even his own discomfort with the actual infinite to support his transfinite numbers. 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.

From the examples we have investigated here, it seems clear that mathematics may be revolutionized by the discovery of something entirely new and completely unexpected within the bounds of previous theory. Discovery of incommensurable magnitudes and the eventual creation of irrational numbers, the imaginary numbers, the calculus, non-Euclidean geometry, transfinite numbers, the paradoxes of set theory, even Gödel's incompleteness proof, are all revolutionary—they have all changed the content of mathematics and the ways in which mathematics is regarded. They have each done more than simply add to mathematics—they have each transformed it. In each case the old mathematics is no longer what it seemed to be, perhaps no longer even of much interest when compared with the new and revolutionary ideas that supplant it.

NOTES

1. The most adamant statement that mathematics does not experience revolutions may be found in M. J. Crowe (1975, pp. 15–20, esp. p. 19). The literature on the subject, however, is vast. Of authors who have claimed that mathematics grows by accumulation of results, without rejecting any of its past, the following sample is indicative: H. Hankel (1871, p. 25); G. D. Birkhoff (1934, esp. p. 302; 1950, p. 557); C. Truesdell (1968, foreword)—'While "imagination, fancy, and invention" are the soul of mathematical research, in mathematics there has never yet been a revolution.'
2. J. W. Dauben, Set theory and the nature of scientific resolution. (MS) for the Colloquium History of Mathematics and Recent Philosophies of Science (at the semicentennial meeting of the History of Science Society, Burndy Library, Norwalk, Conn., 27 October 1974).
3. Crowe's ten 'laws' (Crowe 1975, p. 16); see also M. J. Crowe (1967*b*, pp. 105–26, esp. pp. 123–4).
4. I. B. Cohen (1976*a*). More recently, Professor Cohen has also developed this material in a number of articles (see also Cohen 1980, esp. pp. 39–49).
5. Bernard de Fontenelle (1727); refer in particular to the preface, which is also reprinted in Fontenelle (1792, Vol. VI, p. 43).
6. Bernard de Fontenelle (1719, esp. p. 98). See also Fontenelle (1792, Vol. VII, p. 67).
7. For details of the background to Greek mathematics, and in particular to the history of incommensurability, see the recent works by W. R. Knorr (1975) and H. J. Waschkies (1977). I am especially indebted to Wilbur Knorr for his comments on an early draft of this chapter. Our discussion of the many difficulties

in dealing with pre-Socratic material has been of great help to me in clarifying many murky or puzzling aspects of the history of the theory of incommensurable magnitudes and early Greek geometry.

8. Aristotle, *Metaphysics*, 985b23–986a3. Similarly, 1090a20–25. See the more direct interpretation that 'things are numbers' and variations at 1080b16–21; 1083b11, 18.
9. Aristotle, *Metaphysics*, 1080b16–20; see also *De caelo*, 300a16–19. The whole question of Pythagorean number theory and its character has been vigorously debated. For a general introduction that is careful to underscore the problems in reconstructing what the Pythagoreans may have believed, see J. A. Philip (1966). Harold Cherniss (1951, esp. p. 336) has described the Pythagorean point of view as more 'a materialization of number than a mathematization of nature'. The source for number atomism in Pythagorean mathematics comes from Ecphantus of Syracuse, and as W. Knorr (1975, p. 43) notes, this provides the basis for a thesis long in fashion via P. Tannery and F. M. Cornford, but which seems more recently to have fallen into disrepute. Yet I believe a form of "number-atomism" may be accepted as having been a doctrine of some Pythagoreans.' In a review of J. E. Raven's *Pythagoreans and Eleatics*, Gregory Vlastos (1953, p. 32) argued vigorously that 'number-atomism was not regarded by the tradition stemming from Theophrastus as an original feature of Pythagoreanism'. He carries this further by arguing that number-atomism was surely not a feature of Pythagorean musical formulae, 'nor could there be any question of number-atomism in the extensions of this theory to medicine, moral, or psychological concepts'. Fortunately, the question of number-atomism is not crucial to the issues presented here. Whether the early Pythagoreans, or only some later Pythagoreans like Ecphantus, adopted a view of number as material monads, the significant feature of Pythagorean arithmetic for the present purposes was its emphasis on *ratio*, and its belief that all things could be expressed through ratios of whole numbers.
10. H. Vogt (1909–10, 1913–14) was among the first to attempt the reconstruction of the development of a theory of proportion in response to the discovery of incommensurable magnitudes through transformations in terminology. Later Kurt von Fritz developed a similar approach in his articles on 'Theodoros' and 'Theaitetos' in *Paulys Real-Encyclopädie der classischen Altertumswissenschaft* (second series, Metzlersche Verlagsbuchhandlung, 1934), pp. 1811–31, 1351–72, respectively. See also Fritz (1945).
11. Aristotle, *Metaphysics*, 1092b10. Aristotle reports that Eurytus decided the number of man or horse, for example, 'by imitating the figures of living things with pebbles'. For commentaries on this passage by Alexander (*Metaphysics*, 827, 9) and Theophrastus (*Metaphysics*, 6a19), see G. S. Kirk and J. E. Raven (1957, p. 314). Wilbur Knorr (1975, p. 45) maintains that Eurytus's approach was an attempt to modify Pythagorean number-atomism in response to discovery of incommensurables.
12. For representative passages in Aristotle, *Metaphysics*, turn to 985b23–31, 986a2–8. See as well the discussion in Kirk and Raven (1957, pp. 236–62, esp. pp. 248–50). It should be noted that some writers mimimize the significance of the Pythagoreans in the history of mathematics and science. See, for example, W. A. Heidel (1940, p. 31): 'The role of the Pythagoreans must appear to have been much

exaggerated.' Even more emphatic is the view of W. Burkert (1972, p. 482) 'The tradition of Pythagoras as a philosopher and scientist is, from the historical view, a mistake . . . Thus, after all, there lived on, in the image of Pythagoras, the great Wizard whom even an advanced age, though it be unwilling to admit the fact, cannot entirely dismiss.' As for the Pythagorean concept of a 'perfect number', it must be remembered that their definition differed from that now standard in mathematics. For the Pythagoreans, the number 10 was perfect because it was the sum of the first four integers, $1+2+3+4=10$. Only after Aristotle did the sense of 'perfect numbers', as used by Euclid, make its appearance. Then, as now, a perfect number is equal to the sum of it divisors. Consequently, $6=1+2+3$ and $28=1+2+4+7+14$ are both perfect numbers, but 10 is not, since $10\neq 1+2+5$. For further information see Burkert (1972, p. 431).

13. This, too, is a question that has received much discussion but little agreement in literature on the subject. For the most recent study of the problem, W. Knorr (1975, pp. 36–49, esp. p. 40) presents numerous arguments to establish the discovery within a twenty-year span from 430 to 410 BC.
14. For Aristotle's discussion of the incommensurability of the side and diagonal of a square, see *Prior analytics*, 41–29. W. Knorr (1975, pp. 22–8, esp. p. 23) discusses this proof and its version in Euclid's Book X of the *Elements* at length, noting that 'arguing for the antiquity of this version of the proof is its application of the even and the odd'. Arguing for the discovery of incommensurability by Pythagoreans studying the method of *anthyphairesis*, discussed later (see n. 21), are Kurt von Fritz (1945, p. 46) and S. Heller (1956, 1958). See also the discussion in W. Knorr (1975, pp. 29–36).
15. Although much debate has centred on the advisability of referring to the discovery as a 'crisis', as did H. Hasse and H. Scholz (1928), an important distinction must be made between the effect of the discovery of incommensurability upon mathematics as opposed to Pythgorean arithmology and its close connection with their cosmology or arithmological philosophy. For non-Pythagoreans and mathematicians in general, the ancient literature never mentions a 'crisis' but refers instead to the discovery as an advance, or even as a great 'wonder'. This is precisely the attitude of Aristotle (*Metaphysics*, 983a13–20): 'As we said, all men begin wondering that a thing should be so; the subject may be, for example, the automata in a peepshow, the solstices, or the incommensurability of the diagonal. For it must seem a matter for wonder, to all who have not studied the case, that there should be anything that cannot be measured by any measure, however small.' For Pythagorean arithmology, on the other hand, the discovery must have posed a major problem, and in this context its effect can be accurately described as representing a 'crisis.'

    G. E. L. Owen (1957–8, p. 214) is even more emphatic in asserting that 'discovery of incommensurables was a real crisis in mathematics'. For arguments that there was no such crisis, however, see K. Reidemeister (1949, p. 30) and H. Freudenthal (1966). Burkert (1972, p. 462) comes to similar conclusions.
16. Scholium to Euclid, *Elementa*, X, I, in *Opera omnia* (ed. J. L. Heiberg, Teubner, 1888), p. 417. For other accounts of the drowning episode, see Iamblichus *De vita Pythagorica liber*, XXXIV, 247, and XVIII, 88 (ed. Ludwig Deubner, Teubner,

1937), pp. 132 and 52, respectively, and Iamblichus, *De communi mathematica scientia liber*, XXV (ed. Nicola Festa, Teubner, 1891). pp. 76–8. Burkert (1972, p. 455) writes that 'the tradition of secrecy, betrayal, and divine punishment provided the occasion for the reconstruction of a veritable melodrama in intellectual history'. Pappus, however, viewed the story of the drowning as a 'parable', *The commentary of Pappus on Book X of Euclid's Elements*, Book I, Section 2 (ed. G. Junge and W. Thomson, Harvard University Press, 1930; reprinted by Johnson Reprint Corp., 1968), p. 64: the story was 'most probably a parable by which they sought to express their conviction that firstly, it is better to conceal (or veil) every surd, or irrational, or inconceivable in the universe, and, secondly, that the soul which by error or heedlessness discovers or reveals anything of this nature which is in it or in this world, wanders [thereafter] hither and thither on the sea of non-identity (i.e. lacking all similarity of quality or accident), immersed in the stream of the coming-to-be and the passing-away, where there is no standard of measurement.'

17. Scholium to Euclid, *Elementa*, X, I. For discussion of this passage, see Moritz Cantor (1894, Vol. 1, p. 175). As Burkert (1972, p. 461) has pointed out, later commentators like Plutarch and Pappus might have been especially tempted to seize on the *double entendre* made possible by the multiple connotations of the word ἄρρητος as irrational and unspeakable: 'In Plutarch it is clear that the word ἄρρητος, set in quotation marks, as it were, by λεγόμγναι, is to be understood in a double sense. The "ineffable because irrational" is at the same time the "unspeakable because secret" ... The fascination of the ἀρρήτου lies in the pretense to indicate the fundamental limitations of human expression, which are at the same time transcended by the initiate ... This exciting double sense of the word ἄρρητος is what makes the story of the discovery and betrayal of the irrational an *exemplum* for Plutarch, and even more for Pappus, who is probably following some Platonic source.' For additional discussion of these terminological transformations, refer to K. von Fritz (1939, p. 69; 1955, pp. 13–103, esp. pp. 80–7), as well as to the articles by von Fritz and Vogt cited in n. 10. It should also be added that Mugler, in defining ἄρρητος, writes that 'son sens étymologique étant «indicible, inexprimable»; il était synonyme, à l'origine, de ἄλογος au sens primitif' (*Dictionnaire*, p. 83).

18. The position adopted by Michael Crowe (1975, p. 19), for one, is that 'revolutions may occur in mathematical nomenclature, symbolism, metamathematics, methodology, and perhaps even in the historiography of mathematics', but *not* within mathematics itself.

19. Archytas, *Fragment* B4 (Fragmente der Gespräche) in H. Diels, *Die Fragmente der Vorsokratiker*, Vol. I (Weidmannsche, 1922), p. 337: 'Und die Arithmetik hat ... einen recht betrachtlichen Vorrang ... besonders aber auch vor der Geometrie, da sie deutlicher als diese was sie will behandeln kann ... ⟨Denn die Geometrie beweist, wo die anderen Künste im Stiche lassen,⟩ und wo die Geometrie wiederum versagt, bringt die Arithmetik sowohl Beweise zustande wie auch die *Darlegung* der Formem [Prinzipien?], wenn es überhaupt irgend eine wissenschaftliche Behandlung der Formen gibt.

20. W. Knorr (1975, pp. 170–210, esp. pp. 199, 220–1) 'The early study of

incommensurabilty: Theodorus'. Here the recent research of D. Fowler is also relevant, above all his pair of articles, (Fowler 1980, 1982). I am happy to acknowledge a very stimulating correspondence with David Fowler covering a range of subjects including incommensurability, *anthyphairesis*, and Greek theories of ratio and proportion in general. Although our correspondence on these matters came after this essay was already in the press, I am grateful for his very careful reading of my original paper, and his subsequent comments, only a few of which it has been possible to incorporate here. Readers should also note in particular D. Fowler (1979, 1981).

21. O. Becker (1933), in analysing the concept of ανθυφαίρεσις, reconstructed a pre-Eudoxan theory of proportion. For a detailed discussion of *anthyphairesis*, see W. Knorr (1975, pp. 29–36), '*Anthyphairesis* and the side and diameter', and H. Waschkies (1977, pp. 77–100), 'Die anthyphairetische Proportionentheorie'. Mugler, *Dictionnaire*, p. 61, connects ἀνθυφαιρεῖν, the process of reciprocal subtraction, with study of the irrational magnitudes and the older, archaic term, '*probablement d'origine pythagoricienne*, ἀνταναίρεσις', p. 65. See as well the commentary on Theaetetus's demonstration and *anthyphairesis* by François Lasserre (1964, pp. 68–9).

22. Aristotle, *Posterior analytics*, 74a17–30, refers to the new, more general techniques of proof (ὃ καθόλου ὑποτίθεται ὑπαρχειν). Moreover, Scholia 1 and 3 to Book V of the *Elements* comment on the generality of the results obtained there. See Euclid, *Opera omnia* (ed. Heiberg), Vol. V, pp. 280 and 282, respectively. In fact, the differences between the earliest theory of proportion, generally regarded as authentically Pythagorean and set forth in Book VII of Euclid's *Elements*, and Eudoxus's powerful more general theory as represented in Euclid Book V, may be seen in a comparison of several parallel definitions. For example:

Book VII, Definition 3: Μέρος ἐστὶν ἀριθμὸς ἀριθμοῦ ὁ ἐλάσσων τοῦ μείξονος ὅταν καταμετρῇ τὸν μείξονα.

Book V, Definition 1: Μέρος ἐστὶ μεγέθοζ μεγέθους τὸ ἔλασσον τοῦ μείζονος; ὅταν καταμετρῇ τὸ μεῖςον.

Book VII, Definition 5: Πολλαπλάσις δὲ ὁ μείξων τοῦ ἐλάσσονος, ὅταν καταμετρῆται ὑπὸ τοῦ ἐλάσσονος.

Book V, Definition 2: Πολλαπλάσιον δὲ τὸ μεῖξον τοῦ ἐλάττονος, ὅταν καταμετρῆται ὑπὸ τοῦ ἐλάττονος.

Waschkies (1977, p. 19) also underscores the significance of the term μέγεθος for magnitude in Book V by noting that it became a technical term in geometry directly as a result of Eudoxus's influence.

23. Scholium 1 to Book V of Euclid's *Elements* in *Opera omnia* (ed. Heiberg), Vol. V, p. 280. As W. Knorr (1975, p. 274) notes, 'The fundamental conception of proportion in *Elements* V, if not the completion of the entire theory, is due to Eudoxus.'

24. It should be stressed, however, that the Greeks never attained such a general concept of number. For them, ἀριθμοι, or numbers, were always defined, as in Euclid VII, Definition 2, as a sum of *units*. There were no rational or irrational

numbers, only ratios of whole numbers and proportions defined as equal ratios (van der Waerden 1961, p. 125). Despite the conjectures of some historians (see, e.g., Heath 1921, Vol. I, p. 327), the Greeks *never* had the concept of real numbers, Dedekind cuts, or even the set of rational numbers. For details, see F. Beckmann (1967, esp. pp. 21, 37–41). Knorr (1975, pp. 9–10) stresses that ἀριθμός (=number) and λόγος (=ratio) were *never* equated in the ancient tradition.

25. Aristotle takes Theorem V, 16, on the *ennalax* property of proportions, as epitomizing the great transformation in proof techniques and capabilities brought about by Eudoxus's theory (see n. 22). On a simpler level, Book V duplicates propositions from Book II, where they were originally established for line segments only. Book V, of course, establishes similar theorems for all magnitudes in general. One may also compare, for example, specific propositions like the *di' isou* theorem for proportions, *Elements* V, 22, with the earlier version, VII, 14, where a different method was originally used employing the special properties of integers as opposed to magnitudes. Recently, Wilbur Knorr (1975, p. 304) has argued that in Theorems X, 9–10, Euclid saw the unsuitability of the original pre-Eudoxan proofs of these propositions, and therefore gave them a new, if not very skilful version suitable to post-Eudoxan theory.

26. By directly comparing the proofs of various Euclidean propositions in their pre- and post-Eudoxan forms, it is possible to make clear their comparative 'advantages and limitations', as Knorr (1975, Appendix B, pp. 332–44) does in drawing direct comparisons where possible between theorems in Book V and their counterparts in Book VII.

   As Zeuthen (1910) observed, it is precisely at Theorem VII, 19, that the relation between Book V and Book VII is directly established, for in VII, 19, Euclid shows that the definition of proportion used in Book V is equivalent to definition VII, 20 when applied to numbers. It therefore follows that all theorems on proportion in Book V may be applied to any of the theorems dealing with proportions between numbers alone in Book VII. Zeuthen (1910, p. 412) states that 'l'importance logique du No. 19 consiste précisément en ce qu'on y établit que la définition d'une proportion donné dans le V[e] livre a, si on l'applique à des nombres entiers, tout à fait la même portée que la définition donnée au VII[e] livre'.

   Wholly apart from the significance of Eudoxus's theory of proportion for the development of the Euclidean *Elements*, Kurt von Fritz (1945, p. 264) has pointed out that Eudoxus was 'the author of the method of exhaustion, of the theorem that the volume of a cone is one-third of the volume of a cylinder with the same base and altitude, and undoubtedly of other stereometric theorems which must have been used in the proof of that proposition. All this would have been impossible without the new definition of proportion invented by Eudoxus.' Similarly, Wilbur Knorr (1975, p. 306) has noted that 'the renovation of proportion theory (Book V) was used to improve the foundations of geometry (Books VI and XI) and with the "method of exhaustion" to effect the measurements in Book XIII'.

27. For the details of Cantor's biography and the origins of transfinite set theory, sketched here only in the broadest outline, consult A. Fraenkel (1930). For more recent studies, refer to H. Meschkowski (1967), I. Grattan-Guinness (1971), J. Dauben (1979), and 'The development of Cantorian set theory', Chap. 5 in

I. Grattan-Guinness 1980, pp. 1181–219). I am grateful to Esther Phillips for her comments on an earlier version of this paper. Conversations with her on the subject of revolutions in mathematics have also greatly benefited the analysis that follows.

28. See E. Heine (1870, esp. p. 353). As Cantor noted in a footnote to his first paper on the subject, 'Zu den folgenden Arbeiten bin ich durch Herrn *Heine* angeregt worden. Derselbe hat die Güte gehabt, mich mit seinen Untersuchungen über trigonometrische Reihen frühzeitig bekannt zu machen' (Cantor 1870, p. 130).
29. G. Cantor (1872). For a discussion of the significance of this paper in the context of Cantor's early work, consult J. Dauben (1971) and 'The origins of Cantorian set theory', Chap. 2 in Dauben (1979, pp. 30–46).
30. For a fuller discussion of Cantor's early conceptualization of derived sets and the distinction between sets of the first and second species, see J. Dauben (1974).
31. It should be noted that Richard Dedekind's famous theory of 'cuts' used to define the real numbers was also published in the same year (Dedekind 1872). See also P. E. B. Jourdain (1910) and J. Cavaillès (1962, esp. pp. 35–44).
32. G. Cantor (1883), translated, in part, into French as 'Fondements d'une théorie générale des ensembles', *Acta Mathematica*, **2** (1883), pp. 381–408. There is also an English translation by U. Parpart, 'Foundations of the theory of manifold's', *The Campaigner* (The Theoretical Journal of the National Caucus of Labor Committees), **9**, (January and February), pp. 69–96. The reader should be warned, however, that in addition to missing the distinction between *reellen* and *realen Zahlen* in translating the *Grundlagen*, Parpart also fails to distinguish between *Zahlen* and *Anzahlen*, translating both as 'number' throughout without making clear the differences crucial to Cantor's introduction of the transfinite numbers. For fuller discussion of the significance of such terminological aspects of the *Grundlagen*, see J. Dauben (1979, pp. 125–8).
33. G. Cantor (1895–7). Part I was translated into Italian by F. Gerbaldi 'Contribuzione al fondamento della teoria degli inseimi transfinite', *Rivista di Matematica*, (**5**) (1985), pp. 129–62. Both parts were translated into French by F. Marotte *Sur les fondements de la théorie des ensembles transfinis* (Paris: Hermann, 1899), and into English by P. E. B. Jourdain *Contributions to the founding of the theory of transfinite numbers* (Open Court, 1915). For discussion of Cantor's terminology, and the remarkable fact that he only introduced the transfinite alephs in 1893, although he had introduced the ω for transfinite ordinal numbers in 1883, see J. Dauben (1979, pp. 179–81).
34. See in particular the discussion by Cantor (1833, Sections 4–8, reprinted in *Gesammelte Abhandlungen*, pp. 173–83). The following analysis presents, in its major outline, the views Cantor held on these matters.
35. Gauss wrote to Schumacher from Göttingen on 12 July 1831. See letter 396 (Gauss's letter 177) in *Briefwechsel zwischen K. F. Gauss und H. C. Schumacher* (ed. C. A. F. Peters, Esch, 1860), Vol. II, p. 269.
36. See Cantor's explanation of immanent and transient realities (Cantor 1883, Section 8, reprinted in *Gesammelte Abhandlungen*, pp. 181–3).
37. This is exactly Cantor's point in Section 8 of the *Grundlagen* (1883), where he stresses that the natural sciences are always concerned with the 'fit with facts', while

mathematics need not be concerned with the conditions of natural phenomena as an ultimate arbiter of the truth or success of a given theory. In the natural sciences, however, historians and philosophers of science have been especially interested in the nature of the connections between observation, experiment, and theory. Among many works that might be cited, that of Thomas Kuhn is perhaps the best known and will suffice here to give some sense of the connections that set the sciences in general apart from mathematics: 'The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature and with each other' (Kuhn 1962, p. 77). It was precisely its independence from nature that gave mathematics, in Cantor's view, its 'freedom' as characterized in the passage quoted on p. 61 (Cantor 1883, p. 182).

38. See 'The invisibility of revolutions', Chap. 11 in T. S. Kuhn (1962, 135–42, esp. p. 136).

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5

Appendix (1992): revolutions revisited

JOSEPH DAUBEN

Revolutions never occur in mathematics.

Michael J. Crowe

Cauchy was responsible for the first great revolution in mathematical rigor since the time of the ancient Greeks.

J. V. Grabiner

Nonstandard analysis is revolutionary. Revolutions are seldom welcomed by the established party, although revolutionaries often are.

G. R. Blackley

I argued in my 1984 paper (reprinted as Chapter 4 of this volume) that revolutions in mathematics do occur, and provided details with two examples:

(1) the discovery of incommensurable magnitudes in Antiquity, and the problem of irrational numbers that it engendered;

(2) the creation of transfinite set theory and the revolution brought about by Georg Cantor's new mathematics of the infinite in the nineteenth century.

In what follows, two additional, closely related case histories are considered, each of which represents yet another example of revolutionary change in mathematics:

(1) the introduction of new standards of rigour for the calculus by Augustin-Louis Cauchy in the nineteenth century;

(2) the creation, in this century, of non-standard analysis by Abraham Robinson.

Each of these examples may be regarded as much more than simply another novel departure for mathematics. Each represents a new way of doing mathematics, by means of which its face and framework were dramatically altered in ways that indeed proved to be revolutionary.

5.1. CAUCHY'S REVOLUTION IN RIGOUR

The revolution brought about by Newton and Leibniz (see Chapters 8 and 7, respectively) was not without its problems, as the penetrating critiques of

Bishop Berkeley, Bernhardt Nieuwentijdt, and Michel Rolle attest (Boyer 1959; Grattan-Guinness 1969; Guicciardini 1989). In fact, the eighteenth century, despite its willingness to use the calculus, seems to have been plagued by a concomitant sense of doubt as to whether its use was really legitimate or not. It worked, and, lacking alternatives, mathematicians persisted in applying it in diverse situations. Nevertheless, the foundational validity of the calculus was often the subject of discussion, debate, and prize problems. The best known of these was the competition announced in 1784 by the Berlin Academy of Sciences. Joseph-Louis Lagrange had suggested the question of the foundations of the calculus, and the contest in turn resulted in two books on the subject, Simon L'Huilier's *Exposition élémentaire* and Lazare Carnot's *Réflexions sur la métaphysique du calcul infinitésimal.*[1]

Neither of these, however, was entirely satisfactory, and no one thought that either of them resolved the problem of the validity of the calculus. Most histories of mathematics credit Augustin-Louis Cauchy with providing the first 'reasonably successful rigorous formulation' of the calculus (Grabiner 1981, p. viii). This not only included a precise definition of limits, but aspects (if not all) of the modern theories of convergence, continuity, derivatives, and integrals. As Judith Grabiner has said in her detailed study of Cauchy, what he accomplished was nothing less than an 'apparent break with the past'. The break was also revolutionary, especially in terms of what Cauchy introduced methodologically. As Grabiner maintains (1981, p. 166), Cauchy 'was responsible for the first great revolution in mathematical rigour since the time of the ancient Greeks'.

This, presumably, is a revolution in mathematics that Michael Crowe, for example, would accept, for Cauchy's revolution was concerned with rigour on a metamathematical level affecting the foundations of mathematics. But, as will be argued here, changes in foundations cannot help but affect the structures they support, and in the case of Cauchy's new requirements for rigorous mathematical arguments in analysis, the infinitesimal calculus underwent a revolution in style that was soon to revolutionize its content as well.

In order to appreciate the sense in which Cauchy's work may be seen as revolutionary, it will help to remember that for most of the eighteenth century (with some notable exceptions) mathematicians like the Bernoullis, l'Hôpital, Taylor, Euler, Lagrange, and Laplace were interested primarily in results. The methods of the calculus were powerful and usually worked with remarkable success, although it should be added that these mathematicians were not oblivious to questions about *why* the calculus worked or whether there were acceptable foundations upon which to introduce its indispensible, but also most questionable, element—infinitesimals. Such concerns, however, remained for the most part secondary issues.

In the nineteenth century foundational questions became increasingly of interest and importance, in part for reasons that concern the sociology of mathematics involving both matters of institutionalization and professionalization. As mathematicians were increasingly faced with *teaching* the calculus, questions about how to define and justify limits, derivatives, and infinite sums, for example, became unavoidable.

Cauchy was not alone, however, in his concern for treating mathematics with greater conceptual rigour (at least when he was teaching at the École Polytechnique or writing textbooks like his *Cours d'analyse de l'École Polytechnique*).[2] Others, like Gauss and Bolzano, were concerned also with such problems as treating convergence more carefully, especially without reference to geometric or physical intuitions.[3] Whether or not Cauchy based his own rigorization of analysis upon his reading of Bolzano—as Ivor Grattan-Guinness (1970*a*) has suggested—or by modifying Lagrange's use of inequalities and the development of an algebra of inequalities—as Grabiner (1981, pp. 11, 74) argues—it remains true that Cauchy was a pioneer in writing textbooks that became models for disseminating the new 'rigorous' calculus, and that others soon began to work in the innovative spirit of Cauchy's arithmetic rigour.

Niels Henrik Abel was among the first to apply Cauchy's techniques in connection with his own important results on convergence. Somewhat later, Bernard Riemann revised Cauchy's theory of integration, and Karl Weierstrass further systematized Cauchy's work by carefully defining real numbers and emphasizing the crucial distinctions between convergence, uniform convergence, continuity, and uniform continuity.

Much of what Cauchy accomplished, however, had been anticipated by Lagrange, perhaps much as Barrow and others had prepared the way for Newton and Leibniz. For example, Lagrange had already given a rigorous definition of the derivative, and surprisingly, perhaps, he used the now-familiar method of deltas and epsilons. Actually, the deltas and epsilons were Cauchy's, but the idea was Lagrange's. The only symbolic difference is the fact that Lagrange used *D* (*donnée*) for Cauchy's epsilon and *i* (*indeterminée*) for Cauchy's delta. Both Lagrange and Ampère in fact used inequalities as an expedient method of proof, but Cauchy saw that they could also be used more essentially in definitions. As Grabiner has said, Cauchy extended this method to defining limits and continuity, and in doing so:

> . . . achieved exactly what Lagrange had said should be done in the subtitle of the 1797 edition of his *Fonctions analytiques*; namely the establishment of the principles of the differential calculus, free of any consideration of infinitely small or vanishing quantities, of limits or of fluxions, and reduced to the algebraic analysis of finite quantities. (Grabiner 1981, pp. 138–9)

If one considers Cauchy's new analysis in terms of structures, it seems clear that the new standards of proof it required not only changed the face but even the 'look' of analysis. Cauchy's rigorous *epsilontic* calculus was just as revolutionary as the original discovery of the calculus by Newton and Leibniz had been.

Again, as Grabiner has said:

It was not merely that Cauchy gave this or that definition, proved particular existence theorems, or even presented the first reasonably acceptable proof of the fundamental theorem of calculus. He brought all of these things together into a logically connected system of definitions, theorems, and proofs. (Grabiner 1981, p. 164)

In turn, the greater precision made possible by Cauchy's new foundations led to the discovery and application of concepts like uniform convergence and continuity, summability, and asymptotic expansions—none of which could be studied or even expressed in the conceptual framework of eighteenth-century mathematics. Names alone: Abel's convergence theorem, the Cauchy criterion, Riemann integrals, the Bolzano–Weierstrass theorem, the Dedekind cut, Cantor sequences—all are consequences and reflections of the new analysis.

Moreover, there is that important visual indicator of revolutions—a change in language reflected in the symbols so ubiquitously associated with the new calculus, namely deltas and epsilons, both of which first appear in Cauchy's lectures on the calculus in 1823.

In an extreme but telling example of the conceptual difference that separated Newton and Cauchy, at least when it came to conceiving of and justifying their respective versions of the calculus, Grabiner (1981, p. 1) tells the story of a student who asks what 'speed' or 'velocity' means, and is given an answer in terms of deltas and epsilons: 'The student might well respond in shock', she says, 'How did anybody ever think of such an answer?'

The equally important question is 'why'—*why* did Cauchy reformulate the calculus as he did? One answer, for greater clarity and rigour, seems obvious. By eliminating infinitesimals from polite conversation in calculus, and by substituting the arithmetic rigour of inequalities, Cauchy transformed a great part of mathematics, especially the language analysis would use and the standards by which its proofs would be judged, for the next century and more. Ironically, perhaps, in the infinitesimals that Cauchy had so neatly avoided, lay the seeds of yet another, contemporary revolution in mathematics.

5.2. NON-STANDARD ANALYSIS AS A CONTEMPORARY REVOLUTION

Historically, the dual concepts of infinitesimals and infinities have always been at the centre of crises and foundations in mathematics, from the first 'foundational crisis' that some, at least, have associated with discovery of irrational numbers (or incommensurable magnitudes) by the Pythagoreans,[4] to the debates between twentieth-century Intuitionists and Formalists—between the descendants of Kronecker and Brouwer on the one hand, and those of Cantor and Hilbert on the other. Recently, a new 'crisis' has been identified by the constructivist Errett Bishop (1975, p. 507):

> There is a crisis in contemporary mathematics, and anybody who has not noticed it is being willfully blind. *The crisis is due to our neglect of philosophical issues* . . . [Bishop's emphasis]

Arguing that formalists mistakenly concentrate on 'truth' rather than 'meaning' in mathematics, Bishop (1975, pp. 513–14) criticized non-standard analysis as 'formal finesse', adding that 'it is difficult to believe that debasement of meaning could be carried so far'. Not all mathematicians, however, are prepared to agree that there is a crisis in modern mathematics, or that Robinson's work constitutes any debasement of meaning at all.

Kurt Gödel, for example, believed that Robinson, 'more than anyone else', succeeded in bringing mathematics and logic together, and he praised Robinson's creation of non-standard analysis for enlisting the techniques of modern logic to provide rigorous foundations for the calculus using *actual* infinitesimals. The new theory was first given wide publicity in 1961, when Robinson outlined the basic idea of his 'non-standard' analysis in a paper presented at a joint meeting of the American Mathematical Society and the Mathematical Association of America.[5] Subsequently, impressive applications of Robinson's approach to infinitesimals have confirmed his hopes that non-standard analysis could serve to enrich 'standard' mathematics in substantive ways.

Using the tools of mathematical logic and model theory, Robinson succeeded in defining infinitesimals rigorously. He immediately saw this work not only in the tradition of others like Leibniz and Cauchy before him, but even as vindicating and justifying their views. The relation of their work, however, to Robinson's own research is equally significant (as Robinson himself realized), primarily for reasons that are of particular interest to the historian of mathematics.

This is not the place to rehearse the long history of infinitesimals. There is one historical figure, however, that especially interested Robinson, namely Cauchy, whose work provides a focus for considering the historiographic significance of Robinson's own work. In fact, following Robinson's lead, others like J. P. Cleave, Charles Edwards, Detlef Laugwitz, and Wim Luxemburg have used non-standard analysis to rehabilitate or 'vindicate' earlier infinitesimalists (Cleave 1971; C. H. Edwards 1979; Laugwitz 1975, 1985; Luxemburg 1975). Leibniz, Euler, and Cauchy are among the more prominent mathematicians who have been 'rationally reconstructed'—even to the point of their having had, in the views of some commentators, 'Robinsonian' non-standard infinitesimals in mind from the beginning. The most detailed and methodologically sophisticated of such treatments to date is that provided by Imre Lakatos.

5.3. LAKATOS, ROBINSON, AND NON-STANDARD INTERPRETATIONS OF CAUCHY'S INFINITESIMAL CALCULUS

In 1966 Imre Lakatos read a paper which provoked considerable discussion at the International Logic Colloquium meeting that year in Hanover. The primary aim of Lakatos's paper was made clear in its title: 'Cauchy and the continuum: The Significance of non-standard analysis for the history and philosophy of mathematics'.[6] Lakatos acknowledged his exchanges with Robinson on the subject of non-standard analysis, which led to various revisions of the working draft of his paper. Although Lakatos never published the article, it enjoyed a rather wide private circulation and eventually appeared after Lakatos's death (in 1974) in Volume 2 of his papers on mathematics, science, and epistemology (Lakatos 1978).

Lakatos realized that two important things had happened with the appearance of Robinson's new theory, indebted as it was to the results and techniques of modern mathematical logic. He took it above all as a sign that metamathematics was turning away from its original philosophical beginnings, and was growing into an important branch of mathematics (Lakatos 1966, p. 43). Now, more than twenty years later, this view seems fully justified.

The second claim Lakatos made, however, is that non-standard analysis revolutionizes the historian's picture of the history of the calculus. The grounds for this assertion are less clear—and subject to question. In the words of Imre Lakatos:

> Robinson's work . . . offers a rational reconstruction of the discredited infinitesimal theory which satisfies modern requirements of rigour and which is no weaker than Weierstrass's theory. This reconstruction makes infinitesimal theory an almost respectable ancestor of a fully fledged, powerful modern theory, lifts it from the status of pre-scientific gibberish, and renews interest in its partly forgotten, partly falsified history. (Lakatos 1966, p. 44)

Errett Bishop, somewhat earlier than Lakatos, was also concerned about the falsification of history, but for a different reason. He explained the 'crisis' he saw in contemporary mathematics in somewhat more dramatic terms:

> I think that it should be a fundamental concern to the historians that what they are doing is potentially dangerous. The superficial danger is that it will be and in fact has been systematically distorted in order to support the status quo. And there is a deeper

danger: it is so easy to accept the problems that have historically been regarded as significant as actually being significant. (Bishop 1975, p. 508)

Interestingly, Robinson sometimes made much the same point in his own historical writing. He was understandably concerned over the apparent triumph many historians (and mathematicians as well) have come to associate with the success of Cauchy–Weierstrassian epsilontics over infinitesimals in making the calculus 'rigorous'. In fact, one of the most important achievements of Robinson's work has been his conclusive demonstration—thanks to non-standard analysis—of the poverty of this kind of historicism. It is mathematically Whiggish to insist upon an interpretation of the history of mathematics as one of increasing rigour over mathematically unjustifiable infinitesimals—the '*cholera bacillus*' of mathematics, to use Georg Cantor's colourful description of infinitesimals.[7]

Robinson (1973), however, showed that there was nothing to fear from infinitesimals, and in this connection looked deeper, to the *structure* of mathematical theory, for further assurances: 'Number systems, like hair styles, go in and out of fashion—its what's underneath that counts.' This might well be taken as the leitmotiv of much of Robinson's entire career, for his surpassing interest since the days of his dissertation (written at the University of London in the late 1940s) was model theory, and especially the ways in which mathematical logic could not only illuminate mathematics, but have very real and useful applications within virtually all its branches. For Robinson, model theory was of such surpassing utility as a metamathematical tool because of its power and universality.

In discussing number systems, Robinson wanted to demonstrate, as he put it, that:

. . . the collection of all number systems is not a finished totality whose discovery was complete around 1600, or 1700, or 1800, but that it has been and still is a growing and changing area, sometimes absorbing new systems and sometimes discarding old ones, or relegating them to the attic. (Robinson 1973, p. 14)

Robinson, of course, was leading up to the way in which non-standard analysis had broken the bounds of the traditional Cantor–Dedekind understanding of the real numbers, just as Cantor and Dedekind had substantially transformed how continua were understood a century earlier in terms of Dedekind's 'cuts', or even more radically with Cantor's theory of transfinite ordinal and cardinal numbers (Dauben 1979).

There was an important lesson to be learned, Robinson believed, in the eventual acceptance of new ideas of number, despite their novelty or the controversies they might provoke. Ultimately, utilitarian realities could not be overlooked or ignored forever. With an eye on the future of non-standard analysis, Robinson was impressed by the fate of another theory devised late in the nineteenth century which also attempted, like those of Hamilton, Cantor, and Robinson himself, to develop and expand the frontiers of number.

In the 1890s Kurt Hensel introduced his now familiar *p*-adic numbers in order to investigate properties of the integers and other numbers. He also realized that the same results could be obtained in other ways. Consequently, many mathematicians came to regard Hensel's work as a pleasant game, but as Robinson (1973, p. 16) himself observed, 'many of Hensel's contemporaries were reluctant to acquire the techniques involved in handling the new numbers and thought they constituted an unnecessary burden'.

The same might be said of non-standard analysis, particularly in the light of Robinson's transfer principle that for any non-standard proof in $\mathbb{R}^*$ (the extended non-standard system of real numbers containing both infinitesimals and infinitely large numbers), there is a corresponding standard proof, complicated though it may be. Moreover, many mathematicians are clearly reluctant to master the logical machinery of model theory with which Robinson developed his original version of non-standard analysis. Thanks to Jerome Keisler (1976) and W. A. J. Luxemburg (1964), among others, non-standard analysis is now accessible to mathematicians *without* their having to learn mathematical logic as a prerequisite. For those who see non-standard analysis as a fad, no more than a currently pleasant game like *p*-adic numbers, the later history of Hensel's ideas should give sceptics an example to ponder. Today, *p*-adic numbers are regarded as co-equal with the reals, and have proved to be a fertile area of mathematical research.

The same has been demonstrated by non-standard analysis, for its applications in the areas of analysis, the theory of complex variables, mathematical physics, economics, and a host of other fields have shown the utility of Robinson's own extension of the number concept. Like Hensel's *p*-adic numbers, non-standard analysis can be avoided, although to do so may complicate proofs and render the basic features of an argument less intuitive.

What pleased Robinson about non-standard analysis (as much as the interest it engendered from the beginning among mathematicians) was the way it demonstrated the indispensability, as well as the power, of technical logic:

> It is interesting that a method which had been given up as untenable has at last turned out to be workable and that this development in a concrete branch of mathematics was brought about by the refined tools made available by modern mathematical logic. (Robinson 1973, p. 16)

Robinson had begun his career as a mathematician by studying set theory and axiomatics with Abraham Fraenkel at the Hebrew University in Jerusalem. Following his important work as an applied mathematician during the Second World War at the Royal Aircraft Establishment in Farnborough, he eventually went on to earn his Ph.D. from the University of London in

1949.[8] His early interest in logic was amply repaid in the applications he was able to make of logic and model theory, first to algebra and somewhat later to the development of non-standard analysis. As Simon Kochen has said of Robinson's contributions to mathematical logic and model theory:

Robinson, via model theory, wedded logic to the mainstreams of mathematics . . . At present, principally because of the work of Abraham Robinson, model theory is just that: a fully fledged theory with manifold interrelations with the rest of mathematics. (Kochen 1976, esp. p. 313)

If the revolutionary character of non-standard analysis is to be measured in textbook production and opposition to the theory, then it meets these criteria as well. The first textbook to teach the calculus using non-standard analysis, written by Jerome Keisler, was published in 1971, and opposition was expected. As G. R. Blackley warned Keisler's publisher (Prindle, Weber & Schmidt) in a letter when he was asked to review the new textbook before its publication:

Such problems as might arise with the book will be *political*. It is revolutionary. Revolutions are seldom welcomed by the established party, although revolutionaries often are. (Sullivan 1976, p. 375)

One member of the establishment who did greet Robinson's work with enthusiasm and high hopes was Kurt Gödel. Above all, Gödel recognized that Robinson's approach succeeded in uniting mathematics and logic in an essential, fundamental way. That union has proved to be not only of considerable mathematical importance, but of substantial philosophical and historical content as well.[9]

5.4. REVOLUTIONS IN MATHEMATICS

New discoveries, particularly those of revolutionary import in mathematics, provide new modes of thought within which more powerful and general results are possible than ever before. They do not come about, at least in the examples explored here, by a simple extension of the methods and mathematics in place at the time. Instead, when a true revolution has taken place, a significant part of the 'older' mathematics will come to be replaced or dramatically augmented by concepts and techniques that visibly change the vocabulary and grammar of mathematics. This is as true of Cauchy and the language of epsilontics that in turn made possible finer distinctions, for example, of continuity and convergence, as it is of Robinson's non-standard real numbers.

As mathematicians become comfortable with the new mathematics, learning its vocabulary and its techniques, their thinking is correspondingly transformed, and so is the mathematics they produce as a result. As its history has shown, mathematics is not a simple progression of results leading in a continuous, unbroken chain from Antiquity to the present. It has its own revolutionary moments, and these are as necessary to its progress as revolutions have been to all of science.

It is the revolutions that mathematics has experienced, the seismic episodes marked by the discovery of incommensurable magnitudes, the infinitesimal calculus, Cauchy's epsilontics, Cantor's transfinite set theory, and Robinson's non-standard analysis (to mention but a few), that have brought it from the simple, empirical levels of counting and geometry found in virtually all civilizations in the past, to the extraordinarily rich and powerful body of knowledge modern mathematics represents today.

Each generation, every age sets its own boundaries, limits, blinders to what is possible, to what is acceptable. Revolutions in mathematics take the next generation beyond what has been established to entirely new possibilities, usually inconceivable from the previous generation's point of view. The truly revolutionary insights have opened the mind to new connections and possibilities, to new elements, diverse methods, and greater levels of abstraction and generality. Revolutions obviously do occur *within* mathematics. Were this *not* the case, we would still be counting on our fingers.

NOTES

1. Lagrange also responded to the foundations problem, but did not submit a contribution of his own for the contest set by the Berlin Academy. Nevertheless, his own book, *Fonctions analytiques*, was designed to show how the calculus could be set on a rigorous footing. Although L'Huilier won the Academy's prize, the committee assigned to review the submissions complained that it had 'received no complete answer'. None of the contributions came up to the levels of 'clarity, simplicity, and especially rigour' which the committee expected, nor did any succeed in explaining how 'so many true theorems have been deduced from a contradictory supposition'. On the contrary, the committee was disappointed that none of the prize papers had shown why infinitesimals were acceptable at all. For details, see J. V. Grabiner (1981, pp. 40–3).
2. This was only the first of a series of books that Cauchy produced as a result of his lectures at the *École*. Among others, mention should be made of his *Résumé des leçons données à l'École Polytechnique sur le calcul infinitésimal* (1823), *Leçons sur les applications du calcul infintésimal à la géométrie* (1826–8), and *Leçons sur le calcul différentiel* (1829). For details of Cauchy's life and career, see the recent biography by B. Belhoste (1984), especially the section of Chapter 3 on 'L'enseignement à Polytechnique', pp. 79–85, where opposition to Cauchy's method of teaching the calculus is discussed.
3. What sets them apart, in fact, is that neither Gauss nor Bolzano was concerned with the rigour of their arguments for pedagogical reasons—their interests were both more technical and more philosophical.

4. There is a considerable literature on the subject of the supposed 'crisis' in mathematics associated with the Pythagoreans, notably H. Hasse and H. Scholz (1928). For recent surveys of this debate see J. L. Berggren (1984), Dauben (1984), D. H. Fowler (1987), and W. Knorr (1975).
5. Robinson first published the idea of non-standard analysis in a paper submitted to the Dutch Academy of Sciences (Robinson 1961).
6. Lakatos (1966). Much of the argument developed here is drawn from lengthier discussions of the historical and philosophical interest of non-standard analysis by J. Dauben (1988, 1989).
7. For Cantor's views, consult his letter to the Italian mathematician Vivanti, published in H. Meschkowski (1965, esp. p. 505). A general analysis of Cantor's interpretation of infinitesimals may be found in J. Dauben (1979, pp. 128–32, 233–8). On the question of rigour, refer to J. Grabiner (1974).
8. Robinson completed his dissertation, 'The metamathematics of algebraic systems,' at Birkbeck College, University of London, in 1949; it was published two years later as *On the metamathematics of algebra* (Robinson 1951). Several biographical accounts of Robinson are available, including G. Seligman (1979) and J. Dauben (1990).
9. On Gödel and the high value he placed on Robinson's work as a logician, consult Kochen (1976, p. 315), and a letter from Kurt Gödel to Mrs Abraham Robinson of 10 May 1974, quoted in Dauben (1990, p. 751).

6

Descartes's *Géométrie* and revolutions in mathematics

PAOLO MANCOSU

6.1. INTRODUCTION

In the aftermath of Kuhn's book *The structure of scientific revolutions* (1962), there has been a lively debate on whether Kuhn's picture of the growth of natural sciences can be applied to the growth of mathematics. Paradigm examples of such contributions are Crowe (1975), Mehrtens (1976), Dauben (1984), Dunmore (1989), and, of course, many of the chapters in this book. At the same time, Kuhn's work spurred interest in the historical development and uses of the notion of revolution in science and mathematics, a topic which was pursued by Cohen (see e.g. Cohen 1985). Any position which takes seriously talk of revolutions in mathematics (either to assert or to deny their existence) must of course address the issue of whether Descartes's *Géométrie* constitutes a revolution in mathematics.

The goal in this chapter is twofold. First, I shall present some of the most important results contained in the *Géométrie*, and investigate some of the assumptions on which the Cartesian project is founded. In the process of doing so I hope to acquaint the reader with some of the most important contributions (but by no means all of them!) to the literature on Descartes's *Géométrie*. Although the exposition as a whole aims at the non-specialist, the section on geometrical and mechanical curves should be of interest to the specialist as well. Secondly, I shall discuss the problem of whether Descartes's work constitutes a revolution in mathematics by discussing both pre-Kuhnian and post-Kuhnian debates on the issue.

6.2. DESCARTES'S *GÉOMÉTRIE*

The *Géométrie* was first published in 1637 as an appendix to the *Discours de la méthode*. The work was translated from French into Latin in 1649 by F. van Schooten, who published it with notes by him and F. de Beaune (Descartes 1649). A second Latin edition (Descartes 1659–61) also contained, in addition to the 1649 edition, contributions by De Witt, Hudde, Van Heurat, Bartolinus, and Schooten. These scientists can rightly be considered the first active group of 'Cartesian' mathematicians. (See Lenoir (1974, Chap. 4) for an analysis of this second Latin edition.)

Although the *Géométrie* is a short work (116 pages in the original French edition), its interpretation has given rise to several contrasting positions. However, before we venture on to the delicate issue of the interpretations of Descartes' achievements, it is better to go over the contents of the *Géométrie*. The work is divided into three books: Book I, 'Problems the construction of which requires only straight lines and circles'; Book II, 'On the nature of curved lines'; and Book III, 'On the construction of solid or supersolid problems'.

The first book contains a geometrical interpretation of the arithmetical calculus and a solution to Pappus's problem for four lines by a ruler-and-compass construction. The basic strategies of Cartesian analysis ('analytic geometry') occur for the first time in the solution to Pappus's problem.

The second book can be divided into four main sections. The first one has to do with a new classification of curves; this classification spells out the epistemological and ontological boundaries of the *Géométrie*. The second section contains a complete analysis of the curves required to solve Pappus's problem for four lines, and a special case for Pappus's problem for five lines. The third section presents the celebrated method of tangents (or better, of normals), and the fourth shows the utility of abstract geometrical considerations when applied to the 'ovals', a class of curves extremely useful for solving problems in dioptrics.

The third book contains an algebraic analysis of roots of equations. Here we find, among other things, Descartes's rule of signs, the construction of all problems of third and fourth degree through the intersection of a circle and a parabola, and a reduction of all such problems to the problem of the trisection of the angle or of the finding of two mean proportionals.

Of course, I cannot rehearse in detail all the contents of the *Géométrie*. I shall concentrate on some of its parts, and refer the reader to the literature mentioned in the bibliography for more detailed treatment. My discussion is divided into five sections. Section 6.2.1 presents Descartes's algebra of segments. Section 6.2.2 deals with Descartes's solution to Pappus's problem for four lines, and shows how Cartesian 'analytic geometry' is embedded in such a solution. Section 6.2.3 discusses Descartes's classification of curves and the foundational problems involved in the rejection of the mechanical curves from the domain of Cartesian geometry. Section 6.2.4 is about Descartes's method of tangents. Finally, I summarize some of the main features of Descartes's programme in Section 6.2.5. Admittedly, I devote little attention to Book III, which is in many ways less innovative with respect to the previous algebraic tradition.

6.2.1. Descartes's algebra of segments

The first book of the *Géométrie* opens with a bold claim:

Any problem in geometry can easily be reduced to such terms that a knowledge of the lengths of certain straight lines is sufficient for its construction. (SL 297)[1]

The first book exemplifies how all the problems of *ordinary* geometry (i.e. those that can be constructed by ruler and compass) can be constructed. In particular, constructing any such problem will turn out to be equivalent to the construction of the root of a second-degree equation. In order to show how this can be achieved, Descartes proceeds to explain 'how the arithmetical calculus is related to the operations of geometry' (SL 297). Arithmetical operations are addition, subtraction, multiplication, division, and extraction of root. Let a and b be line segments. Addition and subtraction of line segments are unproblematic. To explain multiplication, division, and extraction of root, Descartes makes use of proportion theory through the introduction of a line segment which functions as unity. Then ab, a/b and $\sqrt{a}$ are line segments which satisfy respectively the following proportions:

$$1:a=b:ab,$$

$$a/b:1=a:b,$$

$$1:\sqrt{a}=\sqrt{a}:a.$$

The construction of ab is as follows (see Fig. 6.1). Let AB = 1 be the unit segment, and assume we want to multiply the segment BD (denoted by a) by BC (denoted by b). This is done by joining A and C and drawing the line DE parallel to AC. Then $\mathrm{BE}=\mathrm{BD}\cdot\mathrm{BC}=ab$. The claim is easily verified by exploiting the proportionality between the triangles ABC and DBE. Similar constructions are given for a/b and $\sqrt{a}$. Descartes also introduces the notation a^2, a^3, and so on for powers of a.

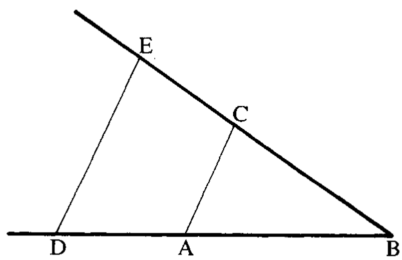

Fig. 6.1.

The main point of the geometrical interpretation of the arithmetical operations is to overcome the problem of dimensionality which limited to a great extent the previous geometrical work. Indeed, in ancient geometry as well as in Viète,[2] the multiplication of two lines is interpreted as an area, and the multiplication of three lines gives rise to a volume. But there is no corresponding interpretation for the product of four or more lines. We shall see how the new interpretation allows Descartes to solve in one fell swoop the extension of Pappus's problem to an arbitrary number of lines.

What follows now is nothing less than the general strategy for solving all geometrical problems. It can be roughly divided into three steps: naming, equating, and constructing.

Naming. One assumes the problem at hand to be already solved, and gives names to all the lines which seem needed to solve the problem.

Equating. Ignoring the difference between known and unknown lines, one analyses the problem by finding the relationship that holds between the lines in the most natural way. One then arrives at an equation (or several equations)—an expression in which the same quantity is expressed in two different ways. (Descartes knows, of course, that for a problem to be determinate there must be as many equations as there are unknown quantities.)

Constructing. The equation must then be constructed: its roots must be found (geometrically). If we now consider only those problems that can be constructed by ruler and compass, then the second step, Descartes claims, will lead to a second-degree equation and all that is left to do is to construct the roots of such an equation. Let us consider, for example, the construction of the root (the positive one!) in the equation $z^2 = az + b^2$, with a and b positive quantities. To construct z we consider the right triangle NLM with legs $\mathrm{LM} = b$ and $\mathrm{LN} = a/2$ (see Fig. 6.2). Now we produce MN to O so that NO = LN. Then OM is the root z we are looking for. Indeed, by Pythagoras's theorem, $\mathrm{MN}^2 - \mathrm{NL}^2 = \mathrm{LM}^2$, and since MN = OM − NL, by substitution $(\mathrm{OM} - \mathrm{NL})^2 - \mathrm{NL}^2 = \mathrm{LM}^2$, i.e. $\mathrm{OM}(\mathrm{OM} - 2\mathrm{NL}) = \mathrm{LM}^2$. But $2\mathrm{NL} = a$ and $\mathrm{LM}^2 = b^2$. Thus, letting $z = \mathrm{OM}$, we have $z(z - a) = b^2$, i.e. $z^2 = az + b^2$.

Descartes concluded the section by mentioning that all the problems of *ordinary* geometry can be constructed using the above. This, says Descartes, could not have been known to the ancient geometers since the length and order of their work shows that they proceeded at random rather than by method. Had they had a method, they would have been able to solve Pappus's problem, which neither Euclid nor Apollonius nor Pappus were able to solve in full generality. I now turn to Pappus's problem and to its solution in the *Géométrie*.

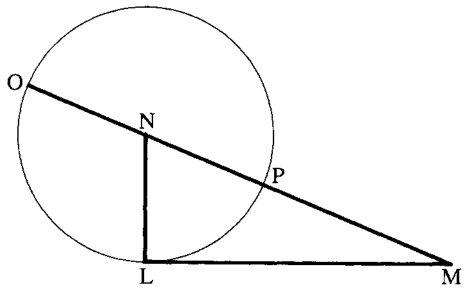

Fig. 6.2.

6.2.2. Pappus's problem for four lines and its solution

Descartes's claim to have achieved in mathematics what neither the ancients nor the moderns had obtained, rested on his solution of a problem stated by Pappus and left unsolved by ancient and modern mathematicians alike. The solution to this problem plays the role of a paradigm example of how to solve all geometrical problems.

Statement of Pappus's problem for four lines.[3] Suppose we are given four lines in position, say AB, AD, EF, GH (see Fig. 6.3). It is required to find a point C such that, given angles $\alpha, \beta, \gamma, \delta$, lines can be drawn from C to the lines AB, AD, EF, GH making angles α, β, γ, δ, respectively, such that $CB \cdot CF = CD \cdot CH$. Moreover, it is required to find the locus of all such points C, i.e. 'to know and to trace the curve containing all such points' (SL 307).

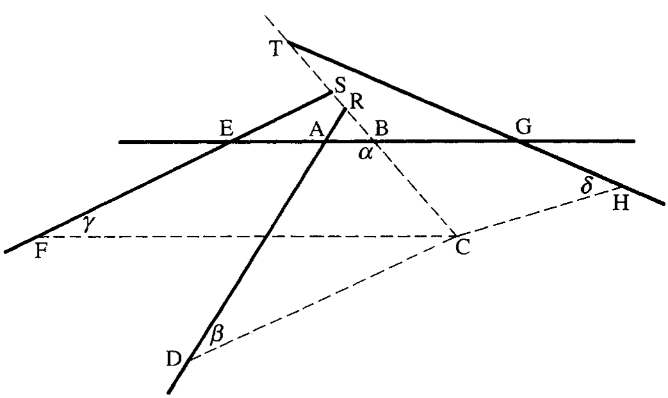

Fig. 6.3.

The solution given by Descartes proceeds as follows. From the various lines, AB and BC are chosen as principal lines in terms of which all the other lines are expressed. In other words, CB, CF, CD, and CH will be expressed in terms of AB, BC, and other data of the problem. According to the general strategy presented in Section 6.2.1, we begin by naming. Let the segments AB and BC be denoted respectively by x and y. Also let $EA=k$ and $AG=l$. The segments k and l are known, since the four lines are specified. For the same reason we know all the angles of the triangles ARB, DRC, ESB, FSC, BGT, TCH; or, which is the same, we know all the ratios of the sides of these triangles. We now set up the equations. Let

$$AB/BR=z/b, \tag{6.1}$$

$$CR/CD=z/c, \tag{6.2}$$

$$BE/BS=z/d, \tag{6.3}$$

$$CS/CF=z/e, \tag{6.4}$$

$$BG/BT=z/f, \tag{6.5}$$

$$TC/CH=z/g, \tag{6.6}$$

where z, b, c, d, e, f, g are all constants.

Since $AB=x$, (6.1) becomes $x/BR=z/b$, and thus $BR=bx/z$. Consequently,

$$CR=CB+BR=y+(bx/z). \tag{6.7}$$

By (6.2), we can write $CD=c\cdot CR/z$ and, by (6.7),

$$CD=(cy/z)+(cbx/z^2).$$

Since $EA=k$ we have $BE=EA+AB=k+x$. By (6.3) $(k+x)/BS=z/d$. Thus $BS=(dk+dx)/z$, and

$$CS=BS+CB=((dk+dx)/z)+y=(dk+dx+yz)/z. \tag{6.8}$$

By (6.4) $CF=CS\cdot e/z$ and by (6.8) we get

$$CF=(ezy+dek+dex)/z^2.$$

Since $AG=l$ and $BG=l-x$, by (6.5) we obtain $BT=(f(l-x))/z$. Thus

$$CT=BC+BT=y+((fl-fx)/z)=(yz+fl-fx)/z. \tag{6.9}$$

By (6.6), $CH=g\cdot CT/z$. Thus, by (6.9),

$$CH=(gzy+gfl-gfx)/z^2.$$

We have therefore expressed CB $(=y)$, CD, CF, and CH in terms of the