ON THE INTERPRETATION OF EXACTNESS

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Introduction: construction and representation

The subject of this talk arose out of a problem which I encountered in my research in early modern mathematics. The theme of that research is 'The Concept of Construction and the Representation of Curves' and the period is roughly from 1550 till 1750. During this period, the concept of 'construction' played an important and intriguing role in the development of mathematics. In classical Greek geometry construction was the standard procedure for solving problems. Geometrical propositions came in two kinds: theorems and problems; Pythagoras' theorem is a theorem; to draw a circle through three given points is a problem. Theorems were proved, problems were constructed. The formal construction of a problem consisted of a sequence of operations performed upon some given configuration and resulting in a new element of the figure with certain required properties. The operations were either postulated to be possible (as the constructions by 'ruler and compass,'[1] implied in Euclid's first three postulates) or explained in earlier problem-type propositions. The construction of a problem was concluded by a proof that the constructed figure did indeed possess the required properties. This classical conception of propositions and of problems in particular was still accepted as a matter of course in the early modern period; hence in a geometrical context problems could only be solved by a construction.

Two characteristic features of early modern mathematics gave an extra significance to the concept of construction. The first concerned the importance of problems. Early modern mathematicians, geometers in particular, saw their task primarily as problem solving and were less interested in proving theorems or investigating properties of geometrical constructs. As a result many mathematical activities were ultimately aimed at finding constructions. The second feature concerned curves. Many new curves were found and studied in the period. These curves had to be described in such a way that henceforth they could be considered known. I use the term 'representation of curves' for such descriptions of curves. At present we are used to representing curves by their algebraic or analytical equations. However, it was only during the eighteenth century

[1] Or rather 'by circles and straight lines', because Euclid did not refer to instruments in his postulates; in the sequel I shall use both expressions.

that the validity of this technique became self-evident. For some 100 years after Descartes and Fermat had introduced analytical geometry the equation of a curve was not considered to provide the essential or basic understanding of a curve. Mathematicians required more than merely the equation for the representation a curve; they preferred a construction of it, that is, a procedure by which (in principle) the curve could be drawn on paper — we will see examples of such constructions of curves below.

What makes the concept of construction an interesting subject of study is its importance in the early modern period, and particularly the fact that it provoked grave conceptual and methodological questions. Many mathematicians struggled with these questions and the various approaches which they adopted to answer or avoid them reveal much about the ways of mathematical thinking and the preferred directions of research at the time.

Basically there were two questions. The first was an old one; it arose in classical Greek geometry, but in the early modern period it gained a new urgency because mathematicians more and more often encountered problems that could not be constructed by ruler and compass. It was:

> *How to construct in geometry when ruler and compass are insufficient?*

The second question concerned the representation of curves. Many of the new problems that confronted mathematicians, especially those generated by the new mechanics of motion, required the determination of hitherto unknown curves. What could count as a solution to such a problem? Not, as explained above, an equation, but a construction. But here the Euclidean means of ruler and compass were entirely insufficient; they provided circles and straight lines only. The second question, then, was:

> *How to represent curves?*

These were serious questions, both conceptually and practically; without commonly accepted answers to them, problem solving was impossible. I want to stress this because the difficulties I encountered in studying the concepts of construction and representation may at first sight seem to be incompatible with the seriousness of the issue. Let me formulate these difficulties succinctly, and with some hint of my initial frustration about them, as follows: i) I found an abundance of ways of constructing and they often seemed very strange. ii) Early modern mathematicians adduced many arguments in support of these constructions but these were seldom, if ever, convincing. iii) At present, practically all these constructions are forgotten, and with good reason.

How, then, was I to write a historical account? How could I avoid ending up with a meaningless list of strange constructions and arguments of which you cannot even decide whether they are correct or not? The ideas about the

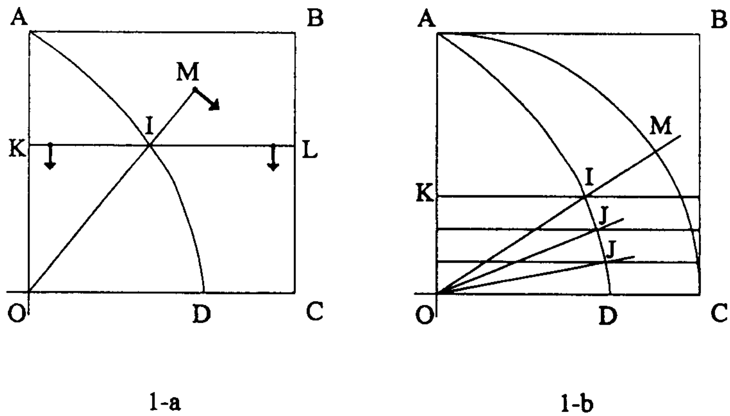

Figure 1.

interpretation of exactness which I want to present in this lecture helped me to pull myself out of this difficulty. I shall first present some examples of the constructions and of the arguments about their adequacy. Then I shall discuss the ideas about the interpretation of exactness and show how they can make more sense of the phenomena than mere description. I'll end the lecture with some more general remarks about the relevance of the ideas for other historical periods (including the present).

Constructions

I give three examples of constructions. They are: Christoph Clavius' 1589 construction of the curve called 'quadratrix;' René Descartes' construction of the roots of the general sixth-degree equation as published in 1637, and Jakob Bernoulli's 1694 construction of a curve called the 'paracentric isochrone'.

Clavius gave the construction in his 1589 edition of Euclid's *Elements*.[2] The quadratrix was introduced by classical Greek mathematicians in relation to the problems of dividing angles and squaring the circle (the latter connection gave the curve its name). It is (see Figure 1-a) the curve AD within the quadrant $OABC$ which is traced by the intersection I of a horizontal line KL and a radius OM when both these lines move uniformly, the radius turning from position OA to position OC and the horizontal line moving, parallel to itself, from position AB to position OC, the two motions being performed in the same time-span. It

[2]Euclid, *Elementorum libri XV accessit XVI de solidorum regularium cuiuslibet comparatione* (ed. C. Clavius), 2 vols, Rome, 1589.

follows from that generation that for any point I on the quadratrix the following proportionality applies:

$$\angle COI : \angle COA = OK : OA \,.$$

By means of the quadratrix it is easy, for instance, to trisect an angle — a problem which cannot be constructed by ruler and compass alone. The solution by the quadratrix is as follows:

> Let (see Figure 1-b) the given angle be $\angle MOC$ drawn with respect to a given quadratrix AD; I is the intersection of OM and the quadratrix. It is required to trisect $\angle MOC$.
> **Construction**
> **1.** Draw a line through I parallel to OC; it intersects OA in K.
> **2.** Divide OK into three equal parts (this can be done by ruler and compass); draw lines through the division points parallel to OC; they intersect the quadratrix in points J; draw radii OJ.
> **3.** These radii divide the angle into three equal parts.
> (The proof that the construction is correct is immediate from the definition of the quadratrix.)

Evidently, the quadratrix is a powerful means for solving problems, for the construction described above is easily generalized to dividing any angle into any number of equal parts or into two parts with any given ratio.[3] Clavius realized this but, taking over objections voiced already in antiquity, he doubted the geometrical acceptability of the construction of the curve by motion, because it was not clear how, without previous knowledge of the ratio between the radius of a circle and its circumference, one could adjust the two motions such that they would be completed in exactly the same time. Clavius therefore devised an alternative construction of the curve, a construction which did not depend on motion and was, he claimed, geometrically fully acceptable. The construction was as follows:[4]

> Given a square $OABC$ (see Figure 2); it is required to construct the quadratrix within the square.
> **Construction**
> **1.** Draw the quarter circle AC.
> **2.** Bisect OA and BC in D and E respectively; draw DE; bisect arc AC in F, draw OF; OF intersects DE in G; G is on the quadratrix.
> **3.** Bisect AD and BE in D' and E' respectively; draw $D'E'$; bisect arc AF in F'; draw OF'; OF' intersects $D'E'$ in G'; G' is on the

[3] The curve can also be used to find the quadrature of the circle. It can be proved that arcAC : OC = OC : OD. Thereby the circumference of a circle with radius OC can be constructed and from that its quadrature, that is, its area. This property gave the curve its name 'quadratrix.'

[4] Euclid, *Elementa* (cf. note 2), pp. 895-896.

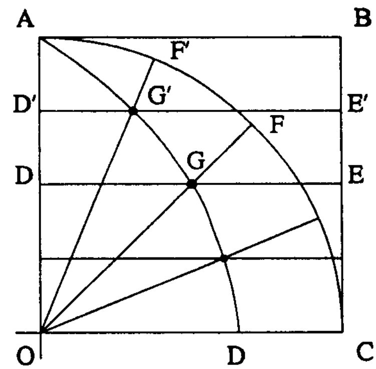

Figure 2.

quadratrix.
4. Repeat this procedure with other segments and corresponding arcs until sufficiently many points on the quadratrix are determined.
5. "... then the quadratrix line has to be drawn uniformly through these points, such that it does not oscillate but continues smoothly all along, making no hump or angle anywhere."[5]
6. To construct point D, the intersection of the quadratrix with the axis OC (which will not be among the points G constructed in (4)), construct points below OC symmetrical with points on the quadratrix near D; draw a smooth curve through these points on both sides; its intersection with OC gives D "without notable error, that is, one which can be perceived by the senses."[6]
(Proof: From the definition of the curve it is obvious that the constructed points are on the curve.)

The procedure is a 'pointwise' construction of a curve. This type of construction was very common in early modern mathematics. Basically it accepts a curve as constructed if a method is provided by which arbitrarily many points, lying arbitrarily near to each other on the curve, can be constructed by geometrically acceptable means (in Clavius' case: by ruler and compass). I shall return below to Clavius' arguments for accepting this procedure as legitimately geometrical.

My second example is Descartes' construction of the roots of a sixth-degree

[5]Euclid, *Elementa* (cf. note 2), p. 896; here and elsewhere, unless stated otherwise, the translations are mine.
[6]Euclid, *Elementa* (cf. note 2), p. 896.

equation as published in his *Géométrie* of 1637.[7] Descartes first showed that by a suitable transformation any sixth-degree equation could be written as

$$x^6 - px^5 + qx^4 - rx^3 + sx^2 - tx + v = 0 \; ,$$

with p, q, ..., v positive.[8] Descartes' construction of the roots of this equation required two curves to be drawn with respect to given perpendicular axes in the plane. The roots were then found as the ordinates of the points of intersection of the two curves. One of the curves was a circle, the other was a so-called 'Cartesian parabola'. Descartes explained how the latter curve could be drawn by a procedure involving the combined motions of a turning ruler and a moving parabola. Consider (see Figure 3) a parabola with vertex B and *latus rectum* a (which means that its equation is $ay = x^2$). The parabola moves vertically along its axis and carries with it the point P on the axis at a fixed distance b from B. There is also a ruler which connects a fixed point D and the moving point P. The distance of D from the axis is c. When the parabola moves, the ruler turns around D; its motion is determined by that of the parabola. The combined motions of the ruler and the parabola (see Figure 4) in their turn determine the motion of the points of intersection I of the ruler and the parabola; during their motion, these intersections trace a new curve $DEFGH$. This curve is the Cartesian parabola. It is a third-degree curve with two branches; the vertical is its asymptote.

The Cartesian parabola, then, is determined by the three parameters a, b and c. The other curve featuring in the construction, the circle, is determined by three more parameters, namely its radius r and the coordinates x_M and y_M of its centre M. The roots of any sixth-degree equation written in the above form can now be constructed by adjusting the parameters to the values of the coefficients. Descartes shows how this should be done:

Construction (see Figure 5)

1. Adjust the parameters of the Cartesian parabola as follows:

$$\begin{aligned} c &= \frac{p}{2} , \\ a &= \sqrt{\frac{t}{\sqrt{v}} + q - \frac{p^2}{4}} , \end{aligned}$$

[7] R. Descartes, *Géométrie*, pp. 402-411. Descartes' *Géométrie* constitutes one of the 'essais' in his *Discours de la methode pour bien conduire sa raison et chercher la verité dans les sciences; plus la dioptrique les meteores et la geometrie qui sont des essais de cete methode*, Leiden, 1637; it is on pp. 297-413. There are facsimile reprints of this work (Osnabrück, 1973; Lecce, 1987). The text of the *Géométrie* is in the *Oeuvres de Descartes* (Ch. Adam, P. Tannery, eds), Paris, 1897 - 1913 (Nouvelle présentation, Paris (Vrin) 1964-1974), vol. 6, pp. 367-485. There is a facsimile reprint with English translation: *The geometry of René Descartes* (tr. ed. D.E. Smith and M.L. Latham), New York, Dover, 1954.

[8] In this standard form the equation has only positive roots; the form can be achieved by a translation.

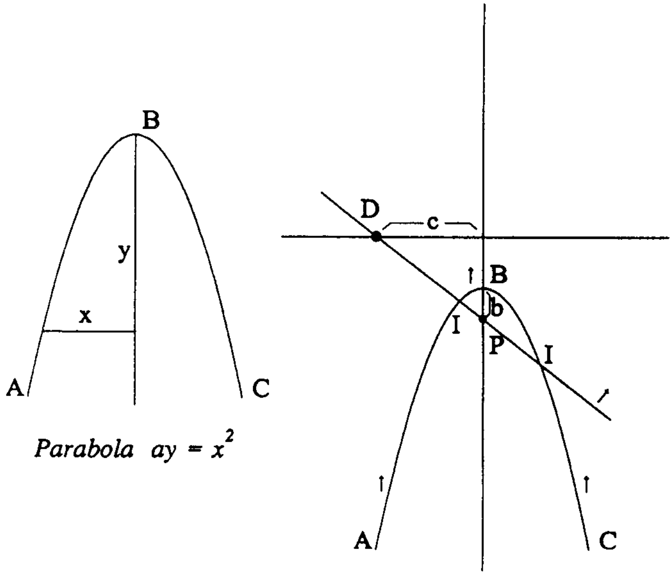

Figure 3.

$$b = \frac{2\sqrt{v}}{pa},$$

(note that these formulas involve square roots as the only irrationalities, so the lengths they represent can be constructed with ruler and compass); draw (by the procedure explained above) the Cartesian parabola with these parameters.

2. Adjust the parameters for the circle as follows:

$$\begin{aligned} x_M &= \frac{2\sqrt{v}}{pa} - \frac{t}{2n\sqrt{v}}, \\ y_M &= \frac{d}{a^2} \text{ with } d = \frac{r}{2} + \sqrt{v} + \frac{pt}{4\sqrt{v}}, \\ r^2 &= \frac{t}{2a\sqrt{v}} - \frac{s + p\sqrt{v}}{a^2} + \frac{d^2}{a^4}, \end{aligned}$$

(again, these lengths are constructible by ruler and compass); draw the circle with centre M and radius r.

3. The two curves determine (at most six) points of intersection

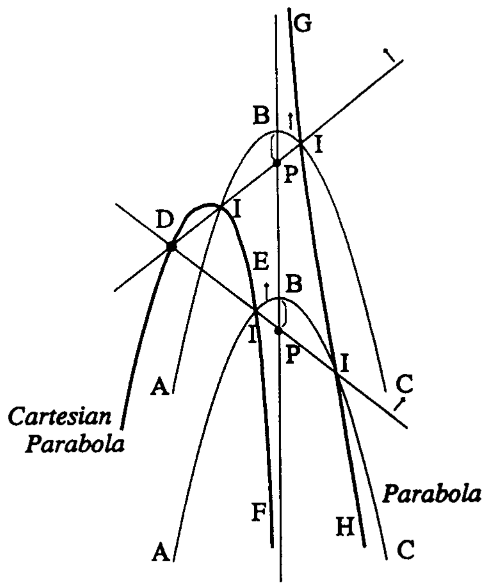

Figure 4.

U; draw their ordinates UW; they are the (positive) roots of the equation.
(Proof. Descartes proves by direct calculation that the constructed ordinates do indeed satisfy the given equation.)

I shall return to Descartes' arguments in favour of this construction, for the moment I merely remark that, in Descartes' opinion, this construction should be accepted as the canonical solution of a sixth-degree equation if it occurred in a geometrical context. He also believed that analogues of this construction could be found for any polynomial equation in one unknown.

My third example[9] of a construction is, like the first one, a pointwise construction of a curve. Its author is Jakob Bernoulli, who gave it in 1694 in response to a problem proposed by Leibniz. Leibniz had challenged mathematicians to

[9]For further information on this episode see my articles "The lemniscate of Bernoulli", *For Dirk Struik* (Cohen, R. S., e.a., eds., Dordrecht, Reidel, 1974), pp. 3-14 and "The concept of construction and the representation of curves in seventeenth-century mathematics", *Proceedings of the International Congress of Mathematicians, August 3-11, 1986 Berkeley, California, USA* (A. Gleason, ed., Providence, American Mathematical Society, 1987), pp. 1629-1641.

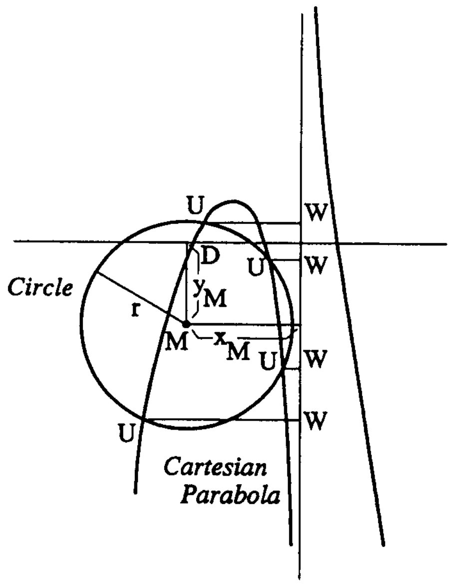

Figure 5.

determine the curve he called the "paracentric isochrone," characterized as follows: A point M (see Figure 6) is assumed to move along the curve OMN in a vertical plane, as if under the influence of gravity. If the form of the curve is such that during the motion of M along it, the radius $r = OM$ varies linearly with time, then OMM is a paracentric isochrone. Figure 6 gives the situation in which it is assumed that the body has a finite initial velocity when starting from the point O. Bernoulli gave the following construction of the curve.[10]

Construction

1. Take an elastic beam (!) (see Figure 7), fix its one end vertically at A and apply a sufficient force F to its other end to bend it such that its direction at O is horizontal; the beam now has the shape of a curve which Bernoulli called the *elastica*; use it to draw the elastica AO on paper.

2. Draw a circle around O with radius $OB = a$, where a is the horizontal distance of A and O.

3. Take an arbitrary point C on BO and draw CD vertically with

[10] Jakob Bernoulli, "Solutio problematis Leibnitiani de curva accessus et recessus aequabilis a puncto dato, mediante rectificatione curvae elasticae", *Acta Eruditorum*, 1694 (June), pp. 276-280; also in Jakob Bernoulli, *Opera*, Geneva, 1744 (reprint Brussels, 1967), pp. 601-607.

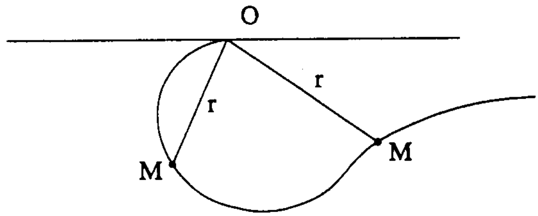

Figure 6.

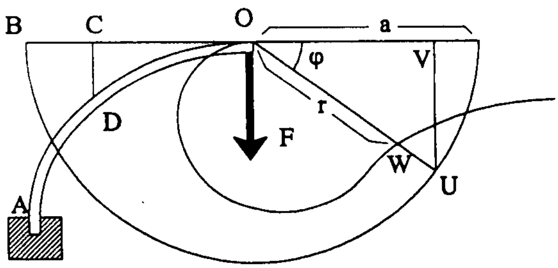

Figure 7.

D on the elastica.
4. Take U on the circle such that $UV = \frac{OC^2}{a}$.
5. Take W on OU such that $OW = \frac{(\text{arc}OD)^2}{a}$ (here it is assumed that the rectification of the elastica can be performed).
6. W is on the paracentric isochrone; repeat the construction for other points C on BO to get arbitrarily many points on it.

As before, I will return to Bernoulli's arguments in favour of this construction; now I remark only that the construction is indeed correct if one accepts that the elastic beam bends in the form which the theory of elastic bending predicts. In fact Bernoulli had just elaborated that theory and in this construction he applied one of the results of that study. The paracentric isochrone is a transcendental (i.e. a non-algebraic) curve. There are two steps in the construction that introduce the transcendental nature, the one is the introduction of the bent elastic beam, the second is the assumption that one can determine the arc length of a given curve.

The three examples may explain my initial perplexity when surveying the seventeenth-century constructions of which these three are extreme but not uncharacteristic examples. If I try to characterize them the first adjectives that come to my mind are methodologically useless: I find Clavius' construction cute; I find the algebraic part of Descartes' construction, the formulas for the parameters, decidedly ugly, but his process of tracing the Cartesian parabola I find intriguing. And Bernoulli's construction I find just weird. But such words — cute, ugly, intriguing, weird — help little. Let me then turn to the arguments of the authors of the constructions to see if these help us to understand the early modern practice of construction.

Arguments

Together with Clavius', Descartes' and Bernoulli's comments on their constructions I shall discuss the opinions on construction expressed by two other mathematicians, namely Viète and Kepler.

Clavius' arguments[11] in favour of his construction of the quadratrix primarily regarded practical precision. He wrote that his construction was very precise indeed. It was true that he only constructed points on the curve, but if pointwise constructions were to be rejected then the work of Apollonius on conics, many of Archimedes' results and the whole practice of making sundials should be rejected as well. Moreover, he wrote, the quadratrix construction was more precise than the usual pointwise constructions of conic sections, and it could be refined at will by constructing more points on the curve. Clavius admitted that the construction did not attain absolute precision, but that did not prevent him from claiming, in 1589, that his procedure was "truly geometrical", and that by means of the quadratrix thus constructed the quadrature of the circle was geometrically ("geometrice") solved. Later, probably in response to criticism, he retracted that statement somewhat; in re-publications of his treatise on the quadratrix in 1604 and 1611-12[12] he added a cautious "in a way" ("quodammodo") to the assertive "geometrice" of the earliest version.

In 1591 François Viète published a short, programmatic treatise, the *Introduction to the analytic art*,[13], in which he sketched a new research programme in mathematics. In the years that followed he carried out this programme and published some of the results in a series of separate treatises. In one of these, *A supplement to geometry*,[14] he treated geometrical constructions beyond ruler and compass. He based his treatment on the fact, already known to classical Greek geometers, that many problems which could not be solved by ruler and compass alone could be reduced to the solution of one special problem called 'neusis'. This neusis problem was as follows:

[11] Euclid, *Elementa* (cf. note 2), pp. 897-898.

[12] Chr. Clavius, *Geometria Practica*, Rome, 1604, p. 362; *Opera Mathematica*, 4 vols, Mainz, 1611 - 1612, vol 2, p. 191.

[13] François Viète, *In artem analyticen isagoge*, Tours, 1591; the text is also on pp 1-12 of Viète's *Opera mathematica (ed. F. van Schooten)*, Leiden, 1646 (facsimile reprint Hildesheim, 1970). An English translation is on pp 11-32 of François Viète, *The analytic art, nine studies in algebra, geometry and trigonometry* (tr. T.R. Witmer), Kent (Ohio), 1983.

[14] François Viète, *Supplementum geometriae*, Tours, 1593; in *Opera* (cf. note 13), pp. 240-257; translation in *Analytic art* (cf. note 13), pp. 388-417.

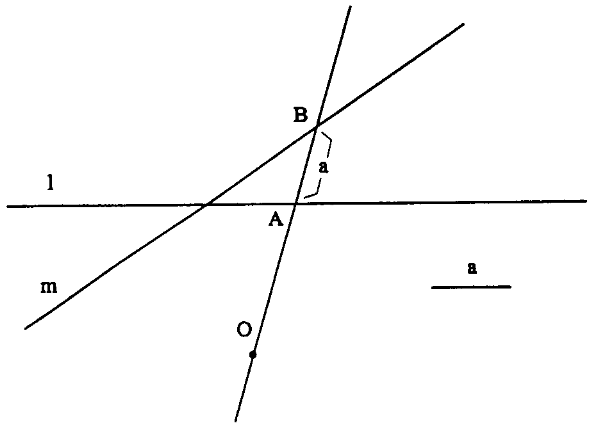

Figure 8.

Given: two straight lines l and m (see Figure 8), a point O and a segment a.
Required: a line through O, intersecting l and m in A and B respectively, such that $AB = a$.

Viète asserted that geometry should be supplemented[15] with a new postulate — I shall call it the 'neusis postulate' — which stated that the neusis problem could be constructed. With that supplement geometry would extend its power over a whole area of problems which it could not handle on the basis of the Euclidean postulates alone. In a sense, then, Viète's proposal was an alternative to Clavius' introduction of the quadratrix. Both legitimated the use of some means of construction beyond the Euclidean canon of ruler and compass. Unlike Clavius, however, Viète adduced no explicit arguments in support of this legitimation; he did not even comment on the question of how the neusis construction should be performed. On the other hand, he gave a beautiful result that showed clearly the power of the new postulate. He proved that any geometrical problem which, translated into algebra, led to an equation (in one unknown) of degree 1, 2, 3 or 4, was constructible once the Euclidean postulates were supplemented by the neusis postulate. In particular such problems as the trisection and the construction of two mean proportionals became solvable through this supplement of geometry.

In the case of Johann Kepler I need not explain a particular construction because he rejected all constructions beyond those by ruler and compass. He was very explicit about the reasons for this purist orthodox attitude. We find his arguments in his great study on the harmonies of the world, the *Harmonices mundi* from

[15] Hence the name of the book, cf. note 14.

1619.[16] He first of all recalled ancient authority: Proclus had explained that the circle and the straight line were the first and the simplest and the most perfect of lines; using others would lessen the perfection of geometrical construction. Moreover, Kepler showed — convincingly, I may add — that many constructions hitherto proposed beyond ruler and compass were inexact, unconvincing if not downright impossible. He concluded that beyond the domain defined by the use of straight lines and circles no truly scientific knowledge was possible in geometry. The other domain, including problems such as the trisection, was separated from true geometry and, as he wrote, "the bridge lies broken on the other shore".[17] The richness of his metaphors indicates how strongly Kepler felt about this matter and indeed he had deeper reasons for adopting such a restrictive vision of geometry. These reasons were philosophical.

The central concept of Kepler's philosophy was *harmony*. God had created the world according to harmonious principles. Only the harmonious was truly knowable. Geometry was one of the ways to attain such knowledge, applying in particular to the realm of magnitudes, where harmony resided in the ratios of magnitudes. Which ratios, then, were harmonious? According to Kepler these were precisely the ratios of magnitudes occurring in geometrical figures which were constructed within Euclid's geometry, that is, exclusively by straight lines and circles. The regular triangle, the square, the regular pentagon and hexagon were such figures, but not, for instance, the heptagon. In space geometry the principal figures providing harmonious ratios were the Platonic solids. These Platonic solids, treated in the thirteenth book of Euclid's *Elements*, formed as it were the pinnacle of the building of geometry; weakening the rules of Euclidean construction removed the cement and the roof of that building and then "the walls stand cracked, the vaults in danger of collapse."[18]

Contrary to Kepler, Descartes did not hesitate to introduce means of construction beyond the Euclidean straight lines and circles. In his *Géométrie* he even proposed that any curve could be used for construction, provided it was algebraic and no simpler curves could be found by which the same construction could be performed. This new opinion on construction enormously expanded the legitimate realm of geometry. Unlike Viète, Descartes did provide arguments for the legitimacy of his new conception of geometrical construction. Basically he claimed that curves were acceptable for use in geometrical constructions when they could be traced by motions or combinations of motions which could be clearly and distinctly imagined by the mind's intuition and were therefore pre-

[16] Johannes Kepler, *Harmonices mundi libri V*, Linz, 1619; I refer to the edition of the work in vol. 6 of Kepler's *Gesammelte Werke* (ed W. von Dyck, M. Caspar, F. Hammer), München, 1937 sqq; there is a German translation: *Weltharmonik* (tr. M. Caspar) (repr. of ed. 1939), München, Oldenbourg, 1973. The most important passages on construction are in book I (pp. 13-64 in the *Werke*).

[17] Kepler, *Harmonices mundi* (cf. note 16), pp 60-61.

[18] Kepler, *Harmonices mundi* (cf. note 16), p. 19.

cise and exact.[19] The tracing of the Cartesian parabola by the combination of a turning ruler and a moving parabola satisfied these criteria. Its use in the construction of the roots of a sixth-degree equation was therefore geometrically legitimate and thereby all problems that led to such equations were solvable in Descartes' new doctrine of geometry.

The paracentric isochrone was a non-algebraic curve — of the class of curves, therefore, which Descartes had rejected from geometry. When Descartes formulated this demarcation of geometry it constituted an extension of the geometrical domain, but by the end of he century it was felt to be an undesirable restriction. Bernoulli did not accept it; he saw the determination of the paracentric isochrone as a legitimate geometrical problem and his construction as a legitimate solution. Yet the solution was sufficiently unusual for him to comment in some detail on his own and other methods for constructing non-algebraic curves in a later article.[20] He distinguished four such methods. The first was 'by quadratures of algebraic curves', which meant that one assumed (as a postulate) that the areas under algebraic curves could be determined. Bernoulli had derived the following equation of the paracentric isochrone in coordinates r and u (see Figure 7):

$$\sqrt{ar} = \int_0^u \frac{a^2 du}{\sqrt{a^4 - u^4}},$$

where $r = WO$ and u is defined by $u^2 = a^2 \sin\varphi$. The right-hand side is an elliptic integral which cannot be integrated in finite terms. But on the basis of this analytical expression a pointwise construction 'by quadratures' was immediate: the integral represented the area under an algebraic curve; if that area were assumed to be constructible, arbitrarily many pairs (u, r), and thereby arbitrarily many points on the curve, could be constructed.

Bernoulli considered such constructions by quadratures acceptable but hardly satisfactory. In his opinion other kinds of construction were better. One was 'by rectification of algebraic curves'. That case occurred if the equation of the curve involved an integral which was interpretable as the arc length of an algebraic curve, so that the curve would be constructible if one assumed that arc lengths of algebraic curves could be determined. It was better to assume that arc lengths could be determined than to assume the same about areas under curves, because, he argued, arc lengths, being one–dimensional, were easier to determine (for instance by applying a cord along the curve) than areas (quadratures), which are two–dimensional. Actually, both Jakob and his brother Johann, independently, found a construction of the paracentric isochrone by rectification

[19] Cf. my article "The structure of Descartes' Géométrie", *Descartes: il metodo e i saggi; Atti del convegno per il 350o anniversario della publicazione del Discours de la Méthode e degli Essais* (ed. Giulia Belgioioso e.a., Florence, 1990), pp. 349-369, in particular pp. 362-363.

[20] Jakob Bernoulli, "Constructio curvae accessus et recessus aequabilis ope rectificationis curvae cuiusdam algebraicae", *Acta Eruditorum*, 1694 (Sept), pp. 336-338.; in his *Opera* (cf. note 10), pp. 608-612, see in particular pp. 608-609.

of an algebraic curve; it turned out that each had hit on the same curve to be rectified, namely the 'lemniscate'.[21]

Another method of constructing non-algebraic curves which Jakob Bernoulli preferred to the use of quadratures (and which he considered to have the same standing as those employing rectification) was a special type of pointwise construction which, for reasons of space, I will not explain in detail here; I only remark that Clavius' construction of the quadratrix was of that type.

Although preferable to constructions by quadratures, the latter two methods were, in Bernoulli's opinion, not ideal. Not surprisingly, the best method was exemplified by his own construction of the paracentric isochrone. He described it as a construction by means of curves 'given in nature.' The 'elastica' was such a curve given in nature (namely by bending an elastic beam). Another such curve, Bernoulli explained, was the catenary, the shape of a chain or rope suspended between two points. It could easily be provided 'by nature': one simply suspended a chain or rope between two points in front of a vertical piece of paper. Constructions which employed these curves which nature supplied free of charge were the best possible.

The interpretation of exactness

This brief overview may explain why, rather than taking away my bewilderment about the constructions, the arguments about their validity only increased my confusion. Especially the ambivalent attitude towards practical feasibility was difficult to interpret; most of the constructions could not actually be executed at all and were not meant to be, and yet practicality was an argument the defence of their geometrical legitimacy.

So how are we to make sense of this? Apparently the intellectual quality of the arguments is not the best guide for studying them historically. And the usual reason for interest in scientific arguments from the past is lacking too, namely their ancestor relation to ideas that we now cherish. Indeed, there is no danger here of a 'Whig' approach to history;[22] both the constructions themselves and the arguments surrounding them have long since disappeared from the mathematical scene.

Yet the issue was important for early modern mathematics. Several developments at the time were crucially influenced by ideas about legitimate geometrical construction; the interest in certain problems can only be understood from the contemporary ideas about construction, and the terminology of the mathematical texts can hardly be understood without an awareness of the issues surrounding construction. As neither the mathematical content nor the

[21] For further details see my "Lemniscate" (cf. note 9).

[22] The term 'Whig history of science' is used, usually in a critical sense, to denote a type of historiography which finds interest in past events only if they can be argued to have led directly to the valuable achievements of modern science. See e.g. Bynum, W.F. e.a., eds, *Dictionary of the history of science*, London, 1984, s.v. 'Whig history', pp. 445-446.

quality of the arguments seems to provide a workable approach to the issue, we should ask: what was the function of the debates on construction within the mathematical enterprise? What exactly were the mathematicians doing when they engaged in these debates? Well, they allowed and forbade, sanctioned and vetoed, tolerated and restricted; in short, they legislated (and disputed legislation) on two important methodological questions of early modern mathematics. These questions were:

> *When is a problem solved?*
> *When is an object known?*

These were crucial questions because without a generally accepted answer to them a large part of the practice of geometry, in particular problem solving, would be impossible.

At this point I should insert a remark about approximation. Clavius referred to the precision of pointwise construction, Bernoulli considered the arc length of a curve easier to determine than the area under a curve — were these mathematicians perhaps talking about the precision of approximative operations? And would not, in that case, the criterion have been obvious, namely closeness of approximation? No, the issue did *not* concern approximation. Early modern mathematicians interested in approximate solutions would choose methods other than the ones we have been discussing: trisecting an angle approximately is best done by trial and error, and for determining roots of equations arithmetical methods of approximation were available which yielded much more precise results than any geometrical method. The above constructions did not aim at the speediest or most precise result. Even when Clavius and Bernoulli referred to practical precision, their aim was not to enhance that precision but rather to abstract from it the criteria which should guide the geometer's choices in an idealized practice of pure geometry.

So seeking the answers to the questions on solving and knowing meant seeking the rules for correct procedure in pure, non-approximative and non-practical geometry. The question was: when do we, as geometers, proceed correctly, legitimately, exactly, in a proper mathematical way, in solving problems or declaring objects found and known. Now I need a term for what is referred to by words like 'correct,' 'legitimate,' 'exact', 'properly mathematical,' etc. Early modern geometers used several different terms in this context; from these I have chosen 'exact'.[23] Thus what the geometers were engaged in while discussing the two questions formulated above was the *interpretation of exactness*; their aim was to interpret what it meant to proceed 'exactly' in geometry, what it meant to have exact geometrical knowledge about solutions or objects. And obviously, a

[23] Cf. for instance Descartes: '... prenant comme on fait pour geometrique ce qui est precis et exact, et pour mechanique ce qui ne l'est pas ..." *Géométrie* (cf. note 7), p. 316.

clear interpretation of exactness was necessary — how else could one claim to do geometry?

So we may characterize the early modern period in mathematics as an episode in which there was much uncertainty and debate about the interpretation of exactness. It was not the first or the last such episode. Others were the emergence of mathematics as an axiomatic deductive science in ancient Greece and the 'foundations crisis' during the first decades of the 20th century. These, one may say, were successstories in the history of mathematics. The Greek mathematicians responded to stricter demands of mathematical exactness by creating a new deductive science, with axioms and proofs, and they did not hesitate, as in the case of the theory of ratios, to reformulate theories completely when they no longer met the standards of a new interpretation of exactness. The results of these endeavours are still strongly present in modern mathematics and they strike us as impressive mathematical creations. The rethinking of the foundations of mathematics in the early twentieth century can also be termed a successstory. No real answers were found to the questions which, because of their impact on the self-image of the mathematical community, justified the term 'crisis' with respect to the episode. Yet these questions gave rise to many deep and powerful new theories in mathematics and logic, and in general to an effective new understanding of the mathematical enterprise; and these successes remained while the original questions lost their sting and thereby much of their interest.

The story of construction and representation in early modern mathematics constituted another episode in the interpretation of exactness, but, contrary to the ones just mentioned, it was no successstory. Indeed it was an almost total failure. From the many arguments that were proposed one may collect a few deeper thoughts, but these did not attain a lasting place in mathematics and most of the activities of mathematicians relating to the issue were lost and forgotten when, in the eighteenth century, mathematical analysis eclipsed the earlier geometrical style and made the questions concerning construction and representation seem insignificant and meaningless — without in any way solving them.

Could they have been solved? Strictly speaking these questions about the legitimacy of constructions were unsolvable. The interpretation of exactness is extra– or meta–mathematical, in the sense that no answer can be derived from the pertaining formalized mathematical theory itself. From the axioms and postulates one cannot derive the reasons why these axioms and postulates are legitimate. Ultimately, legitimation has to be based on arguments outside mathematics. These outside arguments have no ultimate cogency, which means that the protagonists in the debate may agree to disagree or even disagree to disagree for ever.

Strategies — a classification

Thus the mathematicians whose constructions and arguments I have been discussing were confronted with unsolvable questions which nevertheless had to be answered. In responding to this situation they adopted certain attitudes, or strategies. I have found that these attitudes or strategies are a better starting point for an understanding of their activities than the constructions and arguments themselves. Indeed, whereas the choices of the constructions and the arguments fail to convince, the strategy that appears to be followed may well have its own, understandable logic. Thus the question that arises is whether the various approaches to the interpretation of exactness can be divided into categories which have an inner coherence in terms of attitude or strategy.

I have found that for the early modern discussions on construction and representation such categories can indeed be distinguished and that, at least for me, they clarify the matter. So I present these categories here, illustrating them by fitting the above constructions and arguments into them. After that I shall close with some remarks — and hope for discussion.

The categories I discern in the various early modern reactions to the interpretation of the exactness of constructions and representations in geometry are:

1. Appeal to authority and tradition
2. Idealization of practical methods
3. Philosophical analysis of the mathematical intuition
4. Regard for the quality of the resulting system
5. Revolt
6. Non-interest

For the first category, 'appeal to authority and tradition,' the example is Kepler. Kepler was sincerely worried, primarily for philosophical reasons, that the tradition which restricted genuine geometry to those configurations constructible by straight lines and circles, and which in his time was subject to considerable erosion, would be lost. So, in support of his purist orthodox conception of geometry he appealed to the authority of Euclid, and of Euclid's late classical interpreter Proclus. Another example of the recourse to a strategy of appeal to authority and tradition in the interpretation of constructional exactness concerns a passage of Pappus in which the geometer is warned against the "sin" of constructing by improper means. Many early modern geometers gratefully invoked Pappus' authority by quoting this passage when they defended their own ideas on construction or attacked those of others.[24]

[24] Thus, for instance, Fermat: "Certainly it is an offence against pure Geometry if one assumes too complicated curves of higher degrees for the solution of some problem, rather than taking the simpler and more proper ones; because, as Pappus, and recent mathematicians as well, have often declared, in geometry it is a considerable error to solve a problem by means that are not proper to it." Pierre de Fermat, "De solutione problematum geometricorum (-)

The second category, 'idealization of practical methods', covers the cases in which, implicitly or explicitly, mathematicians proceeded from the conviction that the criteria of exactness in a mathematical field should run parallel to the criteria of precision in the corresponding practice. Among the constructions and arguments discussed above, those of Clavius belong to this category. Clavius defended the geometrical status of his pointwise construction of the quadratrix by the argument that the procedure was very precise in practice. It should be remarked that by modelling criteria on those observed in practice Clavius did not make geometrical construction a practical pursuit. The adoption of these criteria still involved a step of idealization and therefore it did not turn geometry into a practical science. For Clavius the legitimation of the quadratrix as means of construction was a contribution to pure geometry.

Bernoulli's arguments for his hierarchy of methods for constructing non-algebraic curves also belong to this category. He claimed that the arc length of a curve was easier to measure than the area under it and that it was even more expeditious to use curves 'given in nature'. Here the step of idealization, by which the practical criterion is transported to pure geometry, is even larger than in Clavius' case; Bernoulli must have been aware that, in practice, working with curves 'given in nature' like the 'elastica' would not be very practical or precise. Yet the idealization of such a practice gave him the arguments he needed to decide on a hierarchy of methods for solving problems involving non-algebraic curves.

In the examples given above, the third category, 'philosophical analysis of the mathematical intuition', is represented by Descartes. Central to his philosophy was an analysis of how the mind attains certainty. This analysis, in which mathematics served as an important source of inspiration, led him to adopt clarity and distinctness as criteria for accepting insights as certain. When in geometry he took up the interpretation of exactness of constructions, he applied the same criteria and concluded that constructions were acceptable if they involved only curves whose tracing by motion could be conceived with clarity and distinctness. He gave various examples of tracing processes which he deemed clearly and distinctly conceivable; tracing the Cartesian parabola by a turning ruler and moving parabola was one of these. He also indicated procedures in which he did not find sufficient clarity and distinctness and he excluded from geometry the curves resulting from such procedures. (He further concluded that on the basis of this criterion the curves that should be accepted in geometry were precisely those that have algebraic equations; his arguments for that statement are too complicated to be detailed here.)

I propose Viète as an example of a mathematician whose approach to the interpretation of exactness falls in category four, 'regard for the quality of the resulting system'. Viète did not produce any explicit arguments in support

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dissertatio tripartita", in *Oeuvres* (P. Tannery, C. Henry eds) (4 vols), Paris 1891-1912, vol. 1 pp. 118-131, quotation on p. 121 (my translation).

of his adoption of the neusis postulate as a legitimate geometrical postulate. Yet his results provided an implicit argument. He showed that on the basis of this postulate a theory could be erected in which a large but definable class of problems became solvable without becoming trivial. This implicit argument was a forceful one because in mathematics large but accessible extent and non-triviality are strong marks of quality of a theory. On that account Viète's use of the neusis postulate was much more effective than, for instance, Clavius' use of the quadratrix. Both provided a considerable extension of the set of solvable problems, but in Clavius' case the solutions (the constructions) were trivial and therefore of no interest.

For the fifth category, 'revolt', I gave no examples; I am not even sure whether enough such examples could be found to justify a separate category. The reason why I formulated it is that I encountered one source whose author seemed to me just to be making fun of the whole business of problem solving by inventing very eccentric constructions indeed.[25]

The sixth and last category, 'non interest', may be the most important one in terms of the number of mathematicians who adopted attitudes that fit into it. Especially if combined with an occasional appeal to tradition or current established practice, such attitudes provide a very workable basis for mathematical activity, especially in fields and periods in which the interpretation of exactness is not in dispute.

So far the classification. It helped me to get over my initial perplexity and to order, understand and interpret the material on construction and representation in early modern mathematics. By identifying the categories I could use another logic in the study of these mathematical activities and arguments than the simple one of truth or conviction, namely the circumstantial logic of the choice for one rather than another strategy. Here other arguments come to play, personal, philosophical, professional and situational ones, which can provide acceptable explanations for the reactions of mathematicians faced with the dilemmas of interpreting exactness. I will not follow up these explanations here; what I wanted to report on was the classification itself and the way it structured phenomenon that was at first sight enigmatic.

Remarks and questions

I would like to close with some questions and remarks about the interpretation of exactness and the classification of possible approaches to the issue. One question is whether the categories are recognizable. They proved useful for my own research on early modern mathematics. But mathematical exactness was

[25] Nicolas Bion, *The construction and principal uses of mathematical instruments translated from the french of M. Bion (-) to which are added the construction and use of such instruments as are omitted by M. Bion (-) by Edmund Stone (-)*, London, 1758; the examples are by Stone, see especially pp. 319-325.

interpreted in other periods as well; does the classification also apply for these periods? Little is known explicitly about the motives that led classical Greek mathematicians to readjust the criteria of exactness in their science, so it may be over optimistic to expect much from an application of the classification in this case. But an exploration of its applicability for the period of the 'foundations crisis' might be a useful means for studying analogies between that episode and the interpretation of constructional exactness in the early modern period.

There is also another area in which the distinctions implicit in the classification may retain some meaning. In the nineteenth and early twentieth century the choice of axioms in mathematics was directly linked with philosophical and foundational concerns and therefore often gave rise to doubts and debate; it was seen as laying down the mathematical laws, interpreting exactness, and therefore it needed legitimation. In present-day mathematics the choice of axioms and postulates has become a standard procedure; the routine axiomatic introduction of new mathematical structures rarely raises questions of legitimation. Yet the motivation of these choices, as in the case of the interpretation of exactness, ultimately lies outside the realm of the formal structures themselves. I would surmise that attitudes and strategies similar to those in the above classification may be discerned in the present-day procedures of choosing and advocating certain mathematical structures as worthy of study.

Another comment relates to the classical question of whether we may learn something from history. To quote the title of a well-known book, history shows us 'distant mirrors' in which, despite distortions, we recognize ourselves. The story of the attempts to interpret exactness in the case of construction and representation in the early modern period was not a successstory but a failure. Maybe, then, it produces little recognition, because as long as the converse is not evident we assume our own efforts to be part of a successstory. Yet choices akin to those in the interpretation of exactness are still being made in mathematics, so it is inviting to consider the classification as a list of possible strategies for interpreting exactness and to ask which of them was the most effective or successful.

If we survey the examples I discussed, the inevitable conclusion seems to be that the winners are numbers four and six: 'regard for the quality of the resulting system' and 'non-interest,' the latter if necessary combined with number one: 'appeal to authority and tradition.' Note that these are precisely the strategies which need few if any explicit arguments; the others require considerable efforts of reasoning. We have also seen that most of the arguments are of poor quality and even the more serious ones have only a short-lived power of conviction.

Thus the more argumentative categories are the losers. I would call number three, 'philosophical analysis of the mathematical intuition,' the glorious loser, impressive for the depth of the arguments and the sincerity of the effort, but leaving posterity unconvinced of the arguments, even if the choices are taken over. In contrast, category two, 'idealization of practical methods,' appears as the colourless loser; the arguments are often petty and the choices are ignored by later mathematicians.

With respect to winning and losing strategies it appears also that restrictive interpretations of exactness tend to lose. Kepler was fighting a lost battle when he argued against any other means of construction than circles and straight lines. Descartes increased the freedom of action in geometry, allowing all algebraic curves in constructions; but that choice implied a new restriction: non-algebraic curves fell outside geometry. Soon, however, this restriction came under pressure and, as we have seen in Bernoulli's example, Descartes' demarcation of geometry was abandoned and 'exact' constructions were achieved for non-algebraic curves as well. Thus we may note here — and other episodes in the history of mathematics may well reveal the same phenomenon — that as soon as inviting mathematics was found to lie beyond restrictive methodological boundaries, the methodologies were soon adapted or, if necessary, forgotten.

Conclusion

All in all, the distant mirror seems to reflect a warning: mistrust methodological arguments, especially if they advocate restrictions, and keep away from questions about the interpretation of exactness. It seems that by the time such questions are explicitly posed, most mathematicians are already moving towards further fields. In the case of the early modern interpretation of exactness of constructions and representations, explicit methodology was generally behind practice, legitimizing procedures which were soon to be accepted as matters of course that needed no legitimation at all.

The interpretation of exactness, then, was largely a post factum phenomenon, legitimizing practices that were already on their way to becoming established. Apparently the search for exactness itself is not what makes mathematics tick; mathematicians are not primarily driven by methodological concerns. Why do mathematicians venture into fields which later require reformulations of the criteria of exactness or other aspects of mathematical methodology? Well, whatever the answer is, they do. Apparently there is a force which tempts mathematicians to enter unexplored, lawless territories. And the mirror says: better yield to temptation than engage in setting the rules.

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