RECOGNITION AND WONDER

Huygens, Tractional Motion and Some Thoughts on the History of Mathematics

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Note: This is the translation of my inaugural lecture, held March 20, 1987, as extraordinary professor in the history of mathematics at the University of Utrecht. The present version differs from the original Dutch text in that the salutation at the beginning and the personal words at the end have been left out and that some errors have been corrected. The original version was separately published (H.J.M. Bos, *Vanuit Herkenning en Verbazing* (Utrecht: OMI Grafisch Bedrijf, 1987); the main text has also been published in *Euclides* 63, 1987, pp. 65-76.

Recognition and wonder. – These two experiences are essential motives behind interest in the past. For that reason I have chosen them as guidelines in presenting my specialty, the history of mathematics, today, upon my accession to office as extraordinary professor.

Recognition makes it possible to distinguish historical events and thus initiates the link of past to present. If recognition or affinity is absent, earlier events can hardly, if at all, be historically described. Wonder, on the other hand, is indispensable too. The unexpected, the essentially different nature of occurrences in the past excites the interest and raises the expectation that something can be discovered and learned. History studied without wonder reduces to a mere listing of recognizable past events, which differ from what is familiar only by having another date.

Let me illustrate this by two examples which for me strongly evoke the two experiences. In the first example recognition is foremost. It concerns a Babylonian clay tablet (see Plates 1 and 2) extant in the Louvre Museum. The tablet dates from about 1750 before Christ.[1] For most of you it probably evokes no immediate experience of recognition. The text contains the solution of a mathematical problem. It is about a rectangle with length, width and area. The sum of length and width is 27; the difference of length and width, added to the area, gives 183. Question: what are the length and the width? Evidently the problem implies two equations in two unknowns. We solve these by first eliminating one of the unknowns. That leads to a quadratic equation, whose roots we find with the familiar "a,b,c – formula":

$$x = \frac{-b \pm \sqrt{b^2 - 4ac}}{2a}\ . \tag{1}$$

[1] The text is analyzed in F. Thureau-Dangin, "Le prisme mathématique AO 8862," *Revue d'Assyriologie et d'Archéologie Orientale* 29, 1932, p. 10, and in O. Neugebauer, *Mathematische Keilschrifttekste* (Berlin, 1935-1937), vol. 1, p. 113; vol. 2, Pl. 35. See Plates 1 and 2.

Plate 1 – Babylonian clay tablet with mathematical text. Photograph from *Revue d'Assiriologie et d'Archéologie Orientale* 29, 1932, Pl. 1, opposite p. 4; see note 1.

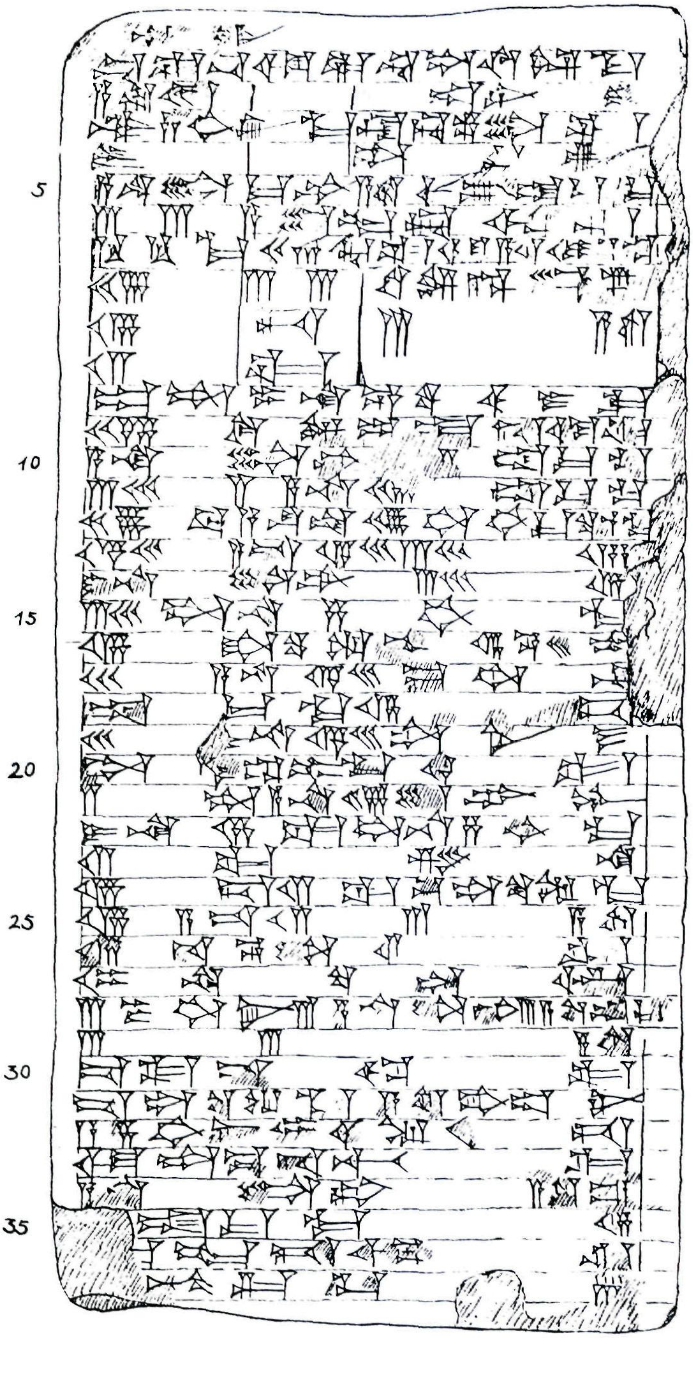


Plate 2 – Transcription of the tablet of Plate 1; from O. Neugebauer, *Mathematische Keilschrifttekste* (Berlin, 1935-1937), vol. 2, Pl. 35; see notes 1 and 2.

Well, some 4000 years ago the Babylonians calculated almost in the same way. To be sure, the formula is not on the tablet, but the numbers that the mathematician wrote down in his calculation (they are in fact not difficult to read)[2] are precisely the ones one encounters when applying the *a,b,c* – formula to the given problem. The modern and the Babylonian procedures are almost exactly the same.

Since 1935, when Neugebauer incorporated it in his grand study of mathematical cuneiform texts, this tablet is a commonplace in writings on the history of mathematics. I meet it often, therefore, and I can assure you that to me it always evokes a strong experience of recognition: The *a,b,c* – formula, quintessence of school algebra, directly recognizable despite a time difference of almost 4000 years! A piece of culture that remained, untouched by the emergence and decline of civilizations. No matter how old, this is undoubtedly mathematics. Freudenthal spoke in his inaugural lecture[3] about 5000 years of international science, and it is especially mathematics that makes us recognize science in the old texts on clay. That recognition is a poignant experience. It is also an invitation to dwell upon old Babylonian mathematics at somewhat more length. But I shall not do that here; I shall proceed to my second example which relates to wonder.

In 1812, during Napoleon's Russian campaign Jean-Victor Poncelet, a military engineer, was taken into captivity. He spent eighteen months in a prisoner of war camp in Saratov on the Volga river. His recreation was mathematics; in particular he studied conic sections: ellipses, parabolas, hyperbolas. Conic sections (see Plate 3) are images of circles under projection. If a circle is placed between a light source and a plane (a wall for instance) it produces a shadow image on the plane; that shadow image is a conic section; the plane, as it were, cuts the image out of the circle's shadow cone. Poncelet considered the images of pairs of circles. Plate 4A shows some images that can be obtained by choosing circles of various position and size (in one plane): the image may be a pair of ellipses, or an ellipse and a parabola; the two curves may be separate or they may intersect in two points. Poncelet posed the question: can every pair of conic sections be obtained in this way? He found (see Plate 4) that if the two conic sections have no more than two points in common, they can indeed be obtained as simultaneous shadow image of two circles. It is not easy to see that that is so; Poncelet proved it and the proof is hard. He also noticed that if the two conic sections have three or four points in common they cannot be obtained by projection of a pair of circles. The latter result is, in fact, easy to see: two circles whose images under projection have three or four points in common must

[2] In Babylonian mathematics numbers are written in a sexagesimal positional system with two basic signs 𒁹 and 𒌋. 𒁹 stands for 1, but, dependently on the position, may also mean 60, 60x60, 60x60x60, or 1/60, (1/60)x(1/60), etc. 𒌋 stands for 10 times 𒁹, and therefore signifies 10, or 600, or 1/6 etc. The interpretation of the number signs depends on the context (in this case the solution of a particular equation). Most lines on the tablet start (to the left) with a number. These numbers are: line 6: 183; l. 8 (in a column): 27, 15, 12; l. 9: 27; l. 11: 210; l. 12: 29; l. 13: 14½; l. 15: 210; l. 16: ¼; l. 17: ½; l. 19: ½; l. 21: 2; l. 23: 12; l. 24: 15; l. 25: 15; l. 26: 15; l. 28: 3; l. 29: 183. Once one has indentified these numbers it is not difficult to trace the other numbers in the text. They are: l. 8: 183, 180; l. 11: 2, 27 (not fully readable); l. 12: 29; l. 13: 14½, 210¼; l. 14: 210¼; l. 16: ¼, ¼; l. 17: 14½; l. 18: 15; l. 19: 14½; l. 20: 14 (not fully readable); l. 21: 27; l. 22: 14; l. 24: 12; l. 25: 12, 180; l. 26: 12; l. 28: 3, 180.

[3] H. Freudenthal, *5000 Jaren internationale wetenschap*. Inaugural lecture Utrecht 9-12-1946 (Groningen, 1946).

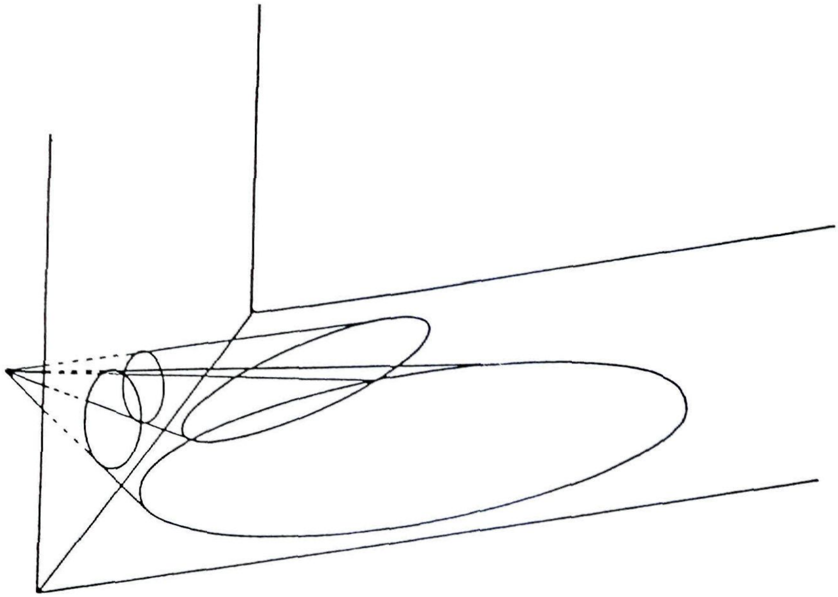
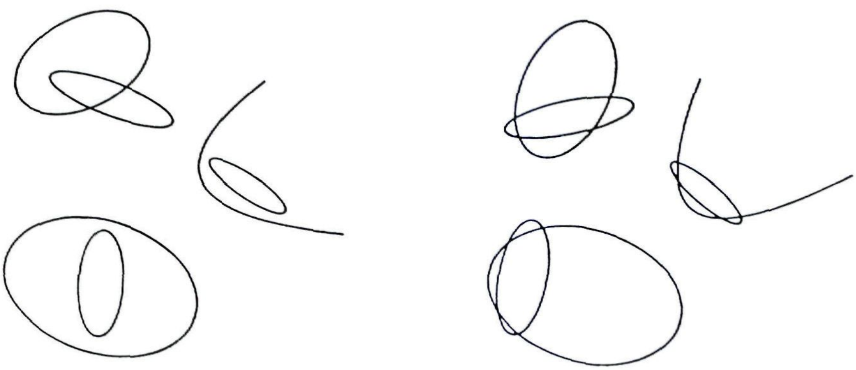


Plate 3 – Two circles in the vertical plane are projected from a point onto another plane. The images are conic sections (in this case ellipses).



A

Plate 4 – Pairs of conic sections. The ones to the left have no more than two points of intersection; they can be obtained as images under projection of pairs of circles. Those to the right have four points of intersection; they cannot be obtained as projective images of pairs of circles.

themselves have three or four common points; but two (different) circles cannot have more than two points in common.

So Poncelet found himself confronted with an impossibility. That was unfortunate for him because the theory he was developing at that time would have been much simpler if every pair of conic sections arose as the projection of a pair of circles.[4] Now such a situation is not uncommon in mathematics and we have come to expect from mathematicians a specific reaction: if within the system on hand something is impossible, then one creates a new, extended system in which it can be done. If it is annoying that with ordinary numbers subtraction is not always possible ($7-5$ can be done but $2-5$ cannot) then one extends the system by creating new numbers, the negative ones. If one thinks that it should be possible to extract square roots from negative numbers as well as from positive ones, one introduces imaginary or complex numbers and then it can be done. If one wants parallel lines in a plane to have a point of intersection, just as non parallel ones, one introduces intersection points "at infinity." One would expect, therefore, that Poncelet did something similar. He could, for instance, introduce an imaginary centre of projection or imaginary circles. Many later mathematicians have interpreted Poncelet's work in this way, because while reading it they were misled by what they were used to. But if one reads carefully it appears that Poncelet did not proceed in this manner. He did not extend the object of geometry, that is, space. What, then, did he do? He introduced an extension of the rules of mathematical argument. He said: although it is impossible that two conic sections with four points of intersection are the images by projection of two circles, we may, nevertheless, argue as if they are. Poncelet codified this extension of the rules of mathematical argument in a principle, his famous and notorious "Principe de Continuité." I shall not attempt to formulate that principle here in general; all Poncelet's own formulations of it were vague and rather unsatisfactory. In the case of the pairs of conic sections the principle implies that one may argue as follows: As any pair of conic sections with two or fewer points of intersection is the image of a pair of circles, therefore it is allowed to reason as if that property applies in general, and hence one may proceed from the premise that the property also applies for pairs of conics with four intersections. This despite the fact that the latter statement is simply false.

Well, that makes one wonder. Wonder, because Poncelet's reasoning reveals a way of thinking which is no longer common in mathematics. Arguments should be correct, they should not proceed from evidently false premises. Poncelet said that one could do precisely that – wonderful. The feeling of wonder entails a challenge and it is rewarding to take up that challenge. For if one takes Poncelet seriously here, accepting that he and other mathematicians in his time used the "Principe de Continuité" and reached results with it, then one perceives a way of thinking which, although unacceptable now, was fairly common at that time. It was a productive way of thinking even (for, surprisingly, almost all theorems that Poncelet derived with his Principle were correct), linking up interestingly with philosophical ideas current in

[4] The theory concerned properties of pairs of conic sections, in particular the property occurring in the "closure theorem" of Poncelet. For more details of this, see H.J.M. Bos, C. Kers, F. Oort, and D.W. Raven, "Poncelet's closure theorem," *Expositiones Mathematicae* 5, 1987, pp. 289-364.

the late eighteenth century.[5] Such aspects are noticed only when one is open to the working of wonder and mindful not to be side-tracked by expectations based on superficial recognition.

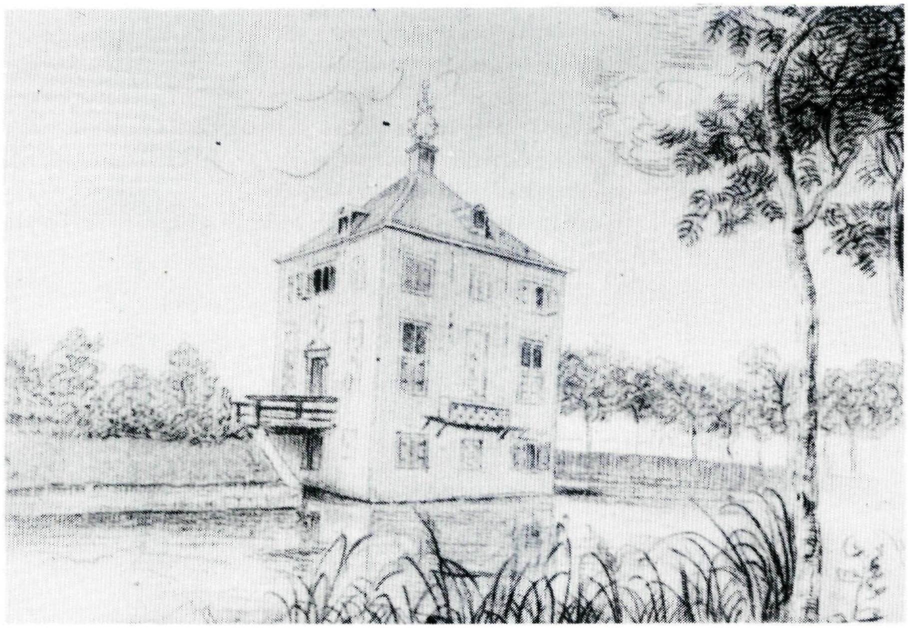


Plate 5 – Hofwijck; drawing by Christiaan Huygens (University Library Leiden, Ms Hug. 14, fol 5r).

After these two short examples I would like to relate in somewhat more detail an episode from the history of mathematics which is part of my present research.[6] It may serve, I hope, to show why that research fascinates me. The episode began in the fall of 1692 in the village of Voorburg near The Hague, or perhaps in The Hague itself. In Voorburg was the summer residence of the Huygens family, "Hofwijck" (see Plate 5). It is still there, a smallish castle, not more than a broad tower in a moat; if you travel to The Hague from Utrecht you can see it from the train. The parking lots of Voorburg station have encroached on much of what once were the formal gardens of the estate. From 1688 on Christiaan Huygens lived at Hofwijck, although during the winter he preferred his apartment on the Noordeinde in The Hague. In 1692 Huygens was 63 years old, his scientific career lay largely behind him. But he was still quite active, as his correspondence and manuscripts bear witness. Among the manuscripts there are ten sheets, dated 29 October–20 November 1692, of a

[5] Confer L.J. Daston, "The physicalist tradition in early nineteenth century French geometry," *Studies in History and Philosophy of Science* 17, 1986, pp. 269-295.

[6] (Note added in 1989.) See H.J.M. Bos, "Tractional motion and the legitimation of transcendental curves," *Centaurus* 31, 1988, pp. 9-62; and "Tractional motion 1692-1767, the journey of a mathematical theme from Holland to Italy," to be published in the *Proceedings* of the conference "Italian scientists in the Low Countries in the XVIIth and XVIIIth centuries" held in Utrecht 25-27 May 1988.

somewhat unusual character.[7] The editors of Huygens' *Oeuvres Complètes* did not quite know what to do with them. They decided not to publish them in full but only to give a summary and some characteristic samples of the text.

I think these sheets deserve more attention. They deal with the mechanical process of traction. Huygens studied in particular the tractional motion that occurs when a heavy body is dragged slowly over a horizontal plane by a cord or a rod which is fixed to the body and whose other end is moved along a straight line. The manuscripts show that Huygens devoted much time and energy to the problem of mechanically implementing this kind of tractional motion in such a way that the trajectory of the dragged object was marked precisely on the horizontal plane. He thought of fixing a weight on top of a drawing pin and dragging the system by a rod (see Plate 6). He sketched various ways of guiding the end of the rod along a straight line. The weight was to be put on top of the pin or perpendicularly below it, connected by a U-shaped rod around the table, evidently to avoid pushing the pin askew. We notice setscrews for placing the plane in an exactly horizontal position. Huygens also sketched (see Plate 7) a little cart tracing the curve while being drawn over the plane and he considered having a body float on a liquid surface (to ensure absolute horizontality); he thought of syrup (which provides much friction) or water.

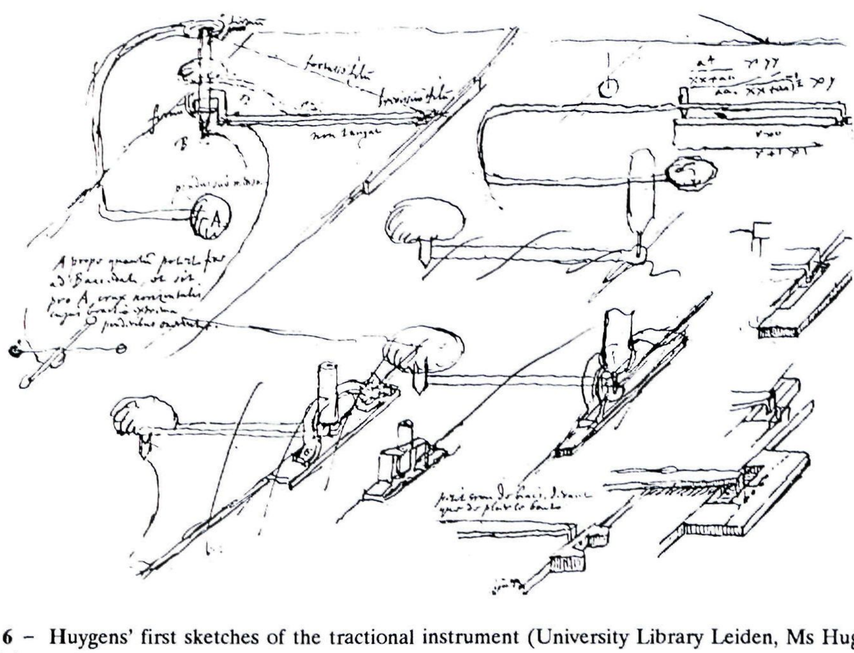


Plate 6 – Huygens' first sketches of the tractional instrument (University Library Leiden, Ms Hug. 6, fol. 59r).

Plate 8 shows the design that ultimately satisfied Huygens best; as far as may be gathered from the documents he did indeed build the instrument and traced the curve with it. Even so he kept looking for alternatives; he elaborated upon the pos-

[7] Leiden University Library, Ms Hug. 6, ff. 59r-68r. Summary and some quotations in C. Huygens, *Oeuvres Complètes*, vol. 10, pp. 409-413. It is my pleasure here to thank the Library for permission to reproduce passages from Huygens' manuscripts.

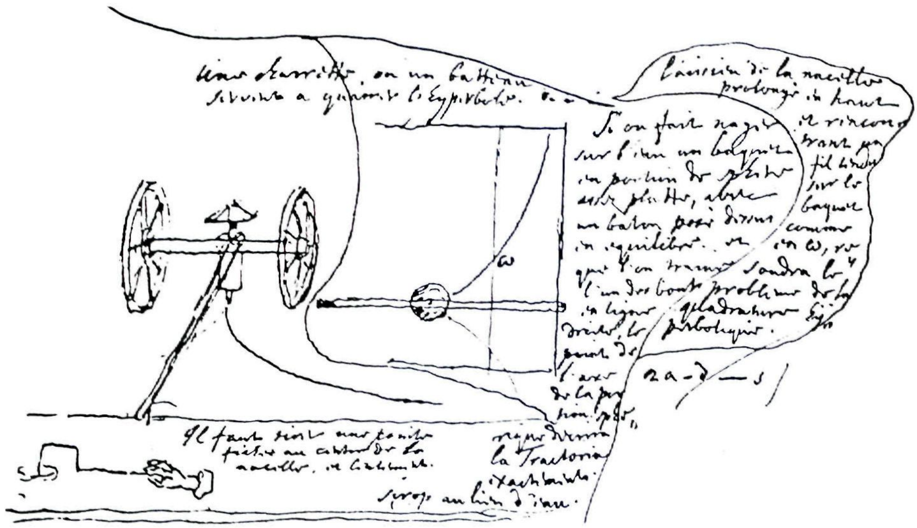


Plate 7 – Detail from Huygens' manuscript on tractional motion (University Library Leiden, Ms Hug. 6, fol. 64r).

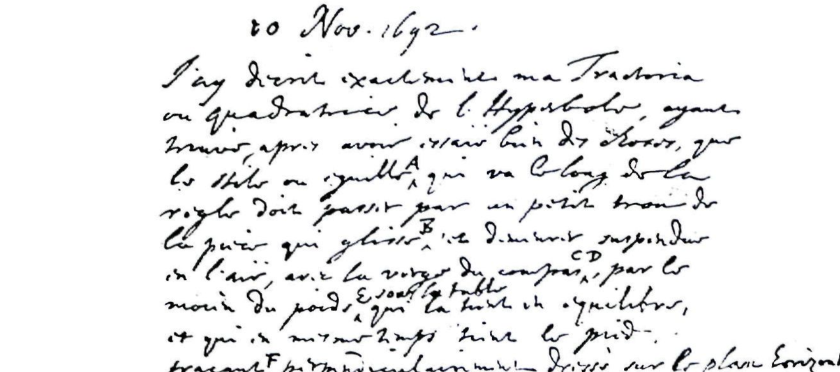


Plate 8 – Huygens' final design of the tractional instrument (University Library Leiden, Ms Hug. 6, fol. 66r).

sibilities of dragging floating objects; he also thought of increasing the friction of the body by using a retort that, after being heated, would suck itself tight to the surface, thus providing considerable friction. Furthermore Huygens devoted several pages to the method of placing the surface exactly horizontally with the help of a level – at that time a recent invention for which *en passant* he worked out a gauging procedure. The horizontal position of the plane is essential; were the plane oblique gravity would deflect the pin's motion from the direction of the cord and thereby the trajectory would be distorted. The possibility of such a sideways distortion rather worried him, at one point he went so far as to remark that the force of attraction does not act along parallel lines but in the direction of the earth's centre, so that in fact there can be only one point of the table where the gravity is truly perpendicular.

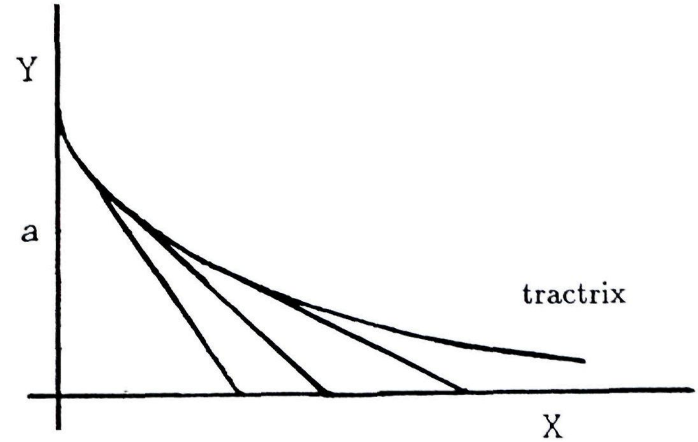


Figure 1

What is happening here? Why did Huygens undertake this study? Was his aim to describe tractional motion as a physical process? Or to make tractional motion applicable to some practical purpose? No, Huygens' principal motivation lay elsewhere. To explain that I must first specify what curve was involved here and which mathematical properties it has. Huygens called the curve traced by the dragged object the *Tractoria*; later the name *Tractrix* became generally accepted. The defining property of the curve is that the segment along the tangent between the axis and the curve remains constant, for that segment is equal to the length of the cord whose other end is moved along the axis; because of the friction the body follows at every instant the direction in which the cord pulls it (see Figure 1). The differential equation of the curve is therefore

$$\frac{dy}{dx} = -\frac{y}{\sqrt{a^2 - y^2}}\,, \tag{2}$$

and one easily derives the equation of the curve by integrating the differential equation:

$$x = a\,log\frac{a + \sqrt{a^2 - y^2}}{y} - \sqrt{a^2 - y^2}\,. \tag{3}$$

Huygens himself did not describe the relation between the coordinates x and y by means of this equation. He did know the relation but formulated it in more geometrical terms. In particular he was aware that it involved a logarithm. He interpreted

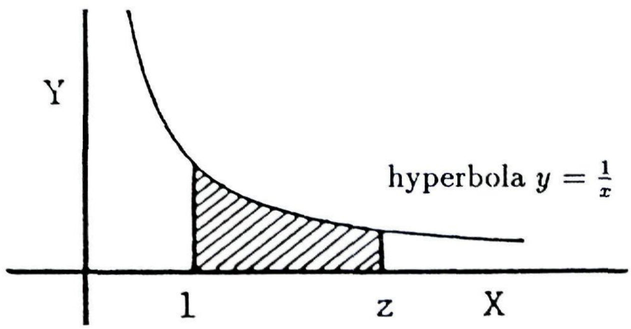


Figure 2

that logarithm geometrically, namely as what he called the "quadrature of the hyperbola." We recognize in that expression the relation we write in terms of integration:

$$\log z = \int_1^z \frac{dx}{x} \,. \tag{4}$$

Indeed (see Figure 2) the integral in the right hand term denotes the area (the "quadrature") of the shaded part under the hyperbola with equation

$$y = \frac{1}{x} \,. \tag{5}$$

Let me return to the question of Huygens' motive for pursuing these investigations. Was his aim a mathematical description of the natural phenomenon of tractional motion? No, for that purpose Formula (3) would have been sufficient; the further investigation of methods to trace the curve accurately is superfluous. Nor, apparently, is there any practical purpose in the various manners of dragging that Huygens explored. Was it the precision of curve tracing, then, that was foremost in his mind? In a sense yes; Huygens stressed that the instruments should trace the curve very accurately. But there must have been more to it, because for mere accuracy of tracing Huygens could have used an easier and much more precise means, namely logarithm tables. With the help of such tables he could apply Formula (3) to calculate the coordinates of points of the curve with a precision that could never be attained by tracing on paper. If Huygens' concern was precision, then it was not practical but ideal precision.

To find what did lay behind Huygens' concerns, let us see what he wrote himself about the motivation of his study. Here are some characteristic passages. In the first he compares his instrument with the classical geometrical instruments of straightedge and compass:

> It should be admitted that, if my curve is supposed or given, one has the quadrature of the hyperbola. So if I find some means to draw it as exactly as a circle is drawn by an ordinary compass, would I not have found that quadrature? ... It is true that I need the parallelism of a plane to the horizon; but that is possible, not in utmost precision, but like the straightness of a ruler. For the rest I draw my curve almost as easily as a circle and the machine I use comes very close to the simplicity of the compass.[8]

With his first sketch of the body that is dragged over a fluid surface (see Plate 7) he noted that that construction "will solve the problem of the hyperbolic quadrature,"[9] and with the drawing of the cart on the same sheet: "A little cart or a boat will serve to square the hyperbola."[10]

We conclude that Huygens' tractrix did not serve as a mathematical model to describe tractional motion, but conversely tractional motion so to speak helped Huygens to mathematically lay his hands on the tractrix, and thereby on the quadrature of the hyperbola and on all other geometrical problems that depended on it.

Why was that necessary? Because apparently the quadrature of the hyperbola (we would say the logarithm function) was not considered truly known, and because a curve as the tractrix could not just simply be taken to serve as a solution of that problem. Now who decided these things? In this case it was Descartes.

Some fifty years earlier Descartes had introduced the techniques of analytic geometry through which curves could be represented by equations. He had also forcefully voiced his opinion about what was acceptable in geometry and what was not, asserting that curves were acceptable only if their equations were algebraic, that is, if these equations involved no other algebraic operations than addition, multiplication, division and subtraction. The tractrix does not have such an equation, it is a transcendental, that is, a non algebraic curve. Huygens had realized that no algebraic equation could be formed for his curve and that therefore within the Cartesian demarcation of geometry the curve wasn't acceptable. That was what he objected to.

I should add that at the time Descartes posited his demarcation of geometry it was not a restriction but an extension of the field. Before Descartes the boundaries of geometry, as far as they were explicitly formulated, were much more restricted. But Huygens already felt that the demarcation of Descartes was too restricted. He expressed that feeling on one of the first sheets of his study:

> Descartes was wrong when he dismissed from his geometry those curves whose nature he could not express by an equation. It would have been better if he had acknowledged that his geometry was defective in so far as it did not extend to the treatment of these curves.[11]

We may say that Huygens was primarily concerned to confer the predicate "geometrical" on his curve. This is also evident in the publication he devoted to the curve one year later. He explained tractional motion (although he did not give details about his instrument) and wrote:

[8] Fol. 62r, also quoted in C. Huygens, *Oeuvres Complètes*, vol. 10, p. 412n. Huygens wrote in Latin and French; here and in other quotations the translation is mine.

[9] Fol. 64r, also quoted in *Oeuvres*, vol. 10, p. 411n.

[10] Fol. 64r.

[11] Fol. 60v.

> If this description, which by the laws of mechanics must be exact, could pass for geometrical, in the same way as those of the conic sections, performed by instruments, one would thereby have the quadrature of the hyperbola, and together with that the perfect construction of all the problems that can be reduced to that quadrature.[12]

And in a letter to Leibniz from this time Huygens noted with some anxiety that Bernoulli had expressed doubts about the geometricity of the curve.[13]

Huygens, then, was not merely interested in making a precision instrument; his concern was to extend the boundaries of pure geometry. Now that is cause enough for wonder: a mechanical legitimation within mathematics? To extend the boundaries of pure geometry with weights, levels, carts, even with syrup? The wonder is inviting. Let us therefore examine the background of this way of thinking.

To do so I first note that it was not just a Huygensian idiosyncracy to handle the legitimation question in such a tinkering do-it-yourself manner. The motif of tractional motion as mechanical legitimation of transcendental curves appeared repeatedly in the period of the early differential calculus. It was not a central theme in the development of mathematics but a recurrently interwoven motif, recognizable and familiar for contemporaries. Huygens published his ideas in 1693.[14] They were immediately taken up by Leibniz who, characteristically, announced that he himself had hit upon the same idea earlier and produced a draft of a generalized tractional machine by which all manner of differential equations could be solved.[15] Somewhat later De Moivre learned about a tractional instrument devised by a certain John Perks (see Plate 9) and found it interesting enough to have an article about it inserted in the *Philosophical Transactions* of 1706 entitled "The construction and properties of a new quadratrix to the hyperbola."[16] In 1728 the Italian scholar Poleni took up the theme again. He designed a tractional instrument (see Plate 10) and sent copies of it to three colleagues. In his covering letter he argued, again, that by means of his instrument the problem of the quadrature of the hyperbola was now for the first time solved in a geometrically acceptable manner. His correspondents reacted positively. One of them, Jacopo Riccati, took the opportunity to formulate at some length his opinion on the role of constructions in pure mathematics. Poleni published the fruits of his research a year later and added the letters he had received in an appendix.[17] Also in Euler's work,[18] and in the writings of Vincenzo Riccati (a son of

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[12] "Lettre de Mr Huygens à l'Auteur," *Histoire des Ouvrages des Scavans*, February 1693, pp. 244 sqq; *Oeuvres*, vol. 10, pp. 407- 417, on p. 411.

[13] Huygens to Leibniz, 17-9-1693, *Oeuvres*, vol. 10, pp. 509-512, on p. 510.

[14] See note 11.

[15] G.W. Leibniz, "Supplementum geometriae dimensoriae," *Acta Eruditorum*, September 1693; in *Mathematische Schriften*, ed. C.I. Gerhardt (Berlin, 1849-1863), vol. 5, pp. 294-301.

[16] J. Perks, "The construction and properties of a new quadratrix to the hyperbola," *Philosophical Transactions*, 1706 (no. 306).

[17] Joh. Poleni, *Epistolarum mathematicarum fasciculus* (Padua, 1729), letter no. 7.

[18] L. Euler, "De constructione aequationum ope motus tractorii aliisque ad methodum tangentium inversam pertinentibus," *Commentarii Academiae Scientiarum Petropolitanae* 8, 1736/1741, pp. 66-85; in L. Euler, *Opera Omnia*, Ser. 1, vol. 22, pp. 83-107.

Jacopo)[19] we encounter the theme of tractional motion. In all these cases the writer's interest was not primarily in the mathematical description of tractional motion, but, conversely, in the legitimation of transcendental curves as acceptable means of solving problems in mathematics.

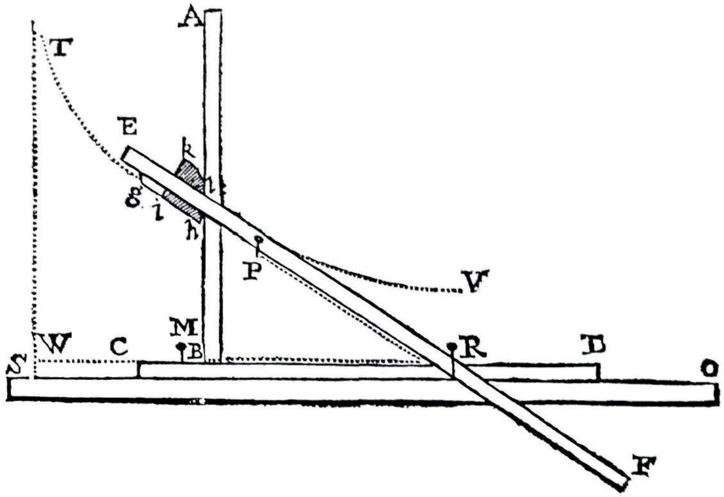

Plate 9 – John Perks' tractional instrument for the quadrature of the hyperbola; see note 16.

Let me return now to Huygens. He was not alone in his interest in tractional motion in connection with the issue of the legitimate bounds of geometry. Our initial wonder has directed us to aspects of earlier mathematics that we do not recognize so easily. Let us try to further analyse this unfamiliar way of thinking to see what we have lighted upon. Huygens worked in a literal (but not the usual) sense at the frontier of his science. His concern was to remove the confines that mathematicians before him had drawn around geometry, and thereby to make acceptable and legitimate certain objects that previously were kept outside geometry. In doing so he fell back on mechanical imagery related to the classical means of geometrical construction, straightedge and compass. His efforts at legitimation had a definite aim: a legitimized object could serve as solution. Huygens thought that if the tractrix was legitimized as an acceptable geometrical object, the problem of the quadrature of the hyperbola would be solved, and thereby also all other problems that could be reduced to that quadrature.

Legitimation bears upon the very structure of the mathematical enterprise: by legitimizing a mathematical object one may make certain problems solvable which earlier could not be solved. Thus we are indeed dealing with a crucial issue within mathematical activity: when may a problem be considered as solved? When may an object be accepted as sufficiently known? What are the criteria for solution and knowledge in geometry, or in mathematics generally? The debate on legitimation in which Huygens engaged was in fact part of an endeavour to establish such criteria.

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19 Vincenzo Riccati, *De usu motus tractorii in constructione aequationum differentialium* (Bologna, 1752).

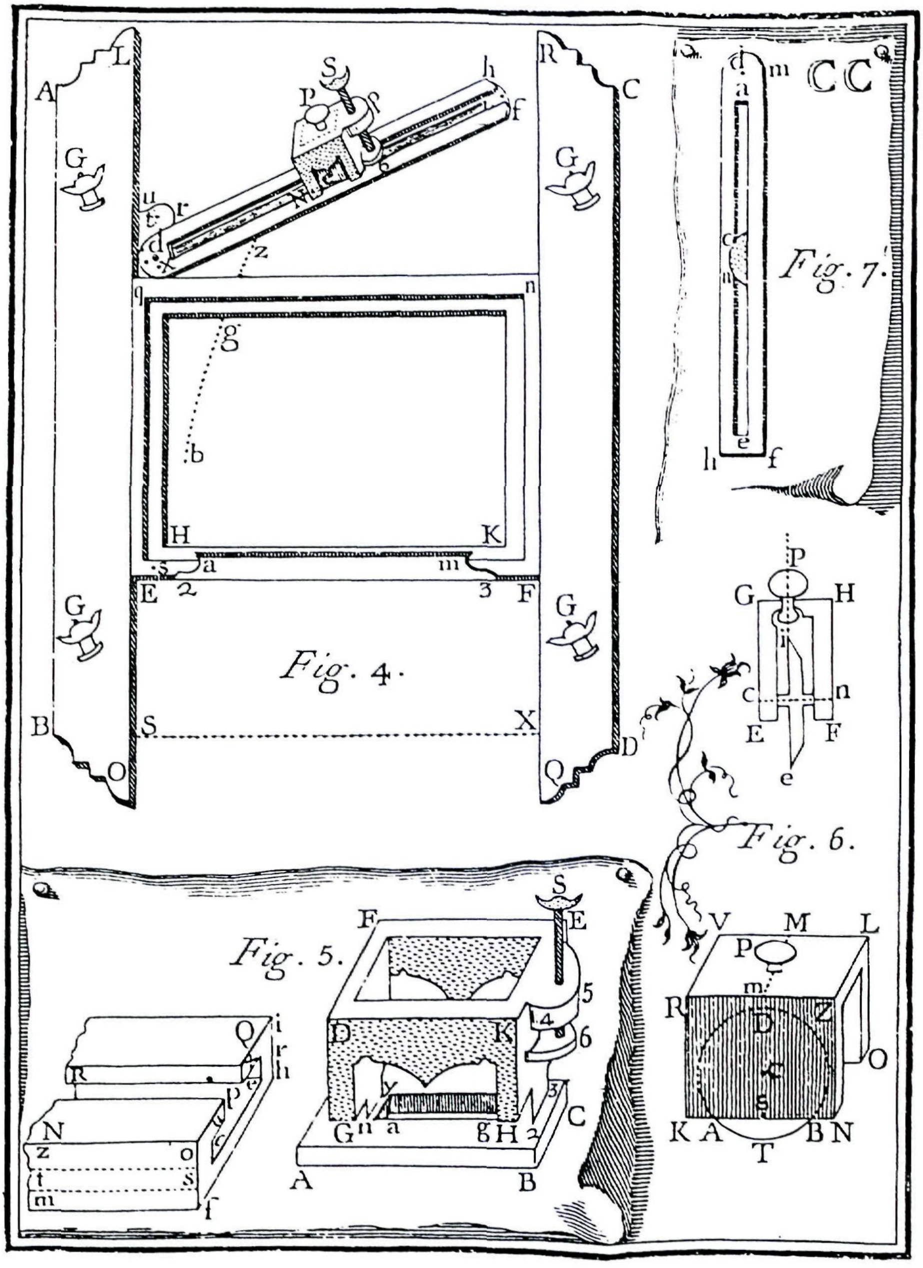

Plate 10 – Poleni's tractional instrument; see note 17.

That endeavour was not restricted to the theme of tractional motion which I have used here as an illustrative example. We find similar discussions in many other regions of mathematics; they were occasioned for instance by the introduction of algebra in early modern geometry, and by the treatment of transcendental curves within the early infinitesimal calculus. These and similar innovations were often accompanied by discussions on the question: When is a problem solved, when do we really know an object?

At this point, of course, we have to expect an obvious objection: Don't mathematical problems just have a solution? An answer, a number or a proof that is either correct or false? Surely we don't need complicated legitimation in a matter of false or correct? The answer is: no, the matter is not that simple. Mathematics is an exact science, certainly, but first mathematicians have to reach some consensus about the question of what exactness means. The meaning of exactness cannot be arbitrarily imposed; mathematical reality enjoins strong restrictions. But ultimately exactness is a matter of consensus. That consensus grows and changes over time. The episode of tractional motion is of interest because it provides us with an opportunity to study that process of growth and change. We see how mathematicians try to give meaning to the concept of exactness. By legitimizing the tractrix the concept of exact geometrical solution is extended in such a way that the problem of the quadrature of the hyperbola becomes solvable. The process did not end there; subsequently ideas about exactness within mathematics moved again. But that does not make the process that we can watch here less important.

The question was: when is a problem solved? Or, when is an object sufficiently known? It arose not only with respect to the quadrature of the hyperbola or the tractrix; time and time again it was posed and discussed during the period of early modern mathematics. It is revealing to study what mathematicians wrote about the question. Such a study helps in understanding the imagery and the terminology in mathematical texts. It explains, for instance, why for a long time curves were not primarily represented by their equations but by geometrical construction procedures, and why until well into the eighteenth century one called "construction" of a differential equation what we call its solution. The study also sheds light on a number of seemingly peculiar phenomena and developments within mathematics. Such is the case with smaller matters, as the fascination with tractional motion, or, for instance, the remarkable preference, expressed by several mathematicians around 1700, for interpreting integrals as arclengths rather than as areas. But the understanding gained by pursuing the questions about the nature of solving and knowing in mathematics may also concern larger developments, such as the case of the "construction of equations" which for more than a century took a rather prominent place within mathematics and then vanished in a short time.

Still, the arguments about the question of when a problem is solved remain in a certain sense elusive and unreal. Elusive because the question is in principle undecidable; hence one could, if desired, disagree forever. And unreal because the arguments do show a lack of depth and quality. Ultimately that lack is the main reason for our wonder about Huygens' tinkering with sliding weights, carts and syrup: it seems so meagre. And there are other such examples; for instance Leibniz' seriously meant suggestion not to carry logarithm tables for navigational calculations aboard ship but simply to suspend a chain freely in front of graph paper and read off the logarithms directly, using some mathematical knowledge about the catenary curve.[20] One meets more quasi-practical arguments of this sort and one wonders: could this be meant seriously? This remark leads us onto a somewhat different trail. For the important thing about the process is not the arguments but what is behind them. The questions was: when is a mathematical object sufficiently known to serve as the solution of a problem? Now "known-ness" is not an objective matter but a subjective one. What is considered known at present may have been seen as very problematical by an earlier generation. The logarithmic function, for instance, was problematical for Huygens; later mathematicians left it in equation (3) without further ado. Meanwhile they had not learned essentially more than Huygens knew about the function. They had done something else: they had become used to it. This explains why discussions about the interpretation of known-ness die out after some time, why they have legitimatory overtones, why they are undecidable and why the reasonings often seem so meagre. However seriously the arguments were meant, what was really at stake was the process of habituation that went on underneath.

Of course such processes of habituation occurred not only in early modern mathematics, they belong to science in general. They are little studied. I think they deserve more attention because they are interesting and important. And thus the story of tractional motion may also serve as an illustration of the kind of processes in the development of science about which the history of mathematics may provide deeper insight.

I have presented to you an episode from the history of mathematics, the story of Huygens, tractional motion and the tractrix. I have mentioned the questions which, stirred by wonder, one comes to ask about it and the trails one is led onto if one follows those questions. It is a small episode, taken from much broader developments. I hope that, nevertheless, the story clarified and illustrated an approach which one could call history of ideas in mathematics. That approach primarily concerns the fundamental concepts of mathematics and the ideas, images and questions that arose among earlier mathematicians about these concepts. This is a part of the history of mathematics in which I feel much at home and I do think it important; but it is certainly not the only part of the field.

The field which has gained a further academic confirmation by the present extraordinary professorate is much broader. It concerns a time period of more than 5000 years. Cultures all over our globe have produced recognizable mathematics and have contributed to the development of that knowledge. In the historical study of these developments one may concentrate on ideas and concepts, but there are several alternative approaches. There is the social history of mathematics – an approach which I mention here as a matter of course, remembering with some wonder how much resentment and distrust could be stirred by combining the words mathematics and social some fifteen years ago. There is institutional history and the more biographically oriented approach. There is the history of mathematics in different cultures or countries, not least the mathematics of the Netherlands. It is also very rewarding to study the development of mathematics within the broader cadre of the

[20] G.W. Leibniz, "De la chainette, ou solution d'un problême fameux, proposé par Galilei, pour servir d'essai d'une nouvelle analise des infinis, avec son usage pour les logarithmes, et une application a l'avancement de la navigation," *Journal des Sçavans*, March 1692; in *Mathematische Schriften*, ed. C.I. Gerhardt (Berlin, 1849-1863), vol. 5, pp. 258-263, in particular p. 262.

general history of science, tracing the role of mathematics in the human endeavour to understand and control nature, in the successes and failures of that enterprise and in the succession of styles, currents, -isms, revolutions and normal periods which we have come to distinguish in the long history of that endeavour. I am convinced that all these approaches are meaningful and that they may find a place within teaching as far as the time, ability and interest of the students allow that. For research the constraints of time and ability impose a stronger restriction. My personal choice of research themes is in the sphere of what I called above the history of ideas in mathematics. That is because it suits me, not because it should be considered the best approach to the mathematical past.

There remains one crucial question: what is history of mathematics about? What is the object that is investigated and taught about? Is it mathematics as it was in the past (and which is now dead)? No, that formulation disregards the importance of recognition and wonder. Recognition and wonder remind us that history is done by and written for people who are alive now. To conceive of the object without keeping that relation with the present in mind is misleading; it is impossible to switch off the present while studying the past.

And here appears the well-known metaphor of history as collective memory; that metaphor satisfies me best in answering the question of what history of mathematics is about. History of mathematics is history and thereby part of the collective memory of our culture. In that way the field, together with history of science, finds its place within general history. It is a modest place, and not easily accessible, because to revive that particular memory requires a considerable familiarity with mathematics. However, history of mathematics is also, and in particular, the collective memory of the community of mathematicians. Here lies, I believe, the primary importance of the field, and therefore also the answer to the question of what history of mathematics is about. As for individuals, or for nations, so also for a scientific field, a community of scientific colleagues, it is important to take memories seriously, not to suppress one's own past, if necessary to recall half forgotten events, and in any case not to dismiss this past as unimportant, puerile or immature. History of mathematics thus assists in the self-reflection of a discipline. That, I honestly think, is very important. I hope to contribute to that self-reflection by my work.

Summary

Two short examples from the history of mathematics, a Babylonian mathematical clay tablet and a principle advocated by the early 19th century geometer Poncelet, are adduced to illustrate the role of recognition and wonder in the historian's activity. Then Christiaan Huygens' 1692 studies on tractional motion are discussed. These studies led Huygens to define and investigate the *Tractrix*, which is the curve generated by the simplest kind of tractional motion. It is shown that Huygens' interest in the curve and in the motion generating it originated in a concern about geometrical exactness. Finally some thoughts are expressed about history of mathematics and its function with respect to the collective memory of the community of mathematicians.

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