of Riemann's mathematics, more specifically the fact that Riemann, in common with other German mathematicians of the time, adopted a more rigorous, abstract style of mathematics in which mathematical theories were less closely connected with applications. Now, of course, such an explanation does not account for the specific details of the approach which Riemann adopted in his treatment of analytic functions of one complex variable. Boi is undoubtedly correct that an internal analysis of the problem situation in mathematics is necessary here, but Mehrtens would perhaps not disagree. Indeed, Mehrtens says explicitly: 'I am not trying to explain everything about mathematics in social or sociological terms' (Chapter 2, p. 30). The sociological approach might yield part of the truth, even if not the whole truth.

In his 'Appendix (1992)', Mehrtens shows perhaps a little more sympathy for the application of the concept of revolution to mathematics, but even here he seems to prefer the concept of epistemological rupture.

This brings me in conclusion to Chapter 15 in which Crowe, in his 'Afterword (1992)', gives a fascinating account of the evolution of his own views on the nature and development of mathematics. I will not attempt to summarize what Crowe says here. Since he started the whole debate, it is only fair, as I have already said, that he should be allowed the last word. I will only remark that, although Crowe's 1975 paper is short, his afterword shows how much study and reflection preceded its composition, and so makes it less surprising that this paper should have given rise to such an interesting debate.

1

Ten 'laws' concerning patterns of change in the history of mathematics (1975)*

MICHAEL CROWE

Approximately a decade ago G. Buchdahl (1965, p. 69) stated that 'we are finding ourselves at present in a revolution in the historiography of science'. No one has announced a revolution in the historiography of mathematics, even though the number of excellent historical studies of mathematics has increased of late. Whereas the present state of the historiography of mathematics differs little (except in quality) from what it was nearly a century ago when Moritz Cantor published the first volume of his *Vorlesungen*, the historiography of science has undergone far-reaching changes which are most explicitly set out in the writings of such authors as J. Agassi and T. Kuhn (whose books Buchdahl was reviewing) as well as in the publications of N. R. Hanson, K. Popper, and S. Toulmin.

In the historiography of mathematics, no comparable group of authors seems to have emerged. Moreover, most historians of mathematics acquainted with the new historiography of science have been sceptical as to whether the insights embodied therein can be applied in any direct way to the historiography of mathematics. The writings of these five authors do not facilitate such application, for their works contain few references to, and generally have been written without detailed consideration of, the history of mathematics. Moreover, the major differences between the conceptual structures of mathematics and of science make it questionable whether their histories should exhibit similar patterns of development.

The situation may, however, be changing. The late Imre Lakatos's *Proofs and refutations* (1963–4) and Raymond Wilder's *Evolution of mathematical concepts* (1968) are examples of works that may pave the way to a new historiography of mathematics. Moreover, the October 1974 History of Science Society meeting included a session which explored various questions in the historiography of mathematics, especially whether the ideas in T. Kuhn's *The structure of scientific revolutions* could fruitfully be applied in the history of mathematics. Much may be at stake here, for the revolution in the historiography of science brought with it not only an increased accessibility for history of science writing and teaching, as well as raising thorny questions in the philosophy of science, but also produced new and more sophisticated standards in the historical study of science.

The present paper has been written to stimulate discussion of the historiography of mathematics by asserting ten 'laws' concerning change in mathematics, which touch on issues that will have to be considered if a new historiography of mathematics is to develop. R. L. Wilder, in his interesting *Evolution of mathematical concepts* (1968, pp. 207–9), has suggested and evidenced ten 'laws' which he believes 'worthy of study with a view to their justification or refutation'. The following ten 'laws', suggested in the same spirit, differ in their origin from Professor Wilder's chiefly in that they have arisen from my efforts to apply the insights of the new historiography to mathematics, whereas Professor Wilder draws upon anthropological and sociological researches. More substantive research in the history of mathematics than can be cited in the present format has provided me with the differing measures of confidence with which these 'laws' have been set down; for some of this evidence see Crowe (1967*a*), although that book was written long before I had formulated many of the ideas contained in the present paper.

1. *New mathematical concepts frequently come forth not at the bidding, but against the efforts, at times strenuous efforts, of the mathematicians who create them.*

Consider Saccheri, whose valiant efforts to prove that no geometry but Euclid's was possible resulted in the first non-Euclidean system. Or consider Hamilton, who sought for a three-dimensional commutative, associative, and distributive division algebra, but who in a long and stubborn pursuit of this goal invented the four-dimensional quaternions.

2. *Many new mathematical concepts, even though logically acceptable, meet forceful resistance after their appearance and achieve acceptance only after an extended period of time.*

The discovery of incommensurable segments by Hippasus led, we are told, to his banishment and to death by shipwreck. More than legends tell us that numbers representing incommensurable ratios were fully accepted only 2200 years later. Invective was a major part of the response of the mathematical community between 1543 and the 1830s to the square roots of negative quantities. Such terms as 'sophistic' (Cardano), 'nonsense' (Napier), 'inexplicable' (Girard), 'imaginary' (Descartes), 'incomprehensible' (Huygens), and 'impossible' (many authors) remind us of the type of welcome accorded these new entities.

3. *Although the demands of logic, consistency, and rigour have at times urged*

the rejection of some concepts now accepted, the usefulness of these concepts has repeatedly forced mathematicians to accept and to tolerate them, even in the face of strong feelings of discomfort.

As Felix Klein suggested, 'imaginary numbers made their own way . . . without the approval, and even against the desires of, individual mathematicians, and obtained wider circulation only gradually and to the extent to which they showed themselves useful' (1939, p. 56). For more than a century mathematicians accepted imaginary numbers without a formal justification for them because they proved useful in saving the fundamental theorem of algebra and in permitting the solution of various scientific problems. Or consider the case of our modern scalar and vector products, which arose not on principle or from conscious desire, but rather from the practice among quaternionists of using separately the scalar and vector parts of the full quaternion product.

4. *The rigour that permeates the textbook presentations of many areas of mathematics was frequently a late acquisition in the historical development of those areas, and was frequently forced upon, rather than actively sought by, the pioneers in those fields.*

As J. Grabiner has recently shown, the early development of rigorous approaches in analysis was in large measure the result of bothersome questions raised by impatient students, the penetrating critique of an aggrieved theologian (Berkeley), the embarrassment emerging from comparisons with a (then) accepted model of rigour (Euclid), and the need for generalization (Grabiner 1974). The brilliant study of the history of the Euler conjecture for polyhedra by I. Lakatos (1963–4) showed no less clearly the elusiveness of the search for rigour. And on a more general level, Morris Kline (1974, p. 69) has remarked:

> It is safe to say that no proof given at least up to 1800 in any area of mathematics, except possibly in the theory of numbers, would be regarded as satisfactory by the standards of 1900. The standards of 1900 are not acceptable today.

5. *The 'knowledge' possessed by mathematicians concerning mathematics at any point in time is multilayered. A 'metaphysics' of mathematics, frequently invisible to the mathematician yet expressed in his writings and teaching in ways more subtle than simple declarative sentences, has existed and can be uncovered in historical research or becomes apparent in mathematical controversy.*

The existence of this 'metaphysics' is suggested by the terms mentioned above which were applied to complex number. Or consider Leibniz's 1702 (Klein 1939, p. 56) remark that 'Imaginaries are a fine and wonderful refuge of the divine spirit, almost an amphibian between being and nonbeing.' As late as 1887, Eugen Dühring (1887, p. 547) criticized mathematicians for the use of the imaginary numbers, 'this darling of complex mysticism'. If 'metaphysics' seems too strong a word here, let 'intuitive knowledge' be substituted.

6. *The fame of the creator of a new mathematical concept has a powerful, almost a controlling, role in the acceptance of that mathematical concept, at least if the new concept breaks with tradition.*

Compare the reception accorded Hamilton's *Lectures on quaternions* (1853) with that of Grassmann's *Ausdehnungslehre* (1844). Both are among the classics of mathematics, yet the work of the former author, who was already famous for empirically confirmed results, was greeted with lavish praise in reviews by authors who had not read his book, whereas the book of Grassmann, an almost unpublished high-school teacher, received but one review (by its author!) and found, before it was used for waste paper in the early 1860s, only a handful of readers. Or consider the fate of Lobachevsky and Bolyai, whose publications remained as unknown as their authors until, thirty years after their publications, some posthumously published letters of the illustrious Gauss led mathematicians to take an interest in non-Euclidean geometry.

7. *New mathematical creations frequently arise within, and depend in the mind of their creators upon, contexts far larger than the preserved content of these creations; yet these contexts, for all their original importance, may impede or even prohibit the acceptance of the creations until they are removed by the mathematical community.*

Gifts arrive in wrappings which must be torn asunder before the gift itself may be used or even seen. The algebraic gifts of Hamilton and Grassmann arrived in philosophic wrappings which at first obscured the view of the mathematical community, and then were unceremoniously discarded. Yet these wrappings were a necessary condition in the minds of Hamilton and Grassmann for their own acceptance of the gifts of their fertile imaginations, and were scarcely seen by them as distinguishable from the gifts themselves. The fates of Berkeley and of Boole were not dissimilar.

8. *Multiple independent discoveries of mathematical concepts are the rule, not the exception.*

A striking illustration comes from the history of attempts to justify complex numbers, where no less than eight mathematicians are cited as discoverers of the two main methods. The multiple discoverers of analytic geometry, the calculus, and non-Euclidean geometry are well known. This law is partially explained by Laws 2 and 7.

9. *Mathematicians have always possessed a vast repertoire of techniques for dissolving or avoiding the problems produced by apparent logical contradictions, and thereby preventing crises in mathematics.*

Kuhn's *The structure of scientific revolutions* exhibits many of the strategies which scientists have used to prevent 'anomalies' from becoming crisis-producing contradictions or refutations. That the mathematician's cabinet is no less richly stored was amply illustrated by Lakatos's *Proofs and refutations*, wherein 'monster-barring' is but the most colourfully named technique. Or, to turn to an early period of mathematics, was the discovery of the incommensurable a discovery that the irrational magnitude is not part of arithmetic, or that algebra was not a fit branch of mathematics, or that Hippasus was not a fit mathematician?

10. *Revolutions never occur in mathematics.*

Surprising as this law may seem to some, it is the conclusion of mathematicians as widely separated in time as J. B. Fourier, H. Hankel, and C. Truesdell. As Fourier wrote in his 1822 *Théorie analytique de la chaleur*, 'this difficult science [mathematics] is formed slowly, but it preserves every principle it has once acquired; it grows and strengthens itself in the midst of many variations and errors of the human mind' (Fourier 1822, p. 7). Hankel wrote no less forcefully when in 1869 he stated, 'In most sciences one generation tears down what another has built . . . In mathematics alone each generation builds a new storey to the old structure' (Moritz 1942, p. 14). And more recently Truesdell (1968, Foreword), who, like Hankel, wrote with a detailed knowledge of both mathematics and its history, stated that 'while "imagination, fancy, and invention" are the soul of mathematical research, in mathematics there has never yet been a revolution'. Yet these quotations, however impressive their authors, cannot stand alone and without qualification. For 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. I have argued more fully elsewhere (Crowe 1967*b*) that a number of the most important developments in science, though frequently called 'revolutionary', lack this fundamental characteristic. My argument was based on a distinction between 'transformational' or revolutionary discoveries (astronomy 'transformed' from Ptolemaic to Copernican), and 'formational' discoveries (wherein new areas are 'formed' or created without the overthrow of previous doctrines, e.g. energy conservation or spectroscopy). It is, I believe, the latter process rather than the former that occurs in the history of mathematics. For example, Euclid was not deposed by, but reigns along with, the various non-Euclidean geometries. Also, the stress in Law 10 on the preposition '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), methodology (e.g. standards of rigour), and perhaps even in the historiography of mathematics.

ACKNOWLEDGEMENTS

This paper initially arose from discussions with various graduate students at Notre Dame, especially Jean Horiszny and Luis Laita. A preliminary version