principal lines and the other data of the problem. If we now set, according to the problem, CB · CF = CD · CH, we obtain an equation of degree two in x and of degree two in y. This completes the second step of the solution. The final step in the solution is constructing the problem. To this end we assign an arbitrary value to y, and thus we obtain an equation which is quadratic in x. The construction of the solution of a quadratic equation is not a problem as it has already been shown how to carry it out. The locus of points is then constructed by taking arbitrary values for y and constructing the corresponding values for x:

If then we should take successively an infinite number of different values for the line y, we should obtain an infinite number of values for the line x, and therefore an infinity of different points, such as C, by means of which the required curve could be drawn. (SL 313)

Pappus's problem can be generalized to an arbitrary number n of lines ($n \geq 3$). The case for three lines is simply the case for four lines with the third and fourth lines coinciding, i.e. $CB \cdot CF = CD^2$. Let $n \geq 4$. Assume that $l_1, \ldots, l_n$ are lines given in position, and $\beta_1, \ldots, \beta_n$ are fixed angles. Let s denote an arbitrary line segment. The problem of Pappus for n lines consists of finding the locus of points C such that if $d_1, \ldots, d_n$ are the segments drawn from C to $l_1, \ldots, l_n$ making angles $\beta_1, \ldots, \beta_n$, then

$$\begin{cases} d_1 \cdot \ldots \cdot d_k = d_{k+1} \cdot \cdots \cdot d_n & \text{if } n = 2k, \\ d_1 \cdot \ldots \cdot d_k = d_{k+1} \cdot \cdots \cdot d_{2k-1} \cdot s & \text{if } n = 2k-1. \end{cases}$$

The solution for four lines easily generalizes to n lines. Indeed, each distance d_i from C(x, y) to the line l_i making an angle β_i is expressed by $\pm A_i x \pm B_i y \pm C_i$. Thus the equation of the general locus is

$$\begin{cases} \prod_{i=1}^{k} (\pm A_i x \pm B_i y \pm C_i) = \prod_{i=k+1}^{2k} (\pm A_i x \pm B_i y \pm C_i) & \text{if } n = 2k, \\ \prod_{i=1}^{k} (\pm A_i x \pm B_i y \pm C_i) = \left[\prod_{i=k+1}^{2k-1} (\pm A_i x \pm B_i y \pm C_i) \right] \cdot s & \text{if } n = 2k-1. \end{cases}$$

Thus for $2k-1$ and $2k$ lines, we end up with an equation of degree k in x and degree k in y.

It should be noted that the generalization of the problem to an arbitrary number of lines was already in Pappus. However, the ancients could not make much sense of it since they could not make sense geometrically of the product of four or more lines.[4] Descartes's new calculus, by interpreting products of line segments as yielding line segments, allows him to bypass the issue with finesse.

6.2.3. Descartes's classification of curves

In the opening part of Book II, Descartes recalls approvingly ('The ancients have very rightly remarked . . .' (SL 315)) Pappus's distinction of problems between plane, solid, and linear problems. Plane problems are those that can be constructed by means of straight lines and circles; solid problems those that can be constructed by making use of conics; and linear problems those which require more composite lines. This last category of problems is called linear 'for lines other than those mentioned are used in the construction, which have a varied and more intricate genesis, such as the spirals, the quadratrices, the conchoids, and the cissoids, which have many marvellous properties' (Pappus 1933, p. 38). However, Descartes continues, this latter category must be further analysed:

I am surprised, however, that they did not go further, and distinguish between different degrees of these more complex curves, nor do I see why they called the latter mechanical, rather than geometrical. (SL 315)

Descartes then endeavoured to find an explanation for why the ancients made the distinction between geometrical and mechanical curves the way they did. In the process of doing so, he claimed that there were misgivings among the ancients about whether to accept the conic sections as fully geometrical. In any case, he suggests that the ancients had put together in the same category spirals, quadratrices, conchoids, and cissoids[5] because in their inquiries they happened to encounter first the first two, which are truly mechanical, and only afterwards the conchoid and the cissoid which are, in Descartes's opinion, acceptable.

Perhaps what stopped the ancient geometers from admitting curves more complex than the conic sections is that the first curves to which their attention was attracted happened to be the spiral, the quadratrix, and similar curves, which really belong only to mechanics, and are not among those that I think should be included here, since they must be conceived of as described by two separate movements whose relation does not admit of exact determination. Yet they afterwards examined the conchoid, the cissoid, and a few others which should be accepted; but not knowing much about their properties they took no more account of these than of the others. (SL 316–17)

This section of Descartes's text was analysed very carefully by Molland, who concluded that Descartes's exposition was 'a misconstrual of the ancient distinction between geometrical and instrumental'. For example, in connection with the passage quoted above, Molland says:

His third attempted explanation was that the spiral and quadratrix, which were not geometrical, were discovered first and only afterwards the acceptable conchoid and cissoid. But, as we have seen, there was no ancient compunction about admitting the spiral and little about the quadratrix, and there could well have been more doubt about the geneses of the conchoid and cissoid. (Molland 1976, p. 35)

However, the misconstrual was instrumental, concludes Molland, in that 'his faulty exegeses allowed him [Descartes] to introduce more naturally his own basis for geometry'. What Molland's analysis leaves unanswered is whether Descartes is completely responsible for the misconstrual of whether he is sharing a reading of the ancients which was commonplace in the contemporary mathematical literature. I shall have something to say about this below when commenting on several passages from Clavius. Two questions await us next. Which curves did Descartes admit? Why did he reject others? These two questions are answered in the next two sections.

Geometrical and mechanical curves

Descartes's proposal is that by 'geometrical' should be understood what is precise and exact, and by 'mechanical' what is not so. The curves to be admitted in geometry are given by a kinematical criterion:

> . . . nevertheless, it seems very clear to me that if we make the usual assumption that geometry is precise and exact, while mechanics is not; and if we think of geometry as the science which furnishes a general knowledge of the measurements of all bodies, then we have no more right to exclude the more complex curves than the simpler ones, provided they can be conceived of as described by a continuous motion or by several successive motions, each motion being completely determined by those which precede; for in this way an exact knowledge of the magnitude of each is always obtainable (SL 316)

The kind of regulated continuous motions that Descartes has in mind are illustrated by the generation of curves provided by the machine shown in Fig. 6.4. It consists of several rulers linked together. YZ is fixed, and Y is a pivot so that XY can rotate. Perpendicular to XY we have a fixed ruler BC and sliding rulers DE, FG (etc.—the machine could be extended indefinitely). Perpendicular to YZ are the sliding rulers CD, EF, GH (etc.). In the initial position YX coincides with YZ. As YX rotates counterclockwise, the fixed ruler BC pushes the sliding ruler CD which, in turn, pushes the sliding ruler DE (etc.). All the curves described by the (moving) points B, D, F, H (etc.) are admissible and are called geometrical. This is by no means the only type of machine considered by Descartes, and in fact he adds that many similar types of machine could be considered. However, a unifying feature of all the curves generated by such instruments is that they all have an algebraic equation:

> I could give here several other ways of tracing and conceiving a series of curved lines, which would be more and more complex by degrees to infinity, but I think the best way to group together all such curves and then classify them in order, is by recognizing the fact that all points of those curves which we may call 'geometric', that is, those which admit of precise and exact measurement, must bear a definite relation to all points of a straight line, and that this relation must be expressed by means of a single equation. (SL 319)

This allows Descartes to classify curves by making use of the degree of their

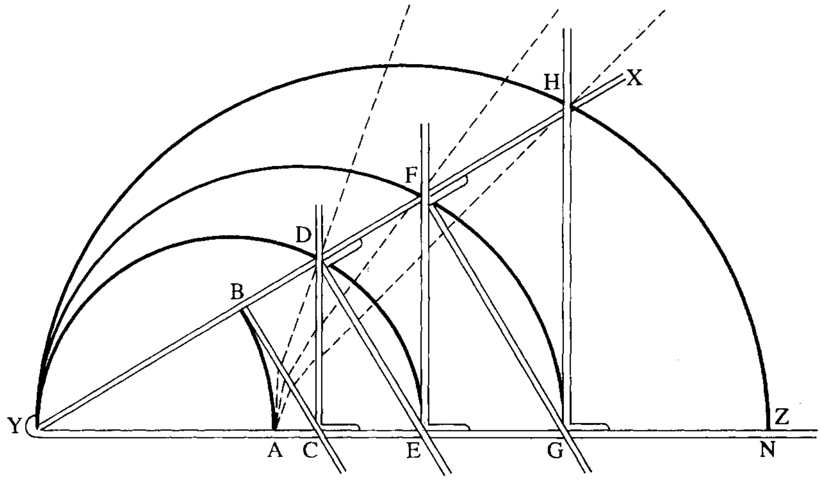

Fig. 6.4.

equation. Descartes classifies curves by gender, curves of gender 1 being the circle and the conics, curves of gender 2 those which have equations of degree 3 or 4, curves of gender 3 those which have equations of degree 5 or 6, and so forth. (See Grosholz (1991, Chap. 2) for an analysis of the notion of gender.) It should be remarked that Descartes never says explicitly that all the algebraic equations define a geometrical curve although, as Bos (1981) has argued, he implicitly assumed this.

We have already encountered two different types of curve construction: by points, as in the solution to Pappus's problem, and by regulated motions. However, not all motions or all pointwise constructions are to be allowed in geometry. Let us begin with the unacceptable motions. I have quoted above a passage where Descartes claims that the quadratrix and the spiral should be rejected because they are generated by two different motions 'between which there is no relation (*raport*) that can be measured exactly'. This is exactly the same criticism that was raised, according to Pappus, by Sporus (third century AD) against the use of the quadratrix in the squaring of the circle.

The quadratrix is a curve which is generated by the intersection of two segments, one moving with uniform rectilinear motion and the other with uniform circular motion. Let ABCD be a square, and BED the quadrant of a circle with centre A (see Fig. 6.5). Let AB rotate uniformly clockwise towards AD, and let BC move with uniform rectilinear motion towards AD, keeping parallel to AD, in such a way that the two lines AB and BC start moving at the same time and end their motion coinciding with AD at the same time. The locus of points described by the intersection of the two moving segments is the quadratrix.

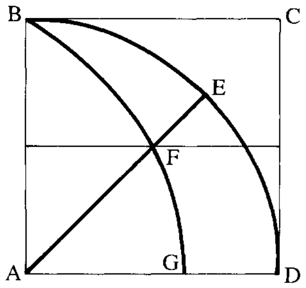

Fig. 6.5.

The quadratrix can be used to trisect the angle but its principal use, as the name indicates, was in attempts to square the circle. However, this use was severely criticized, even in ancient times. In particular, Pappus recalls approvingly Sporus's objections that, in order to adjust the speed of the motions as required and to determine the point G on the quadratrix, one already needs to know what is sought—the quadrature of the circle.[6] Pappus concluded by stating that the construction of the line belonged to mechanics.

One possible way out of the situation could have been to attempt a construction of the quadratrix which required no independent motions and which could be considered more geometrical.[7] This attempt was made by Clavius, in his *Commentaria in Euclidis elementa geometrica*, in an appendix to Book VI entitled 'De mirabili natura lineae cuiusdam inflexae, perquam et in circulo figura quotlibet laterum aequalium inscibitur, & circulus quadratur, & plura alia scitu iucundissima perficiuntur' (Clavius 1591, p. 296).[8] I claim this text to be the source of the reflections on pointwise constructions contained in the *Géométrie*. In it Clavius proposes a pointwise construction of the quadratrix similar to those given for the conic sections, which is therefore, Clavius claims, geometric:

> And although the said authors endeavoured to describe such line [the quadratrix] by two imaginary motions of two straight lines, in which thing they beg the principle, so that on that account the line is rejected by Pappus as useless and not describable; however, we will describe it *geometrically* through the determination of however many of its points through which it must be drawn, just as it is commonly done in the description of the conic sections. (Clavius 1591, p. 296)[9]

The construction given by Clavius can be summarized as follows. Divide the arc DB and the sides AD and BC into 2^n equal parts for n as large as you please (the larger the n, the more accurate the description). Figure 6.6 shows the situation for $n = 3$. Thus we have seven points on DB, AD, and BC. Connect by dashed lines the corresponding points on AD and BC, and the point A to the seven points on the arc DB. The points of intersection are points on the

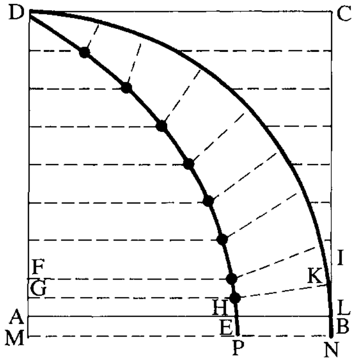

Fig. 6.6.

quadratrix. By refining the partition on the arc and on the sides, Clavius claims to approximate the quadratrix more and more precisely. Moreover, he implicitly assumes that he will be able, with the single exception of the point E, to obtain in this fashion all the points on the quadratrix. He continues by noticing that E cannot be found in such a way (i.e. geometrically) since, when the two motions are completed, the two segments no longer intersect. In order to take care of this case, he resorts to a trick.[10] Consider the segment AF on AD, and bisect it continually until we reach a very small part of it, say AG. Similarly, bisect the arc BI in the same number of parts, and let BK be the arc thus obtained. Now, construct BL, BN, AM equal to AG. Connect G and L, M and N, A and K by dashed lines. The segment AK intersects GL at H. If MP is taken to be equal to GH, and the quadratrix is extended uniformly to P, then the curve must pass through E. Indeed, Clavius argues, one only needs to 'squeeze' E between H and P to an arbitrary degree of accuracy.

Clavius also offers a different construction in which the approximation does not require the curve to be extended below the side AB, and in which all the lines in the construction meet at right angles (whereas in the previous construction the radii originating from A intersected the segments originating on AD at different angles).

It should be remarked that Clavius does not realize that there are an (uncountable) infinity of points (not just E) that can only be approximated, since the construction he has given will produce a (countably) dense set of points, but not all the points on the quadratrix. However, Clavius is convinced he has given a geometric construction of the curve which uniformly constructs all the points on the curve:

This is therefore the description of the quadratrix, which in a certain sense can be called geometrical, just as the description of the conic sections, which are also made by points, as are handed down by Apollonius, are called geometrical, although in truth they are more liable to error than our description is. This is a consequence of the determination of several proportional lines which are necessary for their description and which is not an issue in the description of the quadratrix. On which account, unless someone wanted to reject the whole doctrine of conic sections as useless and not geometrical (which I think nobody will do since the best Geometers employed the conic sections in their demonstrations. [. . .]) he is compelled to admit that our description of the quadratrix is in a certain sense geometrical. Add that the conchoid, through which Nicomedes sharply searches two mean proportional lines, is also described by points, as we say in the book of mensurations. (Clavius 1591, p. 297)[11]

It is hard to overestimate the above passage for an understanding of what Descartes is up to in Book II of the *Géométrie*. The following points should be stressed:

(1) Clavius claims his construction by points to be geometrical;

(2) indeed, he claims it to be more geometrical than the construction by points given for the conics, which is more liable to error than his;

(3) however, constructions of conic sections are to be considered geometrical;

(4) he stresses the similarity between his construction and that given for the conchoid.[12]

Notice that Clavius, following Pappus, rejects the construction by double motion as mechanical, and that by arguing for the geometrical nature of constructions involving conic sections he unwittingly acknowledges that the point might be challenged. This is in line with Descartes's rejection of the quadratrix and with Descartes's doubts as to whether the ancients accepted as geometrical the solutions obtained by means of conic sections.

A consequence of points (1) to (4) is that, in a certain sense, the quadrature of the circle can be effected geometrically. However, as I shall argue in the next section, Descartes could not accept this consequence. In the following passage, although not mentioning Clavius, Descartes claims that there is a difference between the construction by points for the geometrical curves (such as the conics and the conchoid) and that used for the spiral and similar curves (i.e. the quadratrix). Only special points can be constructed on the latter curves.

It is worthy of note that there is a great difference between this method in which the curve is traced by finding several points upon it, and that used for the spiral and similar curves. In the latter not any point of the required curve may be found at pleasure, but only such points as can be determined by a process simpler than that required for the composition of the curve. Therefore, strictly speaking, we do not find any one of its points, that is, not any one of those which are so peculiarly points of this curve that they cannot be found except by means of it. On the other hand, there is no point on these curves which supplies a solution for the proposed problem that cannot be determined by the method I have given. And since this way of tracing a curved line by determining several of its points at random, applies only to those curves which can also be described by a regular and continuous motion, we should not reject it entirely from geometry. (SL 339–40)

Thus Descartes accepts (3) but rejects claims (1), (2), and (4) of Clavius's argument. Of course, the reader might still question whether Descartes had in mind Clavius's passages when writing the above. I shall show this to be the case in the next section.

In conclusion to this section let me remark, following Bos, that Descartes holds that the three classes of curves generated by the following three categories are extensionally equivalent (although some of the implications are only implicit in the *Géométrie*):

(1) curves generated by regulated continuous motions;

(2) curves generated by (uniform) pointwise construction;

(3) curves given by an algebraic equation.

Mechanical curves and the quadrature of the circle

The extent of mechanical curves known to Descartes at the time of the publication of the *Géométrie* was very limited. Indeed, in the *Géométrie* he explicitly mentions only the quadratrix and the spiral as examples of mechanical curves. We have seen that Descartes rejects them because 'they are considered as described by two separate movements, between which there is no relation (*raport*) that can be measured exactly'. Moreover, he mentions that only special points can be constructed on the mechanical curves. Bos (1981, p. 325) remarks that 'there is no evidence that Descartes before 1637 actively studied transcendental curves other than the quadratrix and the spiral'. However, this is not correct. Descartes studied at least one other transcendental curve before 1637, the cylindrical helix, and explicitly rejected it as mechanical.

In addition to the two criteria for rejecting mechanical curves from geometry mentioned above, Descartes invokes another criterion when discussing construction by strings:

For although one cannot admit [in geometry] lines which are like strings, that is, which are sometimes straight and sometimes curved, because the proportion between straight lines and curved lines is not known and I also believe it cannot be known by men, so one cannot conclude anything exact and certain from it. (SL 340–1)[13]

The idea that there is no proportion between curved and straight lines, or motions, goes back at least to Aristotle's *Physics*. In Bos's opinion, the Aristotelean dogma is the very foundation of Descartes's distinction between geometrical and mechanical curves:

Thus the separation between the geometrical and non-geometrical curves, which was fundamental in Descartes's vision of geometry, rested ultimately on his conviction that proportions between curved and straight lengths cannot be found exactly. This, in fact, was an old doctrine, going back to Aristotle. The central role of the incomparability of straight and curved in Descartes's geometry explains why the first rectifications of algebraic (i.e. for Descartes geometrical) curves in the late 1650s were so revolutionary: they undermined a cornerstone of the edifice of Descartes's geometry. (Bos 1981, pp. 314–15)

Although I do not deny that Descartes (and many of his contemporaries) believed in the Aristotelean dogma,[14] I cannot but puzzle over the fact, that, although the algebraic rectification of algebraic curves was essential in destroying the Aristotelean dogma, it did not really undermine the foundations of Descartes's *Géométrie*; nor, to my knowledge, did anybody at that time claim this to be the case. This suggests that the real motivation and foundation for Descartes's exclusion of the spiral, quadratrix, 'and the like', may be based on something else. I suggest this something else to be Descartes's *parti pris*—that the quadrature of the circle is impossible geometrically. The following passage, taken from a letter to Mersenne dated 13 November 1629, points to the likelihood of my hypothesis:

Mr. Gaudey's invention is very good and very exact in practice. However, so that you will not think that I was mistaken when I claimed that it could not be geometric, I will tell you that it is not the cylinder which is the cause of the effect, as you had me understand and which plays the same role as the circle and the straight line. The effect depends on the helix which you had not mentioned to me, which is a line that is not accepted in geometry any more than that which is called quadratrix, since the former can be used to square the circle and to divide the angle in all sorts of equal parts as precisely as the latter can, and has many other uses as you will be able to see in Clavius's commentary to Euclid's *Elements*. For although one could find an infinity of points through which the helix or the quadratrix must pass by, however one cannot find geometrically any one of those points which are necessary for the desired effects of the former as well as of the latter. Moreover, they cannot be traced completely except by the intersection of two movements which do not depend on each other, or better the helix by means of a thread [*filet*] for revolving a thread obliquely around the cylinder it describes exactly this line; but one can square the circle with the same thread, so precisely that this will not give us anything new in geometry. (AT, Vol. I, pp. 70–1)[15]

In this long and dense passage, which leaves no doubt as to Descartes's knowledge of Clavius's work on the quadratrix, Descartes considers explicitly the cylindrical helix which he does not mention in the *Géométrie*. Moreover, he gives several reasons for excluding curves like the quadratrix and the helix. We are already familiar with some of them. Both curves are such that only special points can be constructed on them. The quadratrix is excluded on account of its being generated by two independent motions, and the helix is excluded because it is generated by a *filet* ('thread'). Descartes *ultimately* excludes them because both curves allow us to square the circle (*pource qu'elle sert a quarrer le cercle*; he also mentions once the division of an angle into arbitrary parts). He adds that these curves do not give us anything new in geometry. In a sense they 'beg' the question. This is simply Pappus's criticism.

If we now consider, in addition to the points already made, that the curves which had been used in Antiquity (and which passed down to the seventeenth century) in attempts to square the circle were the spiral, the quadratrix, and the cylindrical helix (as Iamblichus reports, quoted in Heath (1921, Vol. I, p. 225)), I think one can confidently claim that one of the unifying criteria which is at work in Descartes's mind, when he excludes the mechanical curves, is that they can be used to square the circle.

The point about the impossibility of squaring the circle is reiterated in a letter to Mersenne dated 31 March 1638. Descartes states:

> For, in the first place, it is against the geometers' style to put forward problems that they cannot solve themselves. Moreover, some problems are impossible, like the quadrature of the circle, etc. (AT, Vol. II, p. 91)

How could Descartes have been so confident? It was not until 1882 that Lindemann was able to prove that there is no algebraic quadrature of the circle. Moreover, the discussion on whether the quadrature of the circle was possible was still very lively in Descartes's period. For example, Mersenne devotes the 'Question XVI' of his *Questions théologiques* (1634) to the topic 'La quadrature du cercle est-elle impossible?'. He remarks how split is the mathematical world over this very question:

> This problem is extremely difficult, for one can find excellent geometers who claim that it is not possible to find a square whose surface is equal to that of the circle, and others who claim the opposite. (Mersenne 1634, p. 275)

Mathematicians have often been divided over the status of various mathematical propositions (just think of the problem of the independence of the axiom of choice, or the continuum hypothesis from Zermelo–Fraenkel set theory). But what is surprising is to find Descartes basing his whole 'foundational' enterprise on the assumption that the circle cannot be squared. How did he arrive at such a conclusion?

We have evidence that Descartes worked on the problem of squaring the circle. In the tenth volume of the Adam–Tannery edition there is a fragment (number 6, dated 1628 or earlier) which purports to give the best way to effect the quadrature of the circle. Interestingly, the quadrature of the circle is obtained by constructing an infinite sequence of points which converges towards a certain point. What I want to emphasize here is that we have an approximation argument to a point which is akin to the determination of the point E in Clavius's argument for the pointwise construction of the quadratrix.[16] Since the fragment claims to have provided the best possible quadrature of the circle, it is quite likely that Descartes convinced himself that no quadrature of the circle was possible unless it involved infinite approximations of the type we have considered.

How does the criterion that there is no exact relation between curved and straight lines relate to the impossibility of the quadrature of the circle being effected geometrically? The quadrature of the circle is equivalent (by Archimedes' proof) to the rectification of the circumference. Thus what suffices for Descartes's exclusion of the mechanical curves he actually mentioned before 1637 and in the *Géométrie* is that there is no exact proportion between the circumference and the radius—that the circumference cannot be algebraically rectified. A correct guess, but an unproven one at that. However, this is why the algebraic rectification of curves leaves unthreatened the Cartesian distinction stated in the *Géométrie* between geometrical and mechanical curves. Only an algebraic rectification of the circumference would have destroyed the rationale for Descartes's position.

6.2.4. Descartes's tangent method

The class of curves that Descartes called geometrical turned out to be an extremely natural and fruitful one to isolate. Moreover, the fact that each such curve can be described by an algebraic equation allows Descartes to solve in all its generality the problem of drawing a tangent to an arbitrary point on each such curve, or—which is the same—drawing a normal to each point. Let us follow Descartes's example. Suppose we are given an ellipse having the equation

$$x^2 = ry - (ry^2/q), \tag{6.10}$$

where r is the latus rectum and q the major axis.

We wish to draw a normal at an arbitrary point C on the curve (see Fig. 6.7). According to the general strategy for solving problems described in Section 6.2.1, we begin by considering the problem solved and by naming the lines in question. Let $AM = y$, $CM = x$; the normal $PC = s$, $PA = v$, and $PM = v - y$. We now look for the relevant equations. Since CMP is a right triangle, we have

$$s^2 = x^2 + v^2 - 2vy + y^2. \tag{6.11}$$

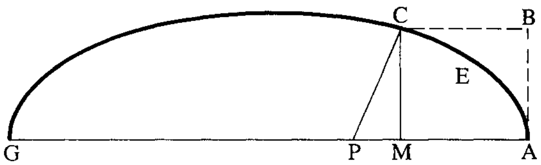

Fig. 6.7.

We impose the condition that the point C must lie on the curve. From (6.10) and (6.11), we obtain

$$ry - (ry^2/q) = s^2 - v^2 + 2vy - y^2,$$

and by simple algebraic manipulations

$$y^2 = [q(2v - r)/(q - r)]y + [q(s^2 - v^2)]/(q - r). \quad (6.12)$$

From (6.12), we proceed to determine r or s. We must exploit the other piece of information at our disposal, that CP must be normal. If CP is not normal then the circle with radius PC will cut the curve in C as well as in another point E different from C. The condition of normality is thus equivalent to the condition that the two points C and E must coincide in one point or, algebraically, there is a double root of equation (6.12). If (6.12) has a double root, say e, then it is of the form $(y - e)^2 = 0$, that is

$$y^2 = 2ye - e^2. \quad (6.13)$$

By equating coefficients in (6.12) and (6.13), we obtain $(2qv - qr)/(q - r) = 2e$, and solving for v we obtain $v = [2e(q - r) + qr]/2q$, and since $e = y$, $v = [y(q - r)/q] + (r/2)$.

The third step of the solution is constructing v. However, the construction is routine and Descartes leaves it out. What should be remarked upon is the extreme generality of the method (which applies to any algebraic curve), the essential role played by the equation of the curve, and the absence of infinitesimal considerations in the solution to the problem.[17]

6.2.5. Some general features of the *Géométrie*

Descartes's programme

The first book of the *Géométrie* has shown through the paradigmatic solution of Pappus's problem for four lines the main strategy for solving problems. Which problems can be solved? Those which admit a geometrical solution, a solution which makes use only of geometrical curves. The class of geometrical curves is described (by no means univocally) in Book II, which delimits the ontological domain of the *Géométrie*. But the problems treated in Book I were plane problems, ones which could be solved by the intersection of straight lines and circumferences. It is in Book III that Descartes shows how the approach is to be generalized, not only to solid problems but to arbitrary problems. An acceptable solution is obtained only when we employ the simplest curve that can be used to solve the problem. Descartes is explicit about this:

> While it is true that every curve which can be described by a continuous motion should be accepted in geometry, this does not mean that we should use at random the first one that we meet in the construction of a given problem. We should always choose with care the simplest curve that can be used in the solution of a problem. (SL 369–70)

The criterion of simplicity is purely algebraic. The complexity of the curve is measured by the degree of the equation by which they can be expressed. Thus in Book III Descartes shows how solid and supersolid problems (i.e. of degree 3 or 4) can be solved through the intersections of a circle and a parabola (a curve of degree 2), and in general how problems of degree $2n-1$ and $2n$ can be solved through the intersection of a circle and a curve of degree n. The grand vision of Descartes consists of a classification of all geometrical problems by means of the simplest curves that can be used to solve them.[18] This, in turn, allows Descartes to claim that his method is better than any other that has been proposed, and that his work marks, if I am allowed the expression, the 'end of geometry'. Writing to Mersenne in December 1637, he says:

> Moreover, having determined as I have done in every type of question all that can be done, and shown the means to do it, I claim that one should not only believe that I have done something more than those who have preceded me but also one should be convinced that our descendants will never find anything in this subject that I could not have found as well as they, if I only bothered to look for it. (AT, Vol. II, p. 480)

Algebra and Geometry

The issue of the relationship between analytic objects (equations) and geometrical objects (curves) is crucial for evaluating the *Géométrie* and has given rise to two different interpretative positions in the literature. Bos, Boyer, Grosholz, Lachterman, and Lenoir have claimed that algebra is simply a tool in the economy of the *Géométrie*. For example, Bos has argued at length that the equation of a curve is not allowable for Descartes as a genuine representation of a curve:

> The conclusion from these facts must be that for Descartes the equation of a curve was primarily a tool and not a means of definition or representation. It was part of a whole collection of algebraic tools which in the *Géométrie* he showed to be useful for the study of geometrical problems. The most important use of the equation was in classifying curves into classes and in determining normals to curves. Here the equation must actually be written out. In many other cases Descartes could get through his calculations about problems without writing the equation of the curve explicitly. (Bos 1984, p. 323)

Moreover, once the equation is found we must always construct the roots geometrically, that is, the equation is never the last step of the solution.

Bos's position has been challenged by Giusti, who claims that for Descartes 'the curve is the equation' and speaks in this connection about a 'revolutionary position'. Giusti emphasizes the algebraic component of the *Géométrie* which allows Descartes to give general and uniform solutions to a variety of problems central to his programme. Giusti grants the presence in the *Géométrie* of more 'constructive' strands, but claims that the identification of the curve with its algebraic equation is at the core of the Cartesian programme. He then claims that the 'constructive' elements (e.g. the appeal to machines for generating curves, or the geometric construction of roots) play more a rhetorical than a scientific role in the economy of the *Géométrie*:

By contrast, our thesis is that from the mathematical point of view these [constructive] representations have a secondary role with respect to the algebraic equation. Thus one needs to justify their presence in Descartes's work, and their role in the economy of the *Géométrie*. In our opinion, their role is more rhetorical than scientific. (Giusti 1987, p. 429)

The *Géométrie* is a striking work in which old geometrical paradigms and new algebraic strands intermingle at the same time. Determining the exact balance between the two will prove to be one of the long-standing interpretative issues in the debate on Descartes's contribution to mathematics.

Finitism

Generations of scholars (see for example Vuillemin 1960; Belaval 1960; Costabel 1985) have remarked on Descartes's finitism. We have seen some explicit examples of Descartes's finitism. His rejection of the mechanical curves is grounded in the idea that their construction involves us in infinite processes of approximation which cannot be exact (geometrical). His method of tangents also exemplifies his careful avoidance of infinitesimal arguments. However, one should not make the mistake of believing that Descartes simply does not have the techniques to engage in 'infinitistic' mathematics. His letters show how well versed he was in infinitesimalist techniques, as his solutions to problems involving the cycloid and Debeaune's problem abundantly show (Milhaud 1921; Vuillemin 1960; Belaval 1960; Scriba 1960–1; Costabel 1985). What is difficult to evaluate is how the limitation to finitary mathematics in the *Géométrie* fits into the Cartesian project. Some interpretations seem to imply that infinitistic mathematics will never be granted 'droit de cité' because they involve procedures 'que sa [Descartes's] méthode récuse' (Vuillemin 1960, p. 9). Recently, Costabel suggested that the elaboration of an incontestable finitary mathematics is only a first step towards the more complex goal of developing 'infinitary' mathematics. Descartes's restriction to finitary mathematics in the *Géométrie* is only a sign that he did not want to engage prematurely in infinitary mathematics (Costabel 1985, p. 38).

Belaval's interpretation sees in Descartes's refusal to admit infinitary mathematics in the *Géométrie* the clearest sign of how 'l'esprit de la méthode cartésienne [. . .] s'oppose à celui de la méthode leibnizienne' (Belaval 1960, p. 301). I shall come back to the problem of Cartesian finitism and Belaval's interpretation in Section 6.3, on revolutions.

Direct proofs and proofs by contradiction

The *Géométrie* is a work of its time. For example, the 'constructive' representations of curves contained in it are in direct line of succession of a long tradition of treatises on the construction of curves by the use of strings and other mechanical means (Ulivi 1990). I have emphasized how the discussion of pointwise constructions is motivated by Clavius's pointwise construction of the quadratrix. I wish to remark here on another feature of the *Géométrie* which joins it, and the analytic method in general, to two other major works published in the fourth decade of the seventeenth century. I am referring to Cavalieri's *Geometria*, published in 1635, and to Guldin's *Centrobaricae*, the last book of which appeared in 1641. In a previous paper of mine (Mancosu 1991) I have shown that both Cavalieri and Guldin aimed at a development of geometry by means of direct proofs, and that they explicitly avoided proofs by contradiction whenever possible. For Cavalieri direct proofs were the welcome outcome of his use of indivisibles, and for Guldin an ostensive development of geometrical theorems was based on his fundamental theorem on centres of gravity (what we now call the theorem of Guldin and Pappus). The emphasis on direct proofs was not just for purely mathematical reasons, but was connected with, and ultimately relied on, more global epistemological positions emerging from a Renaissance debate on the nature of mathematical demonstrations which goes under the name of 'Quaestio de certitudine mathematicarum' and which had important ramifications in the seventeenth century. I shall summarize the main points of the debate, and then proceed to show how deeply embedded is Descartes in these epistemological developments.

Logicians, following Aristotle, had traditionally distinguished two types of demonstration: demonstration of the 'fact' and of the 'reasoned fact'. The two types of demonstration were often identified with the resolutive and compositive method of the mathematicians (i.e. analysis and synthesis). Of the two types of proof, the latter was considered to be superior because proceeds from causes to effects (*a priori*), whereas in the former one starts from the effects to reach the causes (*a posteriori*).

The Quaestio de certitudine was centred on the issue of whether in mathematics one could attain such causal demonstrations. Opinions differed, but all (or almost all) the participants in the Quaestio agreed on singling out proofs by contradiction as being non-causal, and thus inferior to causal (*a priori*) proofs because that they do not explain their conclusions. My claim is that Descartes is heavily influenced by these developments. Lachterman (1989, pp. 158–9) has observed that Descartes reverses the traditional distinction mentioned above which connects analytic proofs with *a posteriori* proofs (from effects to causes) and synthetic proofs with *a priori* proofs (from causes to effects). Descartes claims that analytic methods, by showing how a result is obtained, also show why the result holds, and therefore analysis deserves to be considered as the paradigmatic form of *a priori* proof.[19] Moreover, Descartes claims that the superiority of the analytic method comes from the fact that the proofs obtained by applying it are causal, ostensive, and, therefore, superior to proofs by contradiction. The most explicit statements by Descartes in this connection are to be found in the letters exchanged between Descartes and Mersenne on the subject of Fermat's method of tangents.

Descartes defended the superiority of his own method against the claims made by Fermat (backed by Roberval) as to the superiority of Fermat's method. One of the arguments used by Descartes draws a sharp contrast between proofs by contradiction and *a priori* proofs:

For, in the first place, his method is such that without intelligence and by chance, one can easily fall upon the path that one has to follow in order to find it, which is nothing else than a false position, based on the way of demonstrating which reduces to absurdity, and which is the least esteemed and the least ingenious of all those of which use is made in mathematics. By contrast, mine originates from a knowledge of the nature of equations which, to my knowledge, has never been explained as thoroughly as in the third book of my Geometry. So that it could not have been invented by a person who ignored the depths of algebra. Moreover, my method follows the noblest way of demonstrating that can exist, i.e. the one that is called *a priori* (AT, Vol. I, pp. 489–90)

And again, against Roberval (July 1638), on the issue of proofs by contradiction:

. . . and I do not find anything reasonable in what he says, as when he claims the way of concluding *ad absurdum* to be more subtle than the other. It is absurd and this way has been used by Apollonius and Archimedes only when they could not find a better way. (AT, Vol. II, p. 274)

The appeal to *a priori* proofs against proofs by contradiction places Descartes's project for an ostensive development of mathematics in the same category as those of Cavalieri and Guldin. Of course, the methods on which Cavalieri, Descartes, and Guldin relied to carry through the project were quite different. However, they agreed on the 'metamathematical' preference for direct proofs over proofs by contradiction. Moreover, as the above quotations and references show, their position is deeply embedded in the epistemological issues which characterized the Renaissance and early seventeenth-century debates on the nature of proofs.

6.3. DESCARTES'S *GÉOMÉTRIE*: A REVOLUTIONARY EVENT IN THE HISTORY OF MATHEMATICS? PRE-KUHNIAN AND POST-KUHNIAN DEBATES

6.3.1. Pre-Kuhnian debates

Although I am not aware of any use of the word 'revolution' in connection with Descartes's *Géométrie* in the seventeenth century, it might be worth remarking that the use of the political metaphor of 'revolt' might have been used for the first time in connection with mathematics in 1696 in the preface (written by

Fontenelle, who was l'Hôpital's secretary) to the *Analyse des infiniment petits pour l'intelligence des lignes courbes* by l'Hôpital:

Such was the state of Mathematics, and especially of Philosophy, until M. Descartes. This great man, moved by his genius and by the superiority he felt inside, abandoned the ancients to follow only this very reason that the ancients had followed. And this happy boldness, which was treated as a revolt [*qui fut traitée de révolte*], gave us an infinity of new and useful views in Physics and in Geometry. (l'Hôpital 1696, ii–iii)

Of course, l'Hôpital (or better, Fontenelle) does not say that Descartes rebelled against the ancients, but this is of little interest. What is of interest is the occurrence of the metaphor of 'revolt'.

By the middle of the eighteenth century the political metaphor of revolution became quite common for characterizing Descartes's achievements in mathematics. A few quotations from the eighteenth and nineteenth centuries should suffice to convince the reader. In 1757 E. Montucla in his *Histoire des mathématiques* wrote:

One could not give a better idea of what Descartes's epoch in modern geometry has been than comparing it to that of Plato in ancient geometry. The latter by inventing Analysis gave a new face to this science. The former by the connection he established between it and algebraic analysis, has also brought about in it a happy revolution [*heureuse révolution*]. (Montucla 1957, p. 83; 1799–1802, Vol. 2, p. 112)

A widespread consensus about the revolutionary achievements of Descartes's *Géométrie* characterizes the historiography of mathematics in the nineteenth century. For example, A. Comte in his *Cours de philosophie positive* (1835) places Descartes at the origin of a general revolution in the mathematical sciences:

It is indeed remarkable that men like Pascal payed so little attention to Descartes's fundamental conception without having any foreboding of the general revolution [*révolution général*] that it was necessarily destined to bring about in the whole system of mathematical science. This has happened because without the aid of transcendental analysis this admirable method could not yet lead to essential results which could not have been obtained as well by the geometric method of the ancients. (Comte 1830–42, Vol. I, Lect. VI, note, p. 176)

Thus in Comte's opinion the *Géométrie* was 'necessarily destined' to bring about a revolution in mathematics. (This raises a host of issues, to which I will come back very soon.) Chasles, who once dubbed the *Géométrie* as *proles sine matre creata*,[20] emphasizes the novelty of Descartes's achievements:

But the geometry of this illustrious innovator made, as in all other parts of mathematics, a complete revolution [*révolution complète*] in the theory of these [conic] curves. (Chasles 1837, p. 91)

The above quotations comprise a representative sample of the eighteenth- and nineteenth-century 'consensus' concerning Descartes's revolutionary role in the history of mathematics. One should remark that the term 'revolution' refers, in the above quotations, to very different aspects of Descartes's activity. Indeed, Montucla puts emphasis on the unification of algebra and geometry, whereas Chasles and Comte remark respectively on the break with the past and on the revolutionary developments brought about by Descartes's work.

This historiographical consensus was strongly challenged in our century by the famous Cartesian scholar G. Milhaud in his book *Descartes savant* (1921), which remains to this day one of the best sources for the study of Descartes's scientific activity. One of Milhaud's main goals in this book was to show how dependent was Descartes's scientific activity on previous traditions, and thereby to undermine the idea that Descartes's work brings about a 'revolutionary' new start. The following passage taken from the conclusion of his book is paradigmatic of Milhaud's position:

> When one reads Descartes and one follows in particular the development of his scientific thought, one would say that his work comes out of his brain as wholly made, that he owes nothing to the ancients or the moderns, and that he has accomplished an unprecedented revolution in the human science. One could believe, at least at first sight, that he has realized his programme by reconstructing on the ruins of all that had already been done a completely new science which bears to the highest degree the mark of his strong personality. In the first place this is what we would like to show in some detail. Then, by bringing together this sort of spontaneous generation to the great current that goes from the Greeks to Descartes we will notice how precisely on the contrary it fits in and how little, deep down, Descartes is revolutionary despite all his originality. (Milhaud 1921, p. 228)

Let me rehearse briefly Milhaud's argument about Descartes's mathematics. Claims on behalf of Descartes's revolution rest mainly on the central idea of analytic geometry. Milhaud's first move is to question whether this claim can be rightfully held:

> Of this group of ideas, as well as of Descartes's whole mathematical work, what posterity has maintained as being above all his own creation is the very idea of analytic geometry, through which he has renewed mathematics and determined all its subsequent progress. However, although Cartesian analysis has indeed given invaluable services, does not the word creation, which is too easily applied to it, call for some reservations? (Milhaud 1921, p. 132)

Milhaud begins by describing the main results contained in Fermat's work *Isagoge ad locos planos et solidos* and remarks that the Cartesian representation of curves by equations is also in Fermat, and that Fermat arrived independently (around the same time as did Descartes) to what we may 'consider as the essence of Cartesian geometry'.[21] Of course, simultaneous discoveries are a common occurrence in the history of science. However, the case of analytic geometry, unlike that of the calculus, is remarkable for the complete absence of a priority debate:

Neither Descartes, nor Fermat, nor Roberval, nor Mersenne, nor Pascal, nor any one of those who would have had as a matter of course a judgement to express remarks by a single word the important fact that Descartes's analytic geometry was already clearly defined in its principles and its applications in some writings of Fermat which predate the Geometry. (Milhaud 1921, p. 139)

The solution to this puzzle rests, for Milhaud, on the fact that Descartes's and Fermat's work were simply the natural continuation and development of the method of geometrical loci of the Greeks so that it never crossed the minds of the seventeenth-century mathematicians that there might be a priority issue. Thus any talk of revolution in connection with Descartes's analytic geometry is illusory:

The *Revolution* that Comte and the XIXth century historians have seen in Descartes's *analytic geometry* conceals therefore an illusion. It is neither a question of revolution nor a question of a creation which radically transformed mathematics and renewed science. It is only a matter of normal development, after a return to the Greeks, of the main ideas of their analysis. (Milhaud 1921, p. 141)

Milhaud reached the same conclusions about the algebraic work contained in book III of the *Géométrie*.

I have quoted at length from Milhaud's book because mention of his work is conspicuously absent from contemporary debates on revolutions. Although Milhaud was aiming at Comte, his arguments seem to be more successful against Montucla and Chasles. Indeed, Comte could hold that Descartes's work is in direct line of succession from that of the Greeks, and that it was 'necessarily destined' to bring about a 'general revolution'.

Y. Belaval in his *Leibniz critique de Descartes* (1960) argued, against Comte, that this second assertion cannot be maintained:

It is to transform the fact into a right. In fact, the *results* of the Cartesian method, once expressed in the language of the infinitesimal calculus prepare, and seem to prepare necessarily, the *results* of the Leibnizian method (or the Newtonian one) . . . Let's go back from the fact to the right, that is to the spirit of the Cartesian method: this spirit opposes that of the Leibnizian method and is far from being 'necessarily' destined to produce it. And how does it oppose it? By the refusal, which makes Descartes an ancient, to introduce the consideration of the infinite in mathematics. (Belaval 1960, pp. 300–1)

I think that Belaval's point is well taken. As I have emphasized in the section on finitism, Descartes rejects infinitary mathematics in his geometry. We should also remember that the strongest opposition to the infinitesimal calculus came indeed from Cartesian mathematicians.[22] The calculus was generated by the convergence of different strands of thought, analytical geometry and infinitesimalist traditions, and took off only through the radical subversion of the 'epistemological signature' of the *Géométrie*.

6.3.2. Post-Kuhnian debates

Of the several post-Kuhnian contributions to the issue of Descartes and revolutions in mathematics, I shall discuss here in detail only the claim on behalf of Descartes's revolutionary achievements put forward by Cohen (1985). In particular, I shall not consider the more general problem of whether the shift from ancient modes of geometrical reasoning to more algebraic ones constituted a revolution (Mahoney 1980; Hawkins, quoted in Cohen 1985, pp. 505–7; or the thesis recently defended by Lachterman 1989) that the passage from ancient to modern mathematics is marked by a strong epistemological discontinuity having to do with the different roles of construction in the two periods.

There have also been post-Kuhnian claims aimed at demonstrating the non-revolutionary nature of Descartes's achievements. Usually, as in Boyer (1968), they are simply modified versions of Milhaud's argument for the continuity between Greek mathematics and seventeenth-century mathematics:

> The philosophy and science of Descartes were almost revolutionary in their break with the past; his mathematics, by contrast, was linked with earlier traditions. To some extent this may have resulted from the commonly accepted humanistic heritage—a belief that there had been a Golden Age in the past, a 'reign of Saturn', the great ideas of which remained to be rediscovered. Probably in large measure it was the natural result of the fact that the growth of mathematics is more cumulatively progressive than is the development of other branches of learning. Mathematics grows by accretions, with very little need to slough off irrelevancies, whereas science grows largely through substitutions when better replacements are found. It should come as no surprise, therefore, to see that Descartes's chief contribution to mathematics, the foundation of analytic geometry, was motivated by an attempt to return to the past. (Boyer 1968, p. 369)

Against this position Hawkins has claimed that a revolution takes place in mathematics when 'the methods of solving mathematical problems are radically changed on a large scale'. Cohen summarized Hawkins' position thus:

> In this sense, a revolution occurred in mathematics in the seventeenth century—the principal figures in this revolution were François Viète, René Descartes, Pierre de Fermat, Isaac Newton, and G. W. Leibniz. Of course, as Hawkins points out, their collective endeavor 'did not involve a "rejection" of ancient mathematics in the sense that, for example, Euclid's *Elements* were declared "false"'. But their work 'did involve a rejection of the methods by which the ancients solved problems' and introduced 'new methods', which were devised on the basis 'of the premise that mathematical problems should be reduced to the symbolic form of "equations" and the equations used to effect the resolution' . . . For Hawkins, the 'central figure in initiating this revolution was René Descartes'. (Cohen 1985, pp. 505–6)

As can be gathered from the above quotations, post-Kuhnian discussions as to the nature of Descartes's achievements differ from the pre-Kuhnian ones in that they depend on more global discussions about whether revolutions take place in mathematics (and more generally in science) and, if so, which sense. One of the scholars who has thought more about these issues is Cohen. In his *Revolution in science* he makes a definite claim on behalf of Descartes's revolutionary role. In order to understand the exact nature of Cohen's argument, it is essential to summarize the strategy of his work. Cohen does not give a definition of revolution in science (or mathematics), but suggests four interesting criteria for deciding whether or not a revolution has occurred:

(1) the testimony of contemporary witnesses (including scientists' assessment of their own work);

(2) the critical examination of the documentary history of the subject in which the revolution is said to have occurred;

(3) the judgement of competent historians, notably historians of science and historians of philosophy;

(4) the general opinion of working scientists today.

Although I have some reservations about the value of test (4), I find the other three tests very sound.

Cohen has no doubt that there has been a 'Cartesian revolution' in mathematics. In order to assess Cohen's application of the four tests to Descartes's *Géométrie*, I shall quote from his work. The first test is dealt with in the following passage:

Descartes claimed to have revolutionized *all* science and mathematics and even the methodological or philosophical underpinnings of science. His claim is of course not a sufficient ground for believing in a Cartesian revolution, but it is buttressed by the judgments of many seventeenth-century writers. Joseph Glanvill, for example, in his comparison of ancient and modern learning, not only expressed his appreciation of Descartes's formidable achievements in mathematics and in physical sciences, but printed Descartes's name in a very large bold-faced type that bespoke his greatness. (Cohen 1985, p. 157)

Let us now consider the second test:

Many accounts of Descartes's work in mathematics limit his contributions to coordinate geometry and the solution of 'geometric' problems by means of algebra. But perhaps his major innovation was not on any such simple level of technique but rather in his mode of thinking in general analytic terms . . . For instance, squaring a quantity traditionally meant erecting a square with a side equal to or represented by that quantity: the 'square' would be the area. Similarly for cubing. But once index notation (x^2 for xx or x-quadratum; x^3 for xxx or x-cubus) was introduced—and Descartes was the pioneer in this new mode of representing powers—then the breakthrough was Descartes's conception of such powers or exponents as abstract entities. This enabled mathematicians to write x^n, where n could have values other than 2 or 3, and in fact could even have fractional values. Descartes's freeing of algebra from geometric constraints constituted a revolutionizing transformation of mathematics and produced the 'general algebra' that made possible the claim (in 1628) of having achieved 'all that was humanly possible' in geometry and arithmetic. Newton's earliest ideas concerning the calculus were formed during a close study of the mathematical writings of Descartes and of certain commentators on Descartes's *Geometry* . . . The revolutionary quality of Descartes's mathematics is seen not only by comparing mathematics before and after Descartes, but by noting that seventeenth-century mathematics (and that of the succeeding centuries) bears firmly the Cartesian imprint. Hence Cartesian mathematics passes the historical tests for a revolution (Cohen 1985, pp. 156–7)

And the third test:

Additionally, historians and philosophers have declared for a revolution associated with Descartes ever since the middle of the eighteenth century, when it became common usage to apply the concept of revolution to the development of science. This is the third test. Cartesian science also passes the fourth and final test, the opinion of active scientists. (Cohen 1985, p. 158)

I do not dispute the historical evidence mentioned by Cohen in support of his claim. However, I think that the four tests proposed by Cohen do not allow us to give a clear-cut answer to the problem of whether Descartes made a revolution. My strategy will be simply to remark that for each one of the tests we have strong non-revolutionary evidence.

Consider the first test. First of all, it is essential to remark that Descartes himself seems to have been quite ambiguous about his position *vis à vis* the ancients and his contemporaries. At times he emphasized the novelty of what he had achieved. However, at other times he implied that there was no loss of continuity with previous mathematics, as in the following letter to Mersenne (31 March 1638) where, about his solution to Pappus's problem, he remarks:

However, this does not make it [the solution] at all different from those of the ancients, except for the fact that in this way I can often fit in one line that of which they filled several pages. (AT, Vol. II, p. 83)

The *Géométrie* is not an easy book to read. Few of Descartes's contemporaries were able to completely master it. However, none of the mathematicians who could have given a sound opinion of it (Fermat, Roberval, Pascal, Wallis, Barrow, and so on) speak of Descartes as the mathematician who had revolutionized geometry. For example, Barrow mentions the analytic method of Viète and Descartes only as one of the many novel things in seventeenth-century mathematics, but significantly he praises most of all Cavalieri's method of indivisibles as 'the most fruitful mother of new inventions in geometry'. Well known also is Leibniz's negative opinion of Descartes's achievements;

Those who are well versed in Analysis and Geometry know that Descartes has found nothing of consequence in Algebra, the speciosa itself being the work of Viète; the solution of cubic and quartic equations being the work of Scipio Ferro and Louis of Ferrara; the genesis of equations through a multiplicity of equations set equal to zero being the work of Harriot the Englishman; and the method of tangents, or of maxima and minima, being the work of M. Fermat. So all is left for him is to have applied the equations to the lines of geometry of higher degree which Viète, biased by the ancients which did not consider them geometrical, had neglected. (Quoted in Brunschvicg 1912, p. 114)

In short, contemporary mathematicians do not seem to give the praise that a revolutionary work would deserve. Thus Cohen's first test does not give a clear indication of the revolutionary nature of Descartes's work. Of course, we have already seen the explanation given by Comte for why there was no appreciation of the potential of Descartes's achievements before the calculus came into the picture. However, we have seen that Milhaud and Belaval argued, convincingly in my opinion, that this line of argument is untenable.

Let us look at the second test. Although it is certainly true that the techniques of the *Géométrie* were mastered by a large group of first-rate mathematicians in the seventeenth century, among whom were Newton (Galuzzi 1990), Leibniz (Belaval 1960), and the Bernoullis (Roero 1990), this holds as well for the indivisibilist and the infinitesimalist techniques. And I feel that in arguing in this way we end up with too many revolutions. We should not forget, as I have already remarked, that the calculus could take off only by subverting some of the critical tenets of Cartesian geometry. Moreover, it is not at all clear how much the spread of the analytical techniques is due also to Viète and Fermat. There are very few works on the spread of Descartes's *Géométrie*, the most notable exceptions being Costabel (1988, 1990) and Pepe (1982, 1988, 1990). Pepe, in particular reached the conclusion that the spread of analytic geometry in Italy in the seventeenth century was due more to the works of Fermat than to Descartes's works:

Analytic geometry in the seventeenth century is not only in Descartes: in particular, one must note that in Italy in this period the spread of Fermat's writings was easier than the spread of the *Géométrie*. (Pepe 1982, p. 282)

It would be interesting to have more work done in this area. As for Cohen's claim that Descartes freed algebra from geometric constraints, we have seen that the issue of the relationship between algebra and geometry in the *Géométrie* is one of the main interpretative issues surrounding the work. Moreover, it is misleading to describe Descartes's algebra of segments in such a way, since the new interpretation of the arithmetical (or algebraic) operations is no less geometrical than the previous one: it simply bypasses the issue of dimensionality. This was certainly a great move, but by itself it can hardly support the claim on behalf of Descartes's revolution. Even on metamathematical issues we have seen how dependent is Descartes on the ancients. His plan for classifying problems according to complexity is a refinement, a brilliant one, of the ancient classification by Pappus. Moreover, his rejection of mechanical curves and of 'infinitary mathematics' in his *Géométrie* is more in line with ancient mathematics than with the modern mathematics based on the analysis of the infinite.

Finally, concerning the third and fourth tests, I have shown that the claims on behalf of a Cartesian revolution have been vigorously challenged by claims to the contrary, by Milhaud, Belaval, Boyer, and all those who deny that revolutions take place in mathematics.

Thus I conclude that Cohen's tests do not provide us with an unequivocal answer to the problem of the nature of Descartes' achievements.

Although I have tried to strike a sceptical note on the claim that Descartes's *Géométrie* is a revolutionary event in the history of mathematics, I do not intend to play down the importance of the achievements it represents, or its role in shaping the algebraic techniques which were so masterfully exploited by the developers of the calculus. My aim has been simply to give a sense to the reader of how fraught with difficulties is the question of the revolutionary role of Descartes's geometry.

NOTES

1. Quotations from the *Géométrie* are from the Smith and Latham edition (Descartes 1952). I use the following abbreviations: (SL 54) indicates Descartes (1952, p. 54), and (AT 30) stands for the Adam and Tannery edition, Descartes (1897–1910, p. 30). I have sometimes modified Smith and Latham's translation; all other translations are mine. There are several introductions to the *Géométrie*: see, for example, besides the texts in history of mathematics mentioned in the bibliography, Bos (1981), Giusti (1987), Grosholz (1991), Itard (1956), Lachterman (1989), Milhaud (1921), Scott (1952), and Vuillemin (1960).
2 On the relationship between Viète and Descartes, see, for example, Giusti (1987) and Tamborini (1987).
3. The problem can be stated in an inessential variant by introducing a factor of proportionality:

$$CB \cdot CF = \lambda \cdot CD \cdot CH.$$

In the solution I assume that the lines are positioned exactly as shown in Fig. 6.3, so as to avoid needless complicatons with signs.
4. Pappus says: 'If there be more than six lines, it is no longer permissible to say "if the ratio be given between some figure contained by four of them to some figure contained by the remainder", since no figure can be contained in more than three dimensions. It is true that some recent writers have agreed among themselves to use such expressions, but they have no clear meaning when they multiply the rectangle contained by these straight lines with the square on that or the rectangle contained by those' (Thomas 1957, pp. 601–3).

5. The descriptions of these curves are easily found in any good history of mathematics: see for example Boyer (1968), Kline (1972), and, especially, Heath (1921). The reader should keep in mind that the spiral and the quadratrix are transcendental curves, whereas the conchoid and the cissoid are algebraic curves. See also Lebesgue (1950).
6. 'With this Sporus is rightly displeased for these reasons. The very thing for which the construction is thought to serve is actually assumed in the hypothesis. For how is it possible, with two points starting from B, to make one of them move along a straight line to A and the other along a circumference to D in an equal time, unless you first know the ratio of the straight line AB to the circumference BED? In fact this ratio must also be that of the speeds of motion. For, if you employ speeds not definitely adjusted (to this ratio), how can you make the motions end at the same moment, unless this should sometime happen by pure chance? Is not the thing thus shown to be absurd?

   'Again, the extremity of the curve which they employ for squaring the circle, I mean the point in which the curve cuts the straight line AD, is not found at all. For if, in the figure, the straight lines CB, BA are made to end their motion together, they will then coincide with AD itself and will not cut one another any more. In fact they cease to intersect before they coincide with AD, and yet it was the intersection of these lines which was supposed to give the extremity of the curve, where it met the straight line AD. Unless indeed anyone should assert that the curve is conceived to be produced further, in the same way as we suppose straight lines to be produced, as far as AD. But this does not follow from the assumptions made; the point G can only be found by first assuming (as known) the ratio of the circumference to the straight line' (Heath 1921, Vol. I, pp. 229–30).
7. This seems to be the rationale for Pappus's construction of the quadratrix by means of the spiral and the cylindrical helix. Molland (1976, p. 27) says, 'It seems clear that Pappus regarded the spiral and the cylindrical helix as having a firmer claim to the status of being geometrical than the quadratrix, which could however receive authentication by being derived from them. The constructions used in the derivation must also have been regarded as having a fairly geometrical status.' However, these derivations are not pointwise constructions, and the spiral and the cylindrical helix are, from Descartes's point of view, as problematic as the quadratrix. See Pappus (1933, Book IV) and Molland (1976, p. 27) for a description of Pappus's constructions.
8. This appendix is also reproduced with some variants in the *Geometria practica*, Book VII, pp. 189–94. The appendix is not in the first edition of the work (Clavius 1574), which is why I cite the third edition (Clavius 1591, p.. 349–59). In the third edition the first two diagrams are mislabelled, but Fig. 6.6 here is a correctly labelled version.
9. And again, after having described the standard construction of the quadratrix by two independent motions: 'Sed quia duo isti motus uniformes, quorum unus per circumferentiam DB, sit, & alter per lineas rectas DA, CB, effici non possunt, nisi proportio habeatur circularis lineae ad rectam, merito à Pappo descriptio haec reprehenditur: quippe cum ignota adhuc sit ea proportio, & quae per hanc lineam investiganda proponatur. Quare nos Geometrice eandem lineam Quadratricem