A History of Mathematics

From Mesopotamia to Modernity

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Introduction

Why this book?

[M. de Montmort] was working for some time on the *History of Geometry*. Every Science, every Art, should have its own. It gives great pleasure, which is also instructive, to see the path which the human spirit has taken, and (to speak geometrically) this kind of progression, whose intervals are at first extremely long, and afterwards naturally proceed by becoming always shorter. (Fontenelle 1969, p. 77)

With so many histories of mathematics already on the shelves, to undertake to write another calls for some justification. Montmort, the first modern mathematician to think of such a project (even if he never succeeded in writing it) had a clear Enlightenment aim: to display the accelerating progress of the human spirit through its discoveries. This idea—that history is the record of a progress through successive less enlightened ages up to the present—is usually called 'Whig history' in Anglo-Saxon countries, and is not well thought of. Nevertheless, in the eighteenth century, even if one despaired of human progress in general, the sciences seemed to present a good case for such a history, and the tradition has survived longer there than elsewhere. The first true historian of mathematics, Jean Étienne Montucla, underlined the point by contrasting the history of mathematical discovery with that which we more usually read:

Our libraries are overloaded with lengthy narratives of sieges, of battles, of revolutions. How many of our heroes are only famous for the bloodstains which they have left in their path! . . . How few are those who have thought of presenting the picture of the progress of invention, or to follow the human spirit in its progress and development. Would such a picture be less interesting than one devoted to the bloody scenes which are endlessly produced by the ambition and the wickedness of men?. . .

It is these motives, and a taste for mathematics and learning combined, which have inspired me many years ago in my retreat . . . to the enterprise which I have now carried out. (Montucla 1758, p. i–ii)

Montucla was writing for an audience of scholars—a small one, since they had to understand the mathematics, and not many did. However, the book on which he worked so hard was justly admired. The period covered may have been long, but there was a storyline: to simplify, the difficulties which we find in the work of the Greeks have been eased by the happy genius of Descartes, and this is why progress is now so much more rapid. Later authors were more cautious if no less ambitious, the major work being the massive four-volume history of Moritz Cantor (late nineteenth century, reprinted as (1965)). Since then, the audience has changed in an important way. A key document in marking the change is a letter from Simone Weil (sister of a noted number theorist, among much else) written in 1932. She was then an inexperienced philosophy teacher with extreme-left sympathies, and she allowed them to influence the way in which she taught.

Dear Comrade,

As a reply to the Inquiry you have undertaken concerning the historical method of teaching science, I can only tell you about an experiment I made this year with my class. My pupils, like most other pupils, regarded the various sciences as

compilations of cut-and-dried knowledge, arranged in the manner indicated by the textbooks. They had *no idea* either of the connection between the sciences, or of the methods by which they were created . . .

I explained to them that the sciences were not ready-made knowledge set forth in textbooks for the use of the ignorant, but knowledge acquired in the course of the ages by men who employed methods entirely different from those used to expound them in textbooks . . . I gave them a rapid sketch of the development of mathematics, taking as central theme the duality: continuous–discontinuous, and describing it as the attempt to deal with the continuous by means of the discontinuous, measurement itself being the first step. (Weil 1986, p. 13)

In the short term, the experiment was a failure; most of her pupils failed their baccalaureate and she was sacked. In the long term, her point—that science students gain from seeing their study not in terms of textbook recipes, but in its historical context—has been freed of its Marxist associations and has become an academic commonplace. Although Weil would certainly not welcome it, the general agreement that the addition of a historical component to the course will produce a less limited (and so more marketable) science graduate owes something to her original perception.

It is some such agreement which has led to the proliferation of university courses in the history of science, and of the history of mathematics in particular. Their audience will rarely be students of history; although they are no longer confined to battles and sieges, the origins of the calculus are still too hard for them. Students of mathematics, by contrast, may find that a little history will serve them as light relief from the rigours of algebra. They may gain extra credit for showing such humanist inclinations, or they may even be required to do so. A rapid search of the Internet will show a considerable number of such courses, often taught by active researchers in the field. While one is still ideally writing for the general reader (are you out there?), it is in the first place to students who find themselves on such courses, whether from choice or necessity, that this book is addressed.

On texts, and on history

Insofar as it stands in the service of life, history stands in the service of an unhistorical power, and, thus subordinate, it can and should never become a pure science such as, for instance, mathematics is . . .

History pertains to the living man in three respects; it pertains to him as a being who acts and strives, as a being who preserves and reveres, as a being who suffers and seeks deliverance. (Nietzsche 1983, p. 67)

American history practical math
Studyin hard and tryin to pass. (Berry 1957)

Chuck Berry's words seem to apply more to today's student of history, mathematics, or indeed the history of mathematics, than Nietzsche's; history pertains to her or him as a being who goes to lectures and takes exams. And naturally where there is a course, the publisher (who also has a living to make) appears on the scene to see if a textbook can be produced and marketed. Probably, the first history designed for use in teaching, and in many ways the best, was Dirk Struik's admirably short text (1986) (288pp., paperback); it is probably no accident that Struik the pioneer held to a more mainstream version of Simone Weil's far-left politics. This was followed by John Fauvel and Jeremy Gray's sourcebook (1987), produced together with a series of short texts from the Open University. This performed the most important function, stressed in the British National Curriculum for history, of foregrounding primary material and enabling students to see

for themselves just how 'different' the mathematics of others might appear.[1] Since then, broadly, the textbooks have become longer, heavier, and more expensive. They certainly sell well, they have been produced by professional historians of mathematics, and they are exhaustive in their coverage.[2] What then is lacking? To explain this requires some thought about what 'History' is, and what we would like to learn from it. From this, hopefully, the aims which set this book off from its competitors will emerge.

E. H. Carr devoted a short classic to the subject (2001), which is strongly recommended as a preliminary to thinking about the history of mathematics, or of anything else. In this, he begins by making a measured but nonetheless decisive critique of the idea that history is simply the amassing of something called 'facts' in the appropriate order. Telling the story of the brilliant Lord Acton, who never wrote any history, he comments:

> What had gone wrong was the belief in this untiring and unending accumulation of hard facts as the foundation of history, the belief that facts speak for themselves and that we cannot have too many facts, a belief at that time so unquestioning that few historians then thought it necessary—and some still think it unnecessary today—to ask themselves the question 'What is history?' (Carr 2001, p. 10)

If we accept for the moment Carr's dichotomy between historians who ask the question and those who consider that the accumulation of facts is sufficient, then my contention would be that most specialist or local histories of mathematics do ask the question; and that the long, general and all-encompassing texts which the student is more likely to see do not. The works of Fowler (1999) and Knorr (1975) on the Greeks, of Youschkevitch (1976), Rashed (1994), and Berggren (1986) on Islam, the collections of essays by Jens Høyrup (1994) and Henk Bos (1991) and many others in different ways are concerned with raising questions and arguing cases. The case of the Greeks is particularly interesting, since there are so few 'hard' facts to go on. As a result, a number of handy speculations have acquired the status of facts; and this in itself may serve as a warning. For example, it is usually stated that Eudoxus of Cnidus invented the theory of proportions in Euclid's book V. There is evidence for this, but it is rather slender. Fowler is suspicious, and Knorr more accepting, but both, as specialists, necessarily argue about its status. In all *general* histories, it has acquired the status of a fact, because (in Carr's terms) if history is about facts, you must have a clear line which separates them from non-facts, and speculations, reconstructions, and arguments disrupt the smoothness of the narrative.

As a result, the student is not, I would contend, being offered *history* in Carr's sense; the distinguished authors of these 750-page texts are writing (whether from choice or the demands of the market) in the Acton mode, even though in their own researches their approach is quite different. Indeed, in this millennium, they can no longer write like Montucla of an uninterrupted progress from beginning to present day perfection, and they are aware of the need to be fair to other civilizations. However, the price of this academic good manners is the loss of any argument at all. One is reminded of Nietzsche's point that it is necessary, for action, to forget—in this case, to forget some of the detail. And there are two grounds for attempting a different approach, which

1. There are a number of other useful sourcebooks, for example, by Struik (1969) but Fauvel and Gray is justly the most used and will be constantly referred to here.

2. Ivor Grattan-Guinness's recent work (1997) escapes the above categorization by being relatively light, cheap, and very strongly centred on the neglected nineteenth century. Although appearing to be a history of everything, it is nearer to a specialist study.

have driven me to write this book:

1. The supposed 'humanization' of mathematical studies by including history has failed in its aim if the teaching lacks the critical elements which should go with the study of history.
2. As the above example shows, the live field of doubt and debate which is *research* in the history of mathematics finds itself translated into a dead landscape of certainties. The most interesting aspect of history of mathematics as it is practised is omitted.

At this point you may reasonably ask what better option this book has to offer. The example of the 'Eudoxus fact' above is meant to (partly) pre-empt such a question by way of illustration. We have not, unfortunately, resisted the temptation to cover too wide a sweep, from Babylon in 2000 BCE to Princeton 10 years ago. We have, however, selected, leaving out (for example) Egypt, the Indian contribution aside from Kerala, and most of the European eighteenth and nineteenth centuries. Sometimes a chapter focuses on a culture, sometimes on a historical period, sometimes (the calculus) on a specific event or turning-point. At each stage our concern will be to raise questions, to consider how the various authorities address them, perhaps to give an opinion of our own, and certainly to prompt you for one.

Accordingly, the emphasis falls sometimes on history itself, and sometimes on *historiography*: the study of what the historians are doing. Has the Islamic contribution to mathematics been undervalued, and if so, why? And how should it be described? Was there a 'revolution' in mathematics in the seventeenth century—or at any other time, for that matter; by what criteria would one decide that one has taken place? Such questions are asked in this book, and the answers of some writers with opinions on the subjects are reported. Your own answers are up to you.

Notice that we are not offering an alternative to those works of scholarship which we recommend. Unlike the texts cited above (or, in more conventional history, the writings of Braudel, Aries, Hill, or Hobsbawm) this book does not set out to argue a case. The intention is to send you in search of those who have presented the arguments. Often lack of time or the limitations of university libraries will make this difficult, if not impossible (as in the case of Youschkevitch's book (Chapter 5), in French and long out of print); in any case the reference and, hopefully, a fair summary of the argument will be found here.

This approach is reflected in the structure of the chapters. In each, an opening section sets the scene and raises the main issues which seem to be important. In most, the following section, called 'Literature', discusses the sources (primary and secondary) for the period, with some remarks on how easy they may be to locate. Given the poverty of many libraries it would be good to recommend the Internet. However, you will rarely find anything substantial, apart from Euclid's *Elements* (which it is certainly worth having); and you will, as always with Internet sources, have to wade through a great mass of unsupported assertions before arriving at reliable information. The St Andrews archive (www-gap.dcs.st-and.ac.uk/ history/index.html) does have almost all the biographies you might want, with references to further reading. If your library has any money to spare, you should encourage it to invest in the main books and journals; but if you could do that,[3] this book might even become redundant.

3. And if key texts like Qin Jiushao's Jiuzhang Xushu (Chapter 4) and al-Kāshī's Calculator's Key (Chapter 5) were translated into English.

Examples

For a long time I had a strong desire in studying and research in sciences to distinguish some from others, particularly the book [Euclid's] *Elements of Geometry* which is the origin of all mathematics, and discusses point, line, surface, angle, etc. (Khayyam in Fauvel and Gray 6.C.2, p. 236)

At the age of eleven, I began Euclid, with my brother as my tutor. This was one of the great events of my life, as dazzling as first love. I had not imagined there was anything so delicious in the world. From that moment until I was thirtyeight, mathematics was my chief interest and my chief source of happiness. (Bertrand Russell 1967, p. 36)

Perhaps the central problem of the history of mathematics is that the texts we confront are at once strange and (with a little work) familiar. If we read Aristotle on how stones move, or on how one should treat slaves, it is clear that he belongs to a different time and place. If we read Euclid on rectangles, we may be less certain. Indeed, one could fill a whole chapter with examples taken from the *Elements*, the most famous textbook we have and one of the most enigmatic. Because our history likes to centre itself on discoveries, it is common to analyse the ingenious but hypothetical discoveries which underlie this text, rather than the text itself. And yet the student can learn a great deal simply by considering the unusual nature of the document and asking some questions. Take proposition II.1:

If there are two straight lines, and one of them is cut into any number of segments whatever, then the rectangle contained by the two straight lines equals the sum of the rectangles contained by the uncut straight line and each of the segments.

Let A and BC be two straight lines, and let BC be cut at random at the points D and E.

I say that the rectangle A by BC equals the sum of the rectangle A by BD, the rectangle A by DE, and the rectangle A by EC.

If we draw the picture (Fig. 1), we see that Euclid is saying *in our terms* that $a(x+y+z) = ax+ay+az$; what in algebra is called the distributive law. Some commentators would say (impatiently) that that is, essentially, what he is saying; others would say that it is important that he is using a geometric language, not a language of number; such differences were expressed in a major controversy of the 1970s, which you will find in Fauvel and Gray section 3.G. Whichever point of view we take, we can ask why the proposition is expressed in these terms, and how it might have been understood (a) by a Greek of Euclid's time, thought to be about 300 BCE and (b) by one of his readers at any time between then and the present. Euclid's own views on the subject are unavailable, and are therefore open to argument. And (it will be argued in Chapter 2), the question of what statements like proposition II.1 might mean is given a particular weight by:

1. the poverty of source material—almost no writings from before Euclid's time survive;
2. the central place which Greek geometry holds in the Islamic/Western tradition.

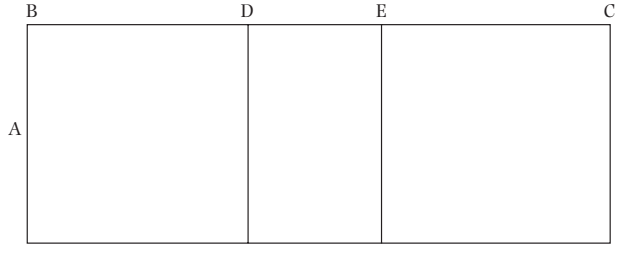

Fig. 1 The figure for Euclid's proposition II.1.

A second well-known example, equally interesting, confronted the Greeks in the nineteenth century. A classical problem dealt with by the Greeks from the fifth century onwards was the 'doubling of the cube': given a cube C, to construct a cube D of double the volume. Clearly this amounts to multiplying the side of C by $\sqrt[3]{2}$. A number of constructions for doing this were developed, even perhaps for practical reasons (see Chapter 3). As we shall discuss later, while Greek writers seemed to distinguish solutions which they thought better or worse for particular reasons, they never seem to have thought the problem insoluble—it was simply a question of which means you chose.

A much later understanding of the Greek tradition led to the imposition of a rule that the construction should be done with ruler and compasses only. This excluded all the previous solutions; and in the nineteenth century following Galois's work on equations, it was shown that the ruler-and-compass solution was impossible. We can therefore see three stages:

1. a Greek tradition in which a variety of methods are allowed, and solutions are found;
2. an 'interpreted' Greek tradition in which the question is framed as a ruler-and-compass problem, and there is a fruitless search for a solution in these restricted terms;
3. an 'algebraic' stage in which attention focuses on proving the impossibility of the interpreted problem.

All three stages are concerned with the same problem, one might say, but at each stage the game changes. Are we doing the same mathematics or a different mathematics? In studying the history, should we study all three stages together, or relate each to its own mathematical culture? Different historians will give different answers to these questions, depending on what one might call their philosophy; to think about these answers and the views which inform them is as important as the plain telling of the story.

Historicism and 'presentism'

Littlewood said to me once, [the Greeks] are not clever schoolboys or 'scholarship candidates', but 'Fellows of another college'. (Hardy 1940, p. 21)

There is not, and cannot be, number as such. We find an Indian, an Arabian, a Classical, a Western type of mathematical thought and, corresponding with each, a type of number—each type fundamentally peculiar and unique, an expression of a specific world-feeling, a symbol having a specific validity which is even capable of scientific definition, a principle of ordering the Become which reflects the central essence of one and only one soul, viz., the soul of that particular Culture. (Spengler 1934, p. 59)

In the rest of this introduction we raise some of the general problems and controversies which concern those who write about the history of science, and mathematics in particular. Following on from the last section in which we considered how far the mathematics of the past could be 'updated', it is natural to consider two approaches to this question; historicism and what is called 'presentism'. They are not exactly opposites; a glance at (say) the reviews in *Isis* will show that while historicism is sometimes considered good, presentism, like 'Whig history', is almost always bad. It is hard to be precise in definition, since both terms are widely applied; briefly, historicism asserts that the works of the past can only be interpreted in the context of a past culture, while presentism tries to relate it to our own. We see presentism in Hardy and Littlewood's belief that the ancient Greeks were Cambridge men at heart (although earlier Hardy has denied that status to the 'Orientals'). By contrast, Spengler, today a deeply unfashionable thinker, shows a radical historicism in going

so far as to claim that different cultures (on which he was unusually well-informed) have different concepts of number. It is unfair, as we shall see, to use him as representative—almost no one would make such sweeping claims as he did.

The origins of the history of mathematics, as outlined above (p. 1), imply that it was at its outset presentist. An Enlightenment viewpoint such as that of Montucla saw Archimedes (for example) as engaged on the same problems as the moderns—he was simply held back in his efforts by not having the language of Newton and Descartes. 'Classical' historicism of the nineteenth-century German school arose in reaction to such a viewpoint, often stressing 'hermeneutics', the interpretation of texts in relation to what we know of their time of production (and indeed to how we evaluate our own input). Because it was generally applied (by Schleiermacher and Dilthey) to religious or literary texts, it was not seen as leading to the radical relativism which Spengler briefly made popular in the 1920s; to assert that a text must be studied in relation to its time and culture is not necessarily to say that its 'soul' is completely different from our own—indeed, if it were, it is hard to see how we could hope to understand it. Schleiermacher in the early nineteenth century set out the project in ambitious terms:

> The vocabulary and the history of an author's age together form a whole from which his writings must be understood as a part. (Schleiermacher 1978, p. 113)

And we shall find such attempts to understand the part from the whole, for example, in Netz's study (1999, chapters 2 and 3) of Greek mathematical practice, or Martzloff's attempt (1995, chapter 4) to understand the ancient Chinese texts. The particular problem for mathematics, already sketched in the last section, is its apparent timelessness, the possibility of translating any writing from the past into our own terms. This makes it *apparently* legitimate to be unashamedly presentist and consider past writing with no reference to its context, as if it were written by a contemporary; a procedure which does not really work in literature, or even in other sciences.

To take an example: a Babylonian tablet of about 1800 BCE may tell us that the side of a square and its area add to 45; by which (see Chapter 1) it means $\frac{45}{60} = \frac{3}{4}$. There may follow a recipe for solving the problem and arriving at the answer 30 (or $\frac{30}{60} = \frac{1}{2}$) for the side of the square. Clearly we can interpret this by saying that the scribe is solving the quadratic equation $x^2 + x = \frac{3}{4}$. In a sense this would be absurd. Of equations, quadratic or other, the Babylonians knew nothing. They operated in a framework where one solved particular types of problems according to certain rules of procedure. The tablet says in these terms: Here is your problem. Do this, and you arrive at the answer. A historicist approach sees Babylonian mathematics as (so far as we can tell) framed in these terms. You can find it in Høyrup (1994) or Ritter (1995).[4]

However, the simple dismissal of the translation as unhistorical is complicated by two points. The first is straightforward: that it can be done and makes sense, and that it may even help our understanding to do so. The second is that (although we have no hard evidence) it seems that there could be a transmission line across the millennia which connects the Babylonian practice to the algebra of (for example) al-Khwārizmī in the ninth century CE. In the latter case we seem to be much more justified in talking about equations. What has changed, and when? A presentist might

4. Høyrup is even dubious about the terms 'add' and 'square' in the standard translation of such texts, claiming that neither is a correct interpretation of how the Babylonians saw their procedures.

argue that, since Babylonian mathematics has become absorbed into our own (and this too is open to argument), it makes sense to understand it in our own terms.

The problem with this idea of translation, however, is that it is a dictionary which works one way only. We can translate Archimedes' results on volumes of spheres and cylinders into our usual formulae, granted. However, could we then imagine explaining the arguments, using calculus, by which we now prove them to Archimedes? (And if we could, what would he make of non-Euclidean geometry or Gödel's theorem?) At some point the idea that he is a fellow of a different college does seem to come up against a difference between what mathematics meant for the Greeks and what it means for us.

As with the other issues raised in this introduction, the intention here is not to come down on one side of the dispute, but to clarify the issues. You can then observe the arguments played out between historians (explicitly or implicitly), and make up your own mind.

Revolutions, paradigms, and all that

> 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. (Gillies 1992, p. 1)

> From Kuhn's sociological point of view, astrology would then be socially recognised as a science. This would in my opinion be only a minor disaster; the major disaster would be the replacement of a rational criterion of science by a sociological one. (Popper 1974, p. 1146f)

If we grant that the subject of mathematics does change, how does it change, and why? This brings us to Thomas Kuhn's short book *The Structure of Scientific Revolutions*, a text which has been fortunate, even if its author has not. Quite unexpectedly it seems to have appealed to the *Zeitgeist*, presenting a new and challenging image of what happens in the history of science, in a way which is simple to remember, persuasively argued, and very readable. Like Newton's Laws of Motion, its theses are few enough and clear enough to be learned by the most simple-minded student; briefly, they reduce to four ideas:

Normal science. Most scientific research is of this kind, which Kuhn calls 'puzzle-solving'; it is carried out by a community of scholars who are in agreement with the framework of research.

Paradigm. This is the collection of allowable questions and rules for arriving at answers within the activity of normal science. What force might move the planets was not an allowable question in Aristotelian physics (since they were in a domain which was not subject to the laws of force); it became one with Galileo and Kepler.

Revolutions. From time to time—in Kuhn's preferred examples, when there is a crisis which the paradigm is unable to deal with by common agreement—the paradigm changes; a new community of scholars not only change their views about their science, but change the kinds of questions and answers they allow. This change of the paradigm is a scientific revolution. Examples include physics in the sixteenth/seventeenth century, chemistry around 1800, relativity and quantum theory in the early twentieth century.

Incommensurability. After a revolution, the practitioners of the new science are again practising normal science, solving puzzles in the new paradigm. They are unable to communicate with their pre-revolutionary colleagues, since they are talking about different objects.

Consider . . . the men who called Copernicus mad because he proclaimed that the earth moved. They were not either just wrong or quite wrong. Part of what they meant by 'earth' was fixed position. Their earth, at least, could not be moved. (Kuhn 1970*a*, p. 149)

Setting aside for the moment the key question of whether any of this might apply to mathematics, its conclusions have aroused strong reactions. Popper, as the quote above indicates, was prepared to use the words 'major disaster', and many of the so-called 'Science Warriors' of the 1990s[5] saw Kuhn's use of incommensurability in particular as opening the floodgates to so-called 'relativism'. For if, as Kuhn argued in detail, there could be no agreement across the divide marked by a revolution, then was one science right and the other wrong, or—and this was the major charge—was one indifferent about which was right? Relativism is still a very dangerous charge, and the idea that he might have been responsible for encouraging it made Kuhn deeply unhappy. Consequently, he spent much of his subsequent career trying to retreat from what some had taken to be evident consequences of his book:

I believe it would be easy to design a set of criteria—including maximum accuracy of predictions, degree of specialization, number (but not scope) of concrete problem solutions—which would enable any observer involved with neither theory to tell which was the older, which the descendant. For me, therefore, scientific development is, like biological development, unidirectional and irreversible. One scientific theory is not as good as another for doing what scientists normally do. In that sense I am not a relativist. (Kuhn 1970*b*, p. 264)

It is often said that writers have no control over the use to which readers put their books, and this seems to have been very much the case with Kuhn. The simplicity of his theses and the arguments with which he backed them up, supported by detailed historical examples, have continued to win readers. It may be that the key terms 'normal science' and 'paradigm' under the critical microscope are not as clear as they appear at first reading, and many readers subscribe to some of the main theses while holding reservations about others. Nonetheless, as Gillies proclaimed in our opening quote, the broad outlines have almost become an orthodoxy, a successful 'grand narrative' in an age which supposedly dislikes them.

So what of mathematics? It is easy to perceive it as 'normal science', if one makes a sociological study of mathematical research communities present or past; but has it known crisis, revolution, incommensurability even? This is the question which Gillies' collection (1992) attempted to answer, starting from an emphatic denial by Michael Crowe. His interesting, if variable, 'ten theses' on approaching the history of mathematics conclude with number 10, the blunt assertion: 'Revolutions never occur in mathematics' (Gillies 1992, p. 19). The argument for this, as Mehrtens points out in his contribution to the volume, is not a strong one. Crowe aligns himself with a very traditional view, citing (for example) Hankel in 1869:

In most sciences, one generation tears down what another has built . . . In mathematics alone each generation builds a new storey to the old structure. (Cited in Moritz 1942, p. 14)

Other sciences may have to face the problems of paradigm change and incommensurability, but ours does not. It seems rather complacent as a standpoint, but there is some evidence. One test case appealed to by both Crowe and Mehrtens is that of the 'overthrow' of Euclidean geometry in the nineteenth century with the discovery of non-Euclidean geometries (see chapter 8). The point made by Crowe is that unlike Newtonian physics—which Kuhn persuasively argued could not be

5. This refers to a series of arguments, mainly in the United States, about the supposed attack on science by postmodernists, sociologists, feminists, and others. See (Ashman and Barringer 2000)

seen as 'true' in the same sense after Einstein—Euclidean geometry is still valid, even if its status is now that of one acceptable geometry among many.

This point, of course, links to those raised in the previous sections. How far is Euclid's geometry the same as our own? An interesting related variant on the 'revolution' theme, which concerns the same question, is the status of geometry as a subject. Again in Chapter 8, we shall see that geometry in the time of Euclid was (apparently) an abstract study, which was marked off from the study of 'the world' in that geometric lines were unbounded (for example), while space was finite. By the time of Newton, space had become infinite, and geometry was much more closely linked to what the world was like. Hence, the stakes were higher, in that there could clearly only be one world and one geometry of it. The status of Euclidean geometry as one among many, to which Crowe refers, is the outcome of yet another change in mathematics, *later* than the invention of the non-Euclidean geometries: the rise of the axiomatic viewpoint at the end of the nineteenth century and the idea that mathematics studied not the world, but axiom-systems and their consequences.

It may be that neither of these radical changes in the role of geometry altered the 'truth-claims' of the Euclidean model. Nonetheless, there is a case for claiming that they had a serious effect on what geometry was about, and so could be treated as paradigm shifts. Indeed, we shall see early nineteenth-century writers treating geometry as an applied science; in which case, one imagines, the Kuhnian model would be applicable.

As can be seen, to some extent the debate relates to questions raised earlier, in particular how far one adheres to a progressive or accumulative view of the past of mathematics. There have been subsequent contributions to the debate in the years since Gillies' book, but there is not yet a consensus even at the level that exists for Kuhn's thesis.

External versus internal

> [In Descartes' time] mathematics, under the tremendous pressure of social forces, increased not only in volume and profundity, but also rose rapidly to a position of honor. (Struik 1936, p. 85)

> I would give a chocolate mint to whoever could explain to me why the social background of the small German courts of the 18th century, where Gauss lived, should inevitably lead him to deal with the construction of the 17-sided regular polygon. (Dieudonné 1987)

An old, and perhaps unnecessary dispute has opposed those who in history of science consider that the development of science can be considered as a logical deduction in isolation from the demands of society ('internal'), and those who claim that the development is at some level shaped by its social background ('external'). Until about 30 years ago, Marxism and various derivatives were the main proponents of the external viewpoint, and the young Dirk Struik, writing in the 1930s, gives a strong defence of this position. Already at that point Struik is too good a historian not to be nuanced about the relations between the class struggle and mathematical renewal under Descartes:

> In [the] interaction between theory and practice, between the social necessity to get results and the love of science for science's sake, between work on paper and work on ships and in fields, we see an example of the dialectics of reality, a simple illustration of the unity of opposites, and the interpenetration of polar forms . . . The history and the structure of mathematics provide example after example for the study of materialist dialectics. (Struik 1936, p. 84)

The extreme disfavour under which Marxism has fallen since the 1930s has led those who believe in some influence of society to abandon classes and draw on more acceptable concepts such

as milieus, groups, and actors; and Dieudonné has died without conceding that anyone had earned his chocolate mint. Yet in a sense the struggle has sharpened, under the influence of what has been called the 'Edinburgh School' or the 'strong program in the sociology of knowledge' (SPSK), originally propounded in the 1980s by Barry Barnes and David Bloor. For Marxists believed that scientific knowledge (including Marxism) was objective, and hence the rising classes would be inspired to find out true facts (as Struik's examples of logarithms and Cartesian geometry illustrate); as Mao famously said:

> Where do correct ideas come from? Do they fall from the sky? No. Are they innate in the mind? No. They come from social practice, and from it alone. They come from three kinds of social practice, the struggle for production, the class struggle and scientific experiment. (Mao Zedong 1963, p. 1)

Notice that Mao too allows for 'internal' factors; the use of scientific experiment to arrive at correct ideas. The Edinburgh school has led the way in an increased scepticism, even relativism on the issue of scientific truth, and in seeing, in the limit, *all* knowledge as socially determined. In one way such a view might be easier for mathematicians to accept than for physicists (say), since the latter consider it important for their justification that electrons, quarks, and so on should be objects 'out there' rather than social constructions. Mathematicians, one would think, are less likely to feel the same way about (say) the square root of minus one, however useful it may be in electrical engineering. In this respect, Leopold Kronecker's famous saying that 'God made the natural numbers; all else is the work of man' places him as a social constructivist before his time.

A deliberately hard test case in a recent text by some of the school goes to work on the deduction of '$2 + 2 = 4$', on proof in general, underlying assumptions, logical steps in proof, and so on.

> So-called 'self-evidence' is historically variable . . . Rather than endorsing one of the claims to self-evidence and rejecting the other, the historian can take seriously the unprovability of the claims that are made at this level, and search out the immediate *causes* of the credibility that is attached or withheld from them. Self-evidence should be treated as an 'actors' category'. . . (Barnes et al. 1996, p. 190)

Because they are sociologists rather than historians, the Edinburgh school tend not to have an underlying theory of historical change; hence they are stronger on identifying difference across cultures or periods than on identifying the basis on which change takes place. While influenced by Kuhn, and so seeing some sort of a crisis or breakdown in the consensus as motivating, they feel that the actors and their social norms must have something to do with it. However, the society in crisis may be simply the mathematical research community, in which case we are still in a modified 'internal' model similar to that of Kuhn (cf. the disputes about the axiom of choice cited in Barnes et al. 1996, pp. 191–2); or it may be influenced by the wider community, as in the case of Joan Richards' study of the relation between Euclidean geometry and the Victorian established church (see chapter 8). As Paul Forman, responsible for one of the best studies of the interaction of science and society (1971), has pointed out recently (1995), the accusation of relativism seems to have driven many advocates of the strong programme into a partial retreat from a position which was never very historically explicit.

And yet, the hard-line internalist position is still considered inadequate by many historians, even if they are not sure what mixture of determinants they should put in its place. Often in the last two centuries, internal determinants seem paramount,[6] though in operational research, computing

6. One could, for example, point out that knot theory, while first developed in the 1870s by an electrical engineer (Tait) to deal with a physical problem, has proceeded according to an apparent internal logic of its own since then. See chapter 9.

and even chaos theory one could see outside forces at work. In earlier history, when we have the evidence (and we often do not) it often seems the other way round. In his commentary on the 'Rectangular Arrays' (matrices) section of the *Nine Chapters* (see chapter 4), Liu Hui analyses a problem on different grades of paddy. He says, 'It is difficult to comprehend in mere words, so we simply use paddy to clarify'. Does he mean that the authors of the classical text first hit on the idea of using matrix algebra and then applied it to grades of paddy for ease of exposition? We have no evidence, but it seems easier to believe that the discovery went the other way round, from problems about paddy (or something) to matrices.

It is easy to say that among most responsible historians now the tendency is to take both internal and external determinants seriously in any given situation and to give them their appropriate weight. The problem is that with the eclipse of Marxism and with doubts about Kuhn's relevance to mathematics, there is no very well organized version of either available to the historian. We shall continue to appeal to Marxism (and indeed to Kuhn) where we find either of them relevant in what follows.

Eurocentrism

> I propose to show . . . that the standard treatment of the history of non-European mathematics exhibits a deep-rooted historiographical bias in the selection and interpretation of facts, and that mathematical activity outside Europe has in consequence been ignored, devalued or distorted. (Joseph 1992, p. 3)

> His willingness to concoct historically insupportable myths that are pleasing to his political sensibilities is obvious on every page. His eagerness to insinuate himself into the good graces of the supposed educators who incessantly preach the virtues of 'multiculturalism' and the vices of 'eurocentrism' is palpable and pervasive. (Review on mathbook.com)

It would appear that the argument set out by Joseph has not been won yet. I have no way of judging the book under review (it is not Joseph's) in the second quote, but there is an underlying suggestion that the reviewer has heard more than enough about eurocentrism and is pleased to find a book which is both anti-eurocentrist and intellectually shoddy, thereby supporting his or her suspicions. This is the 'fashionable nonsense'[7] school of reviewing, and it is not going to go away; in fact, the current anti-Islamic trend in the West, and specifically in the United States, may lend it more support.

What is eurocentrism (for those who have not heard yet)? In general terms, it is the privileging of (white) European/American discourse over others, most often African or Asian; in history, it might mean privileging the European account of the Crusades, or of the Opium Wars, or any imperialist episode over the 'other side'. For what it might mean in mathematics, we should go back to Joseph who, at the time he began his project (in the 1980s), had a strong, passionate, and undeniable point. If we count as the 'European' tradition one which consists *solely* of the ancient Greeks and the modern Europeans—and we shall soon see how problematic that is—a glance through many major texts in the history of mathematics showed either ignorance or undervaluing of the achievements of those outside that tradition. We shall discuss this in more detail later (Chapter 5), but his book was important; it is the only book in the history of mathematics written from a strong personal conviction, and it is valuable for that reason alone. It also stands as the single most influential work in changing attitudes to non-European mathematics. The sources, such as Neugebauer on the

7. The title of a book (Sokal and Bricmont 1998) which is devoted to attacking what it sees as sloppy thinking about science by postmodernists, feminists, post-colonialists, and many others.

Egyptians and Babylonians, or Youschkevitch on the Islamic tradition, may have been available for some time before, but Joseph drew their findings into a forceful argument which since (like Kuhn's work) its main thrust is easy to follow has made many converts. After sketching the views which he intends to counter, Joseph characterizes three historical models which can be used to describe the transmission of mathematical knowledge.

First, the 'classical Eurocentric trajectory' already referred to: mathematics passed directly from the ancient Greeks to the Renaissance Europeans;

Second, the 'modified Eurocentric trajectory': Greece drew to some extent on the mathematics of Egypt and Babylonia; while after Greek learning had come to an end, it was preserved in the Islamic world to be reintroduced at the Renaissance;

Third, Joseph's own 'alternative trajectory'. This—with a great many arrows in the transmission diagram—stresses the central role of the Islamic world in the Middle Ages as a cultural centre in touch with the learning of India, China, and Europe and acting both as transmitter and receiver of knowledge. The more we know, particularly of the Islamic world, the more this appears to be a reasonably accurate picture, and while Joseph's tone can be polemical and some of his detailed points have been questioned, his arguments are rarely overstated. We are learning more of the mathematics of India, China, and Islam, as of the Greeks' predecessors, and scholars are becoming better able to read their texts and understand their way of thinking about mathematics.

The body of the book is given over to a detailed account of the various non-European cultures and their contributions. Interestingly, his account is now to be found substantially unchanged (if with more detail) in most of the standard textbooks. The culture warriors may rage against fashionable anti-Eurocentrism, but as far as mainstream teaching of the history of mathematics is concerned, it seems to have been absorbed successfully. Again, we shall return to this point later.

The specific reasons for Eurocentrism in the history of mathematics (setting aside traditional racism and other prejudices) have been two-fold. The first is the very high value accorded to the work of the ancient Greeks specifically, the second the emphasis on discovery and proof of results. These are indeed linked: much of the Greek work was organized in the form of result + proof. All the same, there is an important point to be made here; namely, that after the Greeks it was the Arabs who continued the tradition, with propositions and proofs in the Euclidean mode. (Khayyam's geometric work on the cubic equations is a model of the form.) If we contrast Islamic mathematics of around 1200 with that of western Europe, we would have no doubt that the former was, in our terms, 'Western', and the latter a primitive outsider. However, this has not, until recently, helped the integration of the great Islamic mathematicians into the Western tradition; and if it did, it would still leave the Indians and Chinese, with very different practices, outside it.

Indeed, the problem of Eurocentrism could be seen in Kuhnian terms as one of paradigms. The Greek paradigm, or a version of it, is one which has in some form persisted into modern Western mathematics[8] and hence traditional histories have constructed themselves around that paradigm, either leaving out or subordinating ways of doing mathematics which did not fit. It is only more recently that a more culturally aware (historicist?) history has been able to ask how other cultures thought of the practice of mathematics, and to escape the trap of evaluating it against a supposed Greek or Western ideal.

8. Not at all times; Descartes, Newton in his early work, and Leibniz initiated a tradition in which the Euclidean mode was at least temporarily abandoned. See chapters 6 and 7.