non-commutative algebra, and this introduces a new theme—that of changes in algebra in the nineteenth century. This theme, along with others, is developed by Jeremy Gray in Chapter 12. Gray argues that the nineteenth century brought about a revolutionary change in the character of the objects studied in mathematics. To illustrate this, he considers examples drawn from various branches of mathematics, including algebraic number theory and the development of the theory of ideals.

It could be argued that the period 1879 to 1931 (from Frege to Gödel) saw a profound revolution in the foundations of mathematics. This led to the emergence of a new paradigm consisting of axiomatic set theory and first order logic, and this has provided the framework within which mathematics has been carried out during the last sixty years. Dauben's discussion of Cantor in Chapter 4 deals with one aspect of this revolution, and Chapters 13 and 14 deal with other aspects. In a sense Herbert Breger in Chapter 13 carries on from where Dauben stops. Breger deals with the emergence of axiomatic set theory which rehabilitated Cantor's approach after it had been shaken by the discovery of the paradoxes. However, Breger tackles the question in a subtle and indirect fashion. Instead of analysing directly the work of Zermelo, Fraenkel, and others, he focuses on the now largely forgotten attempt by Paul Finsler in 1926 to develop a theory of sets. Breger argues persuasively that Finsler's ideas were rejected in favour of those of Zermelo, Fraenkel, Hilbert, and so on, because Finsler's theory involved an old-fashioned nineteenth-century style of thought which was disappearing with the rise of the new paradigm. This new paradigm presents set theory as an axiomatic theory developed within first-order logic. My own contribution in Chapter 14 deals not with set theory, but with logic. I argue that there was a Fregean revolution in logic analogous to the Copernican revolution in astronomy and physics.

Copernicus began the Copernican revolution, but it was brought to a conclusion only by Newton, after the work of many intermediate figures such as Kepler and Galileo. Similarly, Frege began a revolution in logic which was brought to a conclusion only by Tarski and Gödel, after the work of Peano, Russell, Hilbert, and others.

This brief sketch of the contents of the book shows that it deals with most of the major episodes in mathematical history from Descartes in the 1630s to Robinson in the 1960s. There is also some discussion (by Dauben and Dunmore) of ancient Greek mathematics. The references for the various chapters have been collected together in a bibliography at the end of the book, and this gives a very comprehensive selection of recent and classic books and papers on the history of mathematics.

This collection is, however, by no means exclusively a contribution to the history of mathematics. The contributors describe important episodes in the history of mathematics, but they also raise the philosophical question of whether these episodes can correctly be described as constituting revolutions. What is interesting here is that each contributor has a different theoretical perspective on the question of revolutions in mathematics. This is an aspect of the book which I discuss in the next section.

A VARIETY OF THEORETICAL PERSPECTIVES

Each of the twelve contributors to the present volume discusses revolutions in mathematics, and it might therefore be feared that there could be considerable overlap and repetition in what is said. Surprisingly, almost the opposite holds, for each contributor appears to have a different view of mathematical revolutions, and to analyse the notion using different concepts. So the book as a whole provides a rich and diverse set of theoretical perspectives which can be used for thinking about the key notion of a revolution in mathematics. I will now try to give a brief sketch of this variety of theoretical perspectives.

I have already described the Crowe–Dauben debate, but let me begin here with another interesting idea of Dauben's, which has not so far been mentioned. Dauben emphasizes an important feature of revolutions in mathematics (as in other areas), namely resistance to change on the part of the counter-revolutionary party. Dauben puts the point as follows:

> ... resistance to new discoveries may be taken as a strong measure of their revolutionary quality ... Perhaps there is no better indication of the revolutionary quality of a new advance in mathematics than the extent to which it meets with opposition. The revolution, then, consists as much in overcoming establishment opposition as it does in the visionary quality of the new ideas themselves. (Dauben 1984, pp. 63–4)

This resistance is certainly to be found in the early reviews of Frege's work which I analyse in Section 14.5 of Chapter 14. In Chapter 8, Giorello provides another striking example in Section 8.5, appropriately entitled 'Berkeley's "counter-revolution"'.

Giorello, in his analysis of mathematical revolutions, uses a concept introduced by the mathematician René Thom, originally in the context of political revolutions, particularly the French Revolution. This is the concept of a change in the 'paradigm of legitimacy'. Giorello develops this idea within the framework of a most interesting comparison between the Great Rebellion in England from Charles I to the Glorious Revolution, and the development of the infinitesimal calculus, which indeed occurred in more or less the same historical period. In the political upheavals, the paradigm of legitimacy concerned the legitimacy of monarchical rule—initially accepted, at least in words, by the parliamentarians who were subverting it. For the calculus, the paradigm of legitimacy was the 'geometrical rigour of the Ancients' (Euclid and, particularly, Archimedes), which again was accepted, at least in words, by the revolutionary mathematicians who in their practice were subverting it.

In the first section of Chapter 9, Zheng gives an astute analysis of the debate on revolutions in mathematics, but then in Section 9.3 he examines the problem further using some ideas drawn from Chinese philosophy: the theory of types of mathematical truth and 'the harmonious principle of the counter-way thinking'.

Gray differs from some of the other contributors in not considering one or more specific examples, such as the infinitesimal calculus or non-Euclidean geometry, but in surveying nineteenth-century mathematics in more general terms. This leads him to argue for a revolution in mathematical ontology. As he says, 'although the objects of study were still superficially the same (numbers, curves, and so forth), the way they were regarded was entirely transformed' (Chapter 12, p. 227).

So far all the contributors I have considered (except, of course, Crowe) strongly favour the claim that there have been revolutions in mathematics. Yet, apart from the occasional use of the word 'paradigm', none of them gives a particularly Kuhnian analysis of revolutions. Perhaps my own chapter on Frege (Chapter 14) is the most Kuhnian in its approach to a revolution in mathematics. The Fregean revolution in logic is seen as a change from the Aristotelian paradigm, in which the theory of the syllogism is the central core of logic, to the Fregean paradigm, in which propositional calculus and first-order predicate calculus are at the centre. However, I too differ in some respects from Kuhn's approach. To begin with I reject the idea that paradigms are incommensurable, since it seems to me quite easy to compare them in this case (and,. indeed, in other cases in both science and mathematics). Secondly, I suggest that we should use, for the analysis of revolutions in science and mathematics, both Kuhn's concept of paradigm and a modified version of

Lakatos's concept of research programme. The idea is that, in a revolution, there is the introduction of new research programmes, which, although they may initially be pursued by only a few people (or even just one person), lead eventually to the emergence of a new paradigm which is accepted by the community as a whole. This is illustrated by considering the revolutionary research programmes of Frege and Peano.

Mancosu, in connection with the question of the revolutionary nature of Descartes' *Géométrie*, introduces in Chapter 6 two interesting theoretical considerations. First of all he discusses not just post-Kuhnian but also pre-Kuhnian debates about the revolutions in mathematics (see Sections 6.7 and 6.8), and in fact shows that Descartes's work on geometry was widely referred to as a revolution in mathematics in both the eighteenth and nineteenth centuries. Secondly, Mancosu considers Bernard Cohen's important contributions to the question of revolutions in science and mathematics. Cohen (1985) argued that Descartes's work on geometry was a revolution in mathematics, but Mancosu uses Cohen's own four criteria for a revolution to cast doubt on this thesis, and concludes by striking a sceptical note as to whether Descartes's *Géométrie* is a revolutionary event in the history of mathematics. Interestingly, Dunmore, starting from a quite different analysis of revolutions in mathematics, also reaches the conclusion that Descartes's introduction of coordinate geometry did not constitute a revolution (see Chapter 11, Section 11.6). Despite his doubts about the revolutionary status of Descartes's work, Mancosu is sympathetic to the claim that the whole period from Viète to Leibniz could be taken as constituting a revolution in mathematics.

Grosholz, in her discussion of Leibniz's mathematical work in Chapter 7, introduces another new theoretical consideration. The point she stresses is that significant, sudden increases of knowledge can result from the bringing together of previously unrelated domains. Thus, as she argues, Leibniz came to the calculus through his synthesis of geometry, algebra, and number theory, and then further extended it by connecting these areas to mechanics as well. Grosholz further argues that reduction of one domain to another is less fruitful than a partial unification in which the domains 'share some of their structure in the service of problem-solving, but none the less retain their distinctive character' (Chapter 7, p. 118). I argue in Chapter 14 that Grosholz's ideas are strongly supported by the case of the Fregean revolution in logic, for here the remarkable advances of Frege and Peano arose from their putting together the previously unconnected domains of logic and arithmetic (see Section 14.4).

The theory of revolutions in mathematics which Dunmore presents in Chapter 11 is, in a sense, a development of Crowe's. She accepts Crowe's condition for revolutions, namely 'that some previously existing entity . . . be overthrown and irrevocably discarded', and accordingly concludes quite correctly that there cannot be revolutions in mathematics in this strong (or

Russian) sense. She then, however, draws attention to the following interesting qualification which Crowe makes to his Law 10:

Also the stress in Law 10 on the proposition 'in' is crucial, for, as a number of the earlier laws make clear, revolutions may occur in mathematical nomenclature, symbolism, metamathematics (e.g. the metaphysics of mathematics), . . . and perhaps even in the historiography of mathematics. (Crowe 1975, p. 19).

Dunmore picks up the point here about metamathematics, and suggests that while revolutions in the strong (or Russian) sense do not occur in mathematics at the object level, they do occur at the meta-level. In fact, for her a revolution in mathematics occurs if and only if a meta-level doctrine about mathematics is 'overthrown and irrevocably discarded', and is replaced by some new view. For example, before the discovery of non-Euclidean geometry, virtually all mathematicians held the meta-level doctrine that there was only one possible geometry, namely Euclidean geometry, that the truth of this geometry could be established *a priori*, and that this geometry was the correct geometry of space. After the discovery of non-Euclidean geometry, this doctrine was 'overthrown and irrevocably discarded' to be replaced by the view that a number of different geometries were possible. Because of this change at meta-level, the discovery of non-Euclidean geometry is for Dunmore a revolution in mathematics.

One of the interesting features of Dunmore's chapter is her list of episodes which are sometimes thought to be revolutions in mathematics but which she does not regard as such. Most strikingly, she denies that the development of the infinitesimal calculus was a revolution in mathematics. This is quite correct given her meta-level criterion, since the introduction of the calculus did not cause any meta-level doctrines to be 'irrevocably discarded'. On the other hand, this view contrasts strongly with that of Dauben, who argues that the development of calculus was a revolution because it brought about far-reaching changes in mathematics, and caused much earlier mathematics to lose its former importance.

Dunmore does consider the introduction of negative integers to be a revolution in mathematics because, as she shows, it led to the rejection of the earlier meta-level doctrine that negative integers were impossible. It could be objected, however, in terms of Dauben's criteria, that the introduction of negative numbers did not change mathematics sufficiently to be a truly revolutionary event. Perhaps this difficulty could be overcome by speaking of 'micro-revolutions' or 'revolutions restricted to a small area of mathematics'.

Breger in Chapter 13 makes use in his analysis of the concept of 'style of thought' which was introduced by Fleck (1935). Breger considers the very interesting case of the theory of sets which was proposed by Paul Finsler in 1926, but then rejected as inconsistent by the community from 1928 on. Breger's thesis is that the theory was all right relative to Finsler's extreme

Platonist presuppositions, but not in terms of the new style of thought which was emerging. Breger sees Hilbert as perhaps the leading advocate of the new style of thought, and hence as a revolutionary rather than a conservative. As Breger himself puts it:

We are used to the common doctrine according to which Hilbert was the great conservative defeating revolution. But having come to power, revolutionaries tend to present themselves as legitimate heirs of tradition. In fact, Hilbert was the distinguished proponent of the new paradigm; he saved the old formulas, but gave everything a new meaning. To be more precise: Hilbert stripped mathematics of any meaning at all—with the exception of the small domain of finite propositions, mathematics now consists of 'formulas which mean nothing' (Hilbert 1925, p. 176). I tend to the interpretation that *this* was the real revolution (in a Kuhnian sense), because Hilbert rejected the most fundamental ideas concerning mathematical truth as well as legitimation and existence of objects which had been self-evident for more than 2000 years. . . . True, he sweetened the new paradigm by the programme of proving the consistency at a later date. But this was only a programme, and, in fact, failed soon. (Chapter 13, p. 253)

This is a most interesting re-evaluation of Hilbert, which, incidentally, ties in neatly with Giorello's ideas about legitimacy, and with Gray's discussion of a nineteenth-century revolution in mathematical ontology. There is, however, yet more in Breger's chapter, which, in the final section (Section 13.6) broadens out to consider parallel changes in styles of thought in mathematics, physics, and the arts during the period 1870–1930.

I now turn to the contributions of Boi and Mehrtens. These two authors are perhaps the most sceptical (apart from Crowe) about the value of the concept of revolution for the study of the history of mathematics. However—and here is the interesting point—this scepticism arises from positions which are diametrically opposed. Mehrtens favours a sociological approach to the history of mathematics, while Boi advocates an internalist history of mathematics and is very critical of sociological explanations. Let us start with Boi's views, which are set out in Chapter 10. They will subsequently be compared with those of Mehrtens.

Boi rejects the use not just of the concept of revolution, but of all sociological concepts. As he says:

First, I would like to show that the 'nature' of mathematical knowledge cannot be described, and even less explained, using sociological or purely historiographical categories such as 'revolution', for essential reasons which I will try to argue. (Chapter 10, p. 190)

Moreover, later in the chapter, he explicitly rejects use of the concepts of 'scientific community' and 'paradigm': 'in mathematics we do not encounter sociological categories such as "scientific community" and "paradigm"'

(Chapter 10, p. 203). Boi illustrates his position by an example drawn from the work of Riemann:

> No sociological or extra-mathematical reasons could help in understanding the nature of mathematical knowledge and the intrinsic reasons for its development and changes. Can any reason other than mathematical be found to explain the qualitative (geometric) approach developed by Riemann in his study of the analytic functions of one complex variable? Is it not much more fruitful for the mathematical historian and philosopher, and also for the mathematician himself, to analyse the specific contents and the general conceptions which allowed the great German mathematician to state, develop, and justify such a new theory? (Chapter 10, p. 197)

An opposition to the application of sociology to mathematics is Boi's negative thesis, but this is complemented by a positive account according to which mathematics develops through a subtle internal dialectic. Boi elaborates this idea in the context of geometry in the nineteenth century, making use of some of the concepts of the important Parisian school of philosophers of mathematics, which includes René Thom, Jean Petitot, and Jean-Michel Salanskis.

Let us now turn to Mehrtens (1976), whose paper is reprinted as Chapter 2. Mehrtens discusses the question of whether Kuhn's theories can be applied to mathematics, and he concludes that the concept of revolution is not a very useful one:

> I have rejected the concepts 'revolution' and 'crisis' in spite of the existence of phenomena that might bear these names. The reason was that these concepts cannot be formed into forceful tools for historical inquiries. (Chapter 2, p. 29)

This rejection of the concept of revolution is not motivated, as in the case of Boi, by a general opposition to sociological concepts. On the contrary, while Mehrtens rejects Kuhn's specific model in the case of mathematics, he thinks that many of Kuhn's sociological concepts, in particular the concept of scientific community, are very useful for the analysis of the history of mathematics. As he says:

> The general pattern of T. Kuhn's theory of the structure of scientific revolutions seems to be not applicable to mathematics. But many of Kuhn's concepts remain valuable for the historiography of science even if the basic pattern of the theory is rejected. The concepts centring around the sociology of groups of scholars are of high explanatory power and—in my opinion—supply key concepts for the historiography of mathematics. (Chapter 2, p. 35)

Mehrtens illustrates this approach with a number of examples. In Section 2.3 he gives a sociological explanation of why there was a turn to pure mathematics in nineteenth-century Germany. It is interesting to connect this with Boi's remarks about the impossibility of giving a sociological explanation of Riemann's work. Mehrtens' sociological theory does explain some features