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TRACTIONAL MOTION AND THE LEGITIMATION OF TRANSCENDENTAL CURVES

by

H.J.M. Bos

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1. INTRODUCTION

Perhaps it all started with Perrault's watch. Leibniz told the story later [1]: Claude Perrault, at gatherings of the learned in Paris in the 1670's, would take out his watch, put it on a table and move the end of the watch-chain along a straight line; then he would ask the mathematicians present what curve was traced by the watch. Leibniz solved the problem but did not divulge his solution until about 20 years later. By that time Huygens had become interested in the problem. While he was thinking about the curve traced by a body dragged over a horizontal surface he may have remembered Perrault's watch - for he was in Paris at the time when Perrault posed the problem [2] - but if so, he did not mention the matter in his letters or private notes. Huygens published his results in 1693 and very soon the theme of tractional motion was taken up by others: the Bernoulli's, l'Hôpital and Leibniz. During the eighteenth century the theme reccurred several times in mathematical studies.

Tractional motion had a special fascination for mathematicians. The seventeenth century view was that curves were described by motion; motion therefore had a foundational role. Perrault's watch traced a transcendental curve (Huygens called it Tractoria, Leibniz suggested Tractrix; the latter name was the one that stuck), and the variants of tractional motion that were studied later also involved transcendental curves. In the prevalent Cartesian view of geometry such curves were ungeometrical. Tractional motion, however, seemed as creditable as any of the motions that traced the curves which were considered geometrically acceptable. The simplicity and continuity of tractional motion could therefore serve as an argument to legitimate the tractrix and its variants as truly geometrical curves. This was the deeper reason for the interest in these curves and motivated the various studies

which I shall discuss below. These studies date from a comparatively brief period in the 1690's. However, the interest in tractional curves and in their legitimate place in mathematics remained alive much longer; the matter was discussed in print as late as the 1760's [3], almost a hundred years after Perrault had challenged mathematicians in Parisian salons.

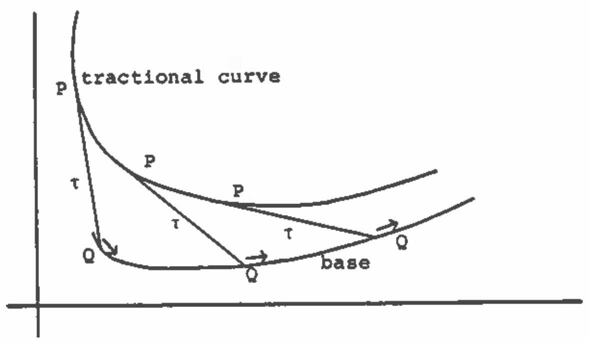
The early studies on tractional motion, then, are of interest because they bring to light ideas about the foundations of geometry and analysis towards the end of the seventeenth century, a period in which these ideas were changing rapidly.

The main aim of the present article is to illustrate these ideas with reference to the work on tractional motion done by Huygens, Leibniz, the Bernoulli's and l'Hôpital (Sections 4-6). A sketch of the background to these ideas is given in Section 3, and Section 7 contains an appraisal of the contemporary debates about them. A brief survey of the various kinds of tractional motion is given in Section 2.

2. TRACTIONAL MOTION

Tractional motion occurs when a heavy object is dragged over a horizontal resisting surface by a chord (whose motion is frictionless) of constant or variable length, the free end of which is moved along a straight line or a curve in the plane. It is supposed that, because of the friction, the direction of the motion of the heavy object is always along the chord. The path described by the heavy object is called a tractional curve.

Figure 2.1



Let (see Figure 2.1) the chord be PQ with length τ. The end Q is moved along a straight or curved line, called the base, whereby its other end P (where the heavy body is assumed to be) describes a curve that is characterized by the property that in all of its points the chord is tangent to it. There are four main variants of this process:

A) The base is a straight line and the length τ of the chord is constant. In this case the resulting curve is the common tractrix (see Section 4).

B) The base is a straight line and the length τ of the chord is variable. We take (see Figure 2.2) the X-axis as the base and put $OQ = r$. In the "Bernoulli problem", discussed in Section 5, the ratio between τ and r is assumed to be

constant:

(2.1) $\tau = pr$.

Leibniz mentioned the generalization of this case in which there is an arbitrary algebraic relation

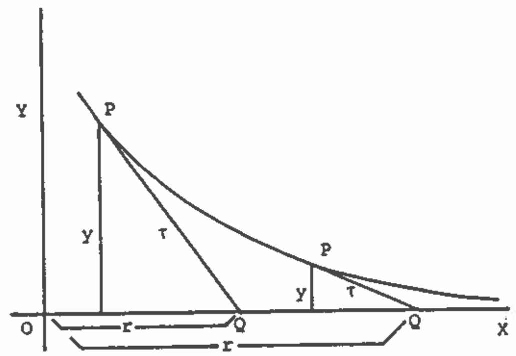
(2.2) $\tau = \tau(r)$.

between τ and r. He studied in more detail the case in which τ depends on y,

(2.3) $\tau = \tau(y)$

(see Section 6).

Figure 2.2



C) The length τ of the chord is constant but the base is a curve. This case occurs in studies by Euler and Riccati [4].

D) The base is a curve and the length of the chord is variable. This case is briefly touched upon by Leibniz (see Section 6), but no explicit calculations occur in the literature.

3. THE BACKGROUND AND THE ISSUES

Problems about tractional motion are representative of a wider class of problems that had a decisive influence on the development of mathematics around 1700. These problems concerned hitherto unknown or unfamiliar transcendental curves and relationships. Although analytical methods (provided by the newly developed infinitesimal calculus) were used, the problems were considered solved only if the required curves or relations were made known in a geometrical way, that is, by some construction. Geometrical constructions had to satisfy criteria of acceptability and adequacy; certain constructions were allowed, others not, and some were preferred to others. However, there were no generally recognized criteria of acceptability and adequacy for the constructions of transcendental curves, nor could such criteria be inferred in an obvious way from constructional practice in other parts of geometry. Hence there was no clear answer to the question that is fundamental in any tradition of problem solving: *When is a problem adequately solved?*

The then current paradigm for the study of curves, Cartesian geometry, had developed a clear-cut answer to that question as far as algebraic curves and relationships were concerned. But Cartesian geometry emphatically rejected transcendental curves as ungeometrical, and the criteria of adequacy developed within Cartesian geometry were in principle inapplicable to non-algebraic relations. In fact, the new problems were instrumental in what may be called the overthrow of the Cartesian paradigm. New methods of enquiry were developed to deal with transcendental curves. In the discussions about the construction of transcendental curves the Cartesian point of view was criticised, and mathematicians came to reconsider geometrical construction and its relation to mathematical exactness, simplicity, practical precision and technical feasibility.

To sketch the background to these discussions it is necessary to give a brief review of relevant earlier ideas and practices, notably: the conception and function of construction, the Cartesian paradigm and the methods available for constructing transcendental curves. [5]

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Constructions, taken in a broad sense, serve to generate new elements within given geometrical configurations. Thus one can construct a bisectrix of a given angle, or a circle through three given points, or a perpendicular to a given line at a given point, or a parabola through a given point with given axis and vertex, etc. There are certain well-defined means for performing constructions. These means vary according to traditions or schools of geometry. Ruler and compass (straight lines and circles) have almost always been accepted as means of construction. When ruler and compass turned out to be insufficient, geometers considered many other means: instruments for tracing conics or other curves, generation of curves by intersection of surfaces, construction by sliding rulers, curve-tracing by mechanically defined motions etc.

Ever since the classical beginnings of deductive geometry there have been discussions about the acceptability and adequacy of these means of construction. The discussions involved various implicit or explicitly articulated criteria for accepting, rejecting or preferring certain means of construction. These criteria were very important because they determined the kind of solutions that geometers looked for while dealing with their problems. The key concepts behind these criteria were exactness and simplicity; constructions had to be precise and as simple as possible. But simplicity and exactness are ambiguous concepts, especially with respect to geometrical construction, which involves an intricate mixture of practical and conceptual features.

Although constructions are described in a terminology that evokes the image of their actual physical execution, their relation to any practice of drawing geometrical figures is indirect and complex. Constructions are occasionally supported by more or less precise figures, but they are primarily mental operations, not physical ones. The motions involved in constructions of curves, for instance, are to be imagined rather than actually implemented.

Some mathematicians did derive criteria for the acceptability of constructions on the basis of practical simplicity and precision (by considering the quality of the instruments for instance). But more often one looked at how readily the procedures and motions involved in the constructions could be imagined. In neither case was the formulation of the criteria a straightforward and obvious matter. But geometers always felt that their constructions had to stay within some canon of allowed procedures.

Thus constructions were the canonical descriptions of certain processes that were primarily mental. They were used mainly for solving problems and proving theorems. The standard geometrical problem was one in which it was required to find a figure or a part of a figure with a prescribed property. The solution consisted of a construction together with a proof that the constructed figure did indeed satisfy the requirements. Sometimes proofs of theorems required auxiliary lines or figures; in that case these were also provided by constructions.

As an extension to this primary use, constructions had another more general function, namely the representation of geometrical objects. This function was of particular importance with respect to curves. I shall use the term "representation of a curve" as a technical term to denote a description of the curve that was sufficiently informative for the curve to be regarded as known or given. A representation is not the same as a definition.

The catenary, for instance, can be defined as the form of a free hanging rope or chain, but that definition does not characterise the curve sufficiently for it to be considered as known; it is therefore not a representation. On the other hand, the expression "parabola" was usually considered to be a sufficient representation of that curve, although it is a mere name and not a definition. A curve may be defined by specifying one of its properties, and for that purpose it is enough that the property determines the curve uniquely. But if a curve is represented by one of its properties, more is required: the property should be sufficiently familiar and recognisable for mathematicians to accept the curve as being given or known unproblematically.

Before Cartesian geometry introduced equations for curves, the only way to represent a hitherto unknown or unfamiliar curve was to describe the process by which that curve was constructed. In that case the construction of the curve served as its representation. It was some time before the Cartesian equations were accepted as adequate representations of curves; initially they were merely seen as an analytical way of describing one property of the curve, which in itself was not enough to make the curve known. By a process of habituation, by the end of the seventeenth century equations had become accepted as sufficient representation of algebraic curves.

For transcendental curves a similar process of habituation occurred, but about 50 years later. Around 1700 the means to express these curves analytically (equations involving exponentials, integrals, infinite series, symbols as "sin" and "log" for the elementary transcendental functions etc.) were still being developed and they were certainly not considered as sufficiently informative to represent a transcendental curve. Thus the representation of transcendental curves was a major issue in mathematics at that time. In discussing the matter geometers approvingly or critically referred to ideas about representation and construction such as they had been developed within Cartesian geometry.

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Cartesian geometry [6] provided a method for solving geometrical problems. Basically these problems were of two kinds. Either it was required to construct a point and the problem allowed only a finite number of solutions; or it was required to construct an infinity of points, that is, a curve. I shall call these two kinds zero-dimensional and one-dimensional construction problems respectively. To trisect an angle, for instance, is a zero-dimensional problem (only one point on the line trisecting the angle needs to be constructed); determining the locus of the centres of circles tangent to two given circles is a one-dimensional problem.

For both types of problem Cartesian geometry provided a method of analysis and a method of construction. The method of analysis was algebra. In the case of zero-dimensional problems the analysis translated and reduced the problem to a polynomial equation in one unknown, the roots of which determined the points sought. The algebraic analysis of a one-dimensional problem resulted in an equation in two unknowns, satisfied by the pairs of coordinates of points on the curve. The analytical method was only applicable if the data of the problem and the required properties involved algebraic relationships only. But in the classical and early modern tradition of geometrical problems this was almost always the case; Descartes considered as ungeometrical the few exceptional cases, occurring for instance in connection with transcendental curves such as the spiral and the quadratrix.

Thus the result of the analysis of a problem was always an equation. But that equation was not considered as the solution of the problem. Nor, in the case of an equation in one unknown, was an algebraic expression of its roots

considered to be an adequate solution. The original problems were geometrical, therefore they required a geometrical solution, that is, the points or curves sought had to be geometrically constructed. Analysis was only half the business of problem solving; the equations had to be translated into constructions.

In Cartesian geometry construction was performed by intersecting curves. In classical ruler-and-compass constructions points were determined by the intersection of straight lines and circles; if these means failed the Cartesian geometer constructed points by intersection of conics or of higher-order curves. The means of construction, therefore, were curves, and the methodological questions about construction concerned particularly these curves. The questions were: Which curves are acceptable as means of construction? How are they themselves traced or constructed? What criteria of exactness and simplicity govern the choice of constructing curves?

According to Descartes all algebraic curves were in principle allowed in geometry. To support this assertion he gave a complicated argument. He considered curves to be traced by special combinations of motions. Starting with the straight line and the circle, traced by the classically accepted circular and rectilinear motions produced by compass and ruler, Descartes imagined pairs of lines or curves, one fixed in the plane and one moving while connected to the plane by some linkage system. During the motion the point of intersection of the two curves traced a new curve which, Descartes claimed, was acceptable in geometry because its generation was as clearly conceived as the generation of the curves he started with. By repetition of these processes with the newly found curves, further curves were generated, all acceptable as geometrical, notwithstanding the complexity of the chain of motions that produced them. Descartes claimed that all these curves admitted algebraic equations and that, conversely, every algebraic curve could be generated in this way, so that the class of algebraic curves coincided precisely with the class of geometrically acceptable curves. (The weak point in his argument was the statement that any algebraic curve could be traced in the way he described. Descartes gave little further argument; only in the nineteenth century was a proof of this assertion given.) While accepting motions guided by linkages, Descartes rejected other combinations of motions, notably those where the connection was by rolling or unwinding chords from circular arcs (such procedures were used in the generation of curves like the quadratrix and the spiral.)

Thus Descartes' criterion for accepting curves within geometry (his exactness criterion) was not algebraic; it concerned motions of geometrical objects. From that starting point Descartes argued that the acceptable curves were precisely the algebraic ones.

His criterion of simplicity, however, was primarily algebraic. He stated that curves were simpler in as much as the degrees of their equations were lower. As we shall see, this criterion underlay his canon of choosing the constructing curves for particular problems. Descartes was well aware that this algebraic criterion of simplicity could be in conflict with more directly geometrical criteria, like the simplicity of the shape of a curve or the simplicity of the process by which the curve is traced. He nevertheless adopted the algebraic criterion.

With this concept of geometrical construction, Descartes provided standard procedures by which an equation found at the end of the analysis of a problem could be translated into the required construction of the problem. For zero-dimensional problems this procedure was called the "construction of equations", which meant in fact the geometrical construction of the roots of an equation in one unknown. The procedure provided standard constructions for

(in principle) any polynomial equation in one unknown. Linear and quadratic equations were dealt with by ruler and compass (straight lines and circles); for third and fourth degree equations the constructions were by the intersection of conic sections, preferably a parabola and a circle. For higher order equations it was necessary to use curves of sufficiently high degree. Accordingly, problems were divided into classes with respect to how they were constructed; those leading to linear and quadratic equations were called plane, those leading to third or fourth degree equations solid, and Descartes suggested continuing this classification by pairs of degrees for higher order problems. Thus the adoption of the degree as criterion of simplicity led to classifications both of curves and of problems. Descartes had canonised the choice of the constructing curves for equations up to the sixth degree. For higher degrees that choice remained a matter of discussion among his followers. Nevertheless it was generally believed that the determination of these constructing curves ws unproblematical, and that construction by the intersection of curves constituted the appropriate geometrical solution to zero-dimensional problems.

For one-dimensional problems construction meant the construction of a curve, given its equation. There were two procedures. The one was tracing by combinations of motions as explained above, sometimes illustrated by (imagined or actually existing) instruments. This method worked for a number of well-known curves, but there was no general method for deriving from a given equation a method by which the corresponding curve could be traced.

The other procedure was pointwise construction. Given the equation $F(x,y) = 0$ of the curve, the equation $F(x,y_o) = 0$ decribes the zero-dimensional problem of constructing the abscissa x of a point on the curve with given ordinate y_o. That problem can be constructed by the procedures of the "construction of equations". Thus, by fixing various values of y, arbitrarily many points on the curve can be constructed. Clearly the assumption that construction of equations (in one unknown) was possible implied the possibility to construct arbitrarily many points on the curve from its equation (in two unknowns). Within the Cartesian school of geometry such a pointwise construction of a curve was considered actually to constitute a construction of the curve itself. Thus in principle every algebraic curve admitted a pointwise construction.

With respect to what earlier had been considered as properly belonging to geometry, Descartes greatly extended the realm of geometrically acceptable objects and procedures. He introduced a new class of curves (the algebraic ones), as well as new means of construction (by intersection of higher degree algebraic curves) that were much more powerful than those used by earlier geometers. Descartes was keenly aware of the importance of this extension and he felt it necessary to explain and argue at length that the new curves and means of construction should be accepted as legitimately geometrical, whereas other curves (non-algebraic ones such as the quadratrix and the spiral) should be excluded from geometry as being "mechanical". Indeed, Descartes and his followers called algebraic curves "geometrical" and all the others "mechanical". Descartes' argument for legitimating algebraic curves was that the motions by which they are traced (as explained above) could be imagined as clearly as those by which the circle and the straight line were traced. Because the circle and the straight line were obviously geometrical, there was no reason to exclude the new curves. It is essential to note that this legitimating argument relied on the imagination rather than on practical feasibility or precision.

Thus, within the structure of Cartesian geometry algebra was much more than a mere analytical tool. Algebra marked the boundary of geometry and this

demarcation was supported by legitimating arguments. Moreover, algebra provided the criteria of adequacy for solutions of problems within Cartesian geometry; the degrees of the curves used in constructions should be the lowest possible; a problem solvable by the intersection of conics, for instance, should not be constructed by higher degree curves. The restriction to algebraic curves was essential also for constructibility itself; only for such curves did the equation (in two unknowns) provide a pointwise construction via the methods of the "construction of equations".

The demarcation of Cartesian geometry, excluding all non-algebraic relations, was essential for the structure of the theory, for its justification, its methodology and its techniques. All these aspects lost their meaning when mathematicians studied transcendental curves with Cartesian methods. For such curves simplicity was no longer defined; they admitted no equations from which pointwise constructions could be immediately derived; they did not fit into the classification of curves and their use in constructions would totally upset the classification of problems.

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Thus the new non-algebraic problems that became prominent in mathematics towards the end of the seventeenth century challenged the Cartesian paradigm; in fact they were the main cause of its overthrow. There were two types of such problems, namely "quadrature problems" and "inverse tangent problems". In the case of a quadrature problem it was required to determine the area bounded by a given curve, two axes and an arbitrary ordinate. Since the ordinate was arbitrary, quadrature problems were one-dimensional problems; their solution generally involved a curve representing the relation of the quadrature to the abscissa. The modern equivalent of a quadrature problem is the integration of a function. Inverse tangent problems were also one-dimensional; it was required to determine a curve whose tangents had a prescribed property. The modern equivalent is the solution of a first-order differential equation.

Quadrature problems had already been studied in classical Greek mathematics; inverse tangent problems began to attract the attention of mathematicians around the middle of the 17th century. It was particularly the study of motion in mechanics which often gave rise to such problems.

Mathematicians found that quadrature and inverse tangent problems very often involved non-algebraic solutions. In face of these problems, the Cartesian paradigm failed with respect to both analysis and construction. Cartesian analysis could not deal with non-algebraic relations, nor did it provide methods for dealing adequately with the infinitesimal concepts involved in quadratures and tangents. This failure was remedied by the new infinitesimal calculus developed by Newton and Leibniz and their respective followers. In a relatively short period the new infinitesimal methods developed into a well-articulated and recognizable body of concepts, methods and procedures.

Cartesian methods of construction could not deal with transcendental curves either. The story of how mathematicians remedied that defect is less clear-cut. In fact, ultimately (during the second half of the eighteenth century) mathematicians lost interest in the construction of non-algebraic curves, without having reached a consensus on its solution. But around 1700 the matter was keenly debated. Whenever a mathematician had to make it clear to himself or to others which (non-algebraic) curve he was dealing with, he had to give a proper representation of that curve. Representation by equation was impossible since equations for non-algebraic curves were only beginning to be used and were felt to be insufficient to characterise a curve. Hence the only way to represent the curve was by means of some construction. Cartesian geometry could not be used here and it was not at all clear what criteria of

acceptability and simplicity should be applied to these constructions. We shall see that tractional motion gave mathematicians an opportunity (there were also others) to rethink and clarify the questions concerning construction of non-algebraic curves.

Non-algebraic curves occurred primarily in connection with quadratures and integrals. When an inverse tangent problem was analysed by means of the infinitesimal calculus, the first result was a differential equation. Since the data of the problem usually did not themselves include transcendental relationships, the differential equation consisted of algebraic terms. The analyst's next step was to try and separate the variables by appropriate substitutions. If that was possible, one arrived, by integration, at an equation of the curve involving integrals of algebraic integrands. If the integrals were algebraic, the curve itself was algebraic and could be constructed by Cartesian means. If the integrals were not algebraic the construction of the curve was problematical. Even before the Newtonian and Leibnizian calculuses had become available, mathematicians had adopted the same approach in analysing inverse tangent problems; they tried to reduce these problems to quadratures. In the case of quadrature problems the analysis immediately led to integrals, after which the situation as regards construction was the same as in the case of inverse tangent problems.

The central issue therefore was how to construct non-algebraic quadratures. Confronted with that issue mathematicians could opt for several approaches. One was simply to assume the construction possible. This was called "construction by quadratures" or "reducing to quadratures". That procedure is well illustrated by Leibniz' construction of the tractrix, published in 1693.

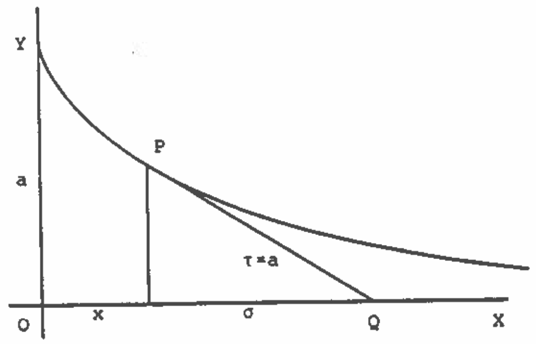

Figure 3.1

Analysing the problem (cf. Figure 3.1 and Formulas 4.1-2), Leibniz had derived the differential equation

$$(3.1) \qquad dx = -\frac{\sqrt{a^2-y^2}}{y}\, dy ,$$

which yielded, by integration [7]

$$(3.2) \qquad ax = -a \int \frac{\sqrt{a^2-y^2}}{y}\, dy$$

Leibniz then gave the following

Construction [8] (1) Draw a quadrant AOB (see Figure 3.2), prolong OB to both sides and draw AD parallel to OB. (2) For arbitrary U on the circle, draw OU, intersecting AD in V; draw the vertical through V and the horizontal through U; they intersect in Z. (3) By repeating (2) for other points U more points Z are found; they lie on a curve; that curve is thereby constructed pointwise. (4) For an arbitrary point Y on the axis draw XY horizontally to the right such that a.XY = area YAZ (here the quadrature of the curve AZZ is assumed to be possible); the point X thus found is on the tractrix. (5) Repeat (4) for other points Y to find more points on the tractrix. ⊠

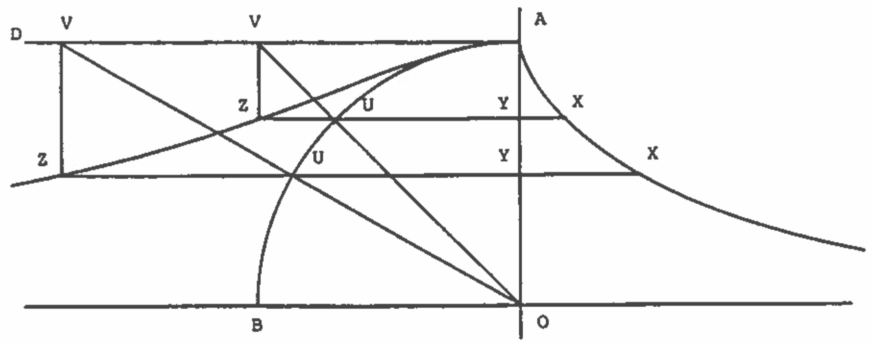

Figure 3.2

The procedure begins ((1)-(3)) with a pointwise construction of a curve whose equation, as is easily seen, is

$$(3.3) \qquad z = -a\frac{\sqrt{a^2-y^2}}{y} ,$$

Leibniz called it the "linea tangentium," because if we set $\measuredangle$ AOV = ϕ then $y = a\cos\phi$ and $z = a\tan\phi$. Apparently Leibniz preferred here the construction to the equation as the representation of that algebraic curve. From the analysis it is clear that this is the curve whose quadrature is required to construct points on the Tractrix. Then Leibniz proceeded with a pointwise construction of the Tractrix itself ((4)-(5)) on the basis of the assumption that rectangles could be found that were equal to areas under the first constructed curve.

Thus this construction by quadrature was a direct translation of the analytical expression (3.2) into the terminology of construction. The procedure begged the question of how the quadrature was actually performed. Such constructions were in general considered unsatisfactory; nevertheless they were often used, if only to represent in geometrical terminology the result gained by analysis.

The second way to approach the problem of constructing non-algebraic quadratures was to try and reduce the problem to quadratures of the simplest possible curves. In particular mathematicians tried to reduce a quadrature either to that of the circle (in modern terms to the sine or arcsine function) or to that of the rectangular hyperbola (the logarithmic or exponential functions). Such constructions by quadratures of circle and hyperbola were usually considered adequate as solution of the problem at hand. But there was always the awareness that these quadratures themselves were still problematical and that their geometrical status was still open.

The third approach to non-algebraic quadrature was construction by means of standard transcendental curves, which were considered to have been given or constructed previously. Certain transcendental curves were known to be dependent on the quadratures of the circle or the hyperbola. The most important of these was the logarithmica (modern: $y = b^x$). The curve had been known since the 1660's; it was usually defined as the curve with constant subtangent, or alternatively as the curve with the property that for each arithmetical sequence of abscissae the corresponding sequence of ordinates was geometrical. The latter property implied an obvious pointwise construction of the curve, given two points on it. The curve was known to be related to the hyperbola quadrature. The cycloid, on the other hand, was known to be related to the circle quadrature; it was usually defined by the familiar rolling-off process. Obviously, if one assumed a logarithmica or a cycloid as given, problems depending on the quadratures of the hyperbola or the circle could be constructed from these curves. This way of constructing quadratures by means of standard transcendental curves was considered adequate in as far as these curves could be considered as given. Thus the adequacy of these constructions depended on the geometrical legitimation of the transcendental curves in question. (Examples of such constructions will be discussed in Section 5.)

It follows from the above that the crucial question for those who wanted to extend the constructional possibilities beyond the restrictions imposed by the Cartesian paradigm was in how far certain transcendental curves could be considered as sufficiently known or given to be acceptable as means of geometrical construction. What criteria should curves satisfy in order to qualify as legitimate means of construction in addition to the standard Cartesian ones for geometrically solving quadrature and inverse tangent problems?

This was indeed the main question behind the interest in tractional motion around 1700: could curves traced by such motion be legitimized as means of constructing non-algebraic quadratures or other relationships? We will see how mathematicians argued in favour of this legitimation and against the Cartesian restriction to algebraic curves.

4. HUYGENS AND THE TRACTRIX

The tractrix is the tractional curve which arises if the base is a straight line and the chord length is constant (see Figure 3.1 and cf. Section 2). Taking the X-axis as base, chord length $r = a$, and the initial position with Q in the origin O and PQ along the Y-axis, one obtains the differential equation

$$(4.1) \qquad \frac{dy}{dx} = \frac{-y}{\sqrt{a^2-y^2}} .$$

Since the variables are separated we can integrate:

$$(4.2) \qquad x = -\int_a^y \frac{\sqrt{a^2-\eta^2}}{\eta} d\eta .$$

The integral depends on the logarithmic function. The equation of the curve can be written as

$$(4.3) \qquad x = a \log\frac{a+\sqrt{a^2-y^2}}{y} - \sqrt{a^2-y^2} .$$

or equivalently (because $\frac{a+\sqrt{a^2-y^2}}{y} = \frac{y}{a-\sqrt{a^2-y^2}}$) as:

$$(4.4) \qquad x = a \log\frac{y}{a-\sqrt{a^2-y^2}} - \sqrt{a^2-y^2} .$$

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In the autumn of 1692 Huygens embarked on a study of the tractrix and of the means by which that curve can actually be drawn. His manuscript notes [9] about this, dated 29 October - 20 November 1692, are extensive. He published parts of his findings in an article in the Histoire des Ouvrages des Sçavans of February 1693. [10]

Huygens analysed the curve along the lines of Formulas (4.1-2), although he did not formally use the differential calculus. He found (cf. Formula 4.2) that the relation between the coordinates x and y of points on the tractrix depended on the quadrature of the curve C that has the equation

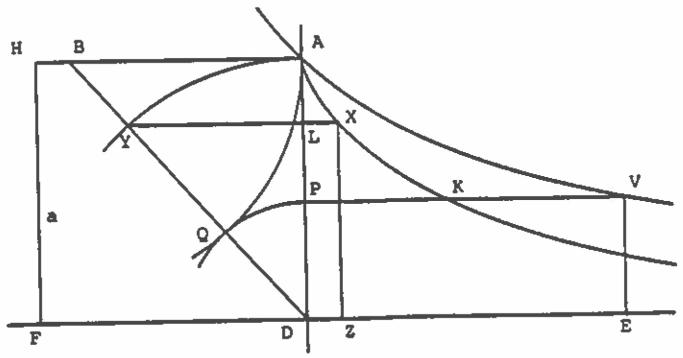
$$(4.5) \qquad z = a\frac{\sqrt{a^2-y^2}}{y} \qquad \text{or} \qquad a^4 = y^2z^2 + y^2a^2 .$$

Thus the analysis showed that the tractrix could be constructed "by

quadrature" of the curve C. Huygens recognized this curve; he had met it about two years earlier in his studies on the catenary. At that time (helped by hints in Leibniz' and Johann Bernoulli's publications) he had found that the quadrature of the curve depended on the quadrature of the hyperbola. Using these earlier results Huygens derived the relation between the tractrix and the hyperbola quadrature equivalent to Formula 4.4. The relation implied that if a tractrix was given, the quadrature of the hyperbola could be constructed. Huygens gave that construction in his article. It is an illustrative example of a construction of a quadrature by means of a given (non-algebraic) curve. It is as follows:

Construction [11]. FDAH (see Figure 4.1) is a square with side a; AV is a hyperbola through A with asymptotes FD and FH. AK is a given tractrix with asymptote FD and initial point A. Let V be any point on the hyperbola and EV the corresponding ordinate. It is required to find the area ADEV. (1) Draw VP parallel to the axis and take B on AH such that BD = BA + PD (that is BQ = BA and DQ = DP). (2) Take Y on BD such that DY = DA = a. (3) Draw a horizontal straight line through Y intersecting AD in L and the tractrix in X. (4) Then Area ADEV = a.YX. ∎

Figure 4.1



In the article Huygens did not provide a proof of the construction. His analysis in the manuscript is too complicated to be given here. That the result is correct can be seen from the following: Call

(4.6) $LX = x,\ LD = y,\ DE = u,\ EV = DP = v,\ BA = w,\ YL = z.$

The point X is on the tractrix, so (cf. Formula 4.3)

$$(4.7)\qquad x = a \log \frac{a+\sqrt{a^2-y^2}}{y} - \sqrt{a^2-y^2}\ .$$

V is on the hyperbola, so

$$(4.8)\qquad (a+u)v = a^2\ .$$

By the construction

$$(4.9)\qquad z = \sqrt{a^2-y^2}$$

and

$$(4.10)\qquad w = \frac{a^2-v^2}{2v}\ .$$

Now

$$(4.11)\qquad \text{area } ADEV = a^2 \log \frac{a+u}{a} = a^2 \log \frac{a}{v}\ .$$

On the other hand

$$(4.12)\qquad a.YX = a(z+x) = a^2 \log \frac{a+\sqrt{a^2-y^2}}{y} = a^2 \log \frac{a+z}{y}\ .$$

Thus to prove that area $ADEV = a^2 \log \frac{a}{v}$, we have to show that $\frac{a+z}{y} = \frac{a}{v}$. And indeed

$$(4.13)\qquad \frac{a+z}{y} = \frac{a}{y} + \frac{z}{y} = \frac{DB}{DA} + \frac{AB}{DA} = \frac{v+w}{a} + \frac{w}{a} = \frac{v+2w}{a} = \frac{v^2+a^2-v^2}{av} = \frac{a}{v}\ .$$

We see that Huygens adjusted the relations of the segments within the square FDAH so as to fit the required equality.

Huygens noted that conversely the relation between the tractrix and the hyperbola quadrature can also be used to construct the tractrix itself by quadrature of the hyperbola:

> From which it is seen, conversely, how one can find the points of that curve, supposing the quadrature of the hyperbola. [12]

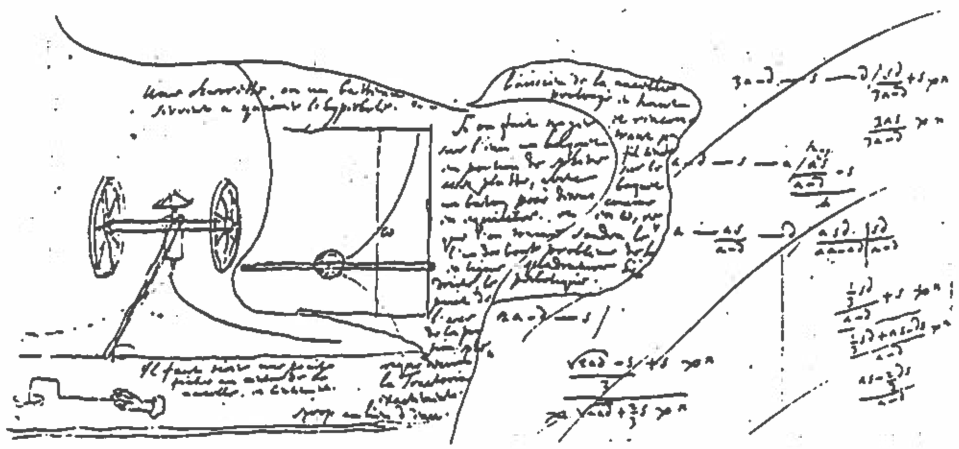
However, his primary interest was to use the tractrix as a means of construction for determining non-algebraic quadratures such as that of the hyperbola.

Before using the curve in that way, Huygens had to convince himself that the tractrix was a legitimate means of geometrical construction. Huygens based his arguments for the legitimation on the criterion of practical feasibility. He tried to show that it was possible in practice to draw a tractrix with a precision comparable to the precision obtained with the ruler and the compass. If that could be done, he argued, geometers should accept the tractrix on an equal footing with the circle and the straight line. He formulated this thought at several places in his manuscript, for instance:

> 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? What more is there to say of my construction than of the construction of a mean proportional line between two given straight lines? [a standard construction by ruler and compass, HB] 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. [13]

For Huygens this line of argument carried great conviction, as is apparent from the efforts he made to solve the problem of actually tracing curves by tractional motion.

Huygens considered various designs of instruments for tracing the tractrix. In his drawings [14] (see Figure 4.2) we see the heavy dragged object resting on a vertical tracing pin, or, vertically under the pin, connected via a U-shaped beam around the table. The weight and the tracing pin are dragged by means of a beam (not a chord) the other end of which is moved along the rim of the table. Huygens also considered variants of the process, like for instance a small two-wheeled cart, which traces the curve by means of a pin through the middle of its axis, or like a spherical shell, dragged while floating on a fluid (ensuring horizontality of the plane). As far as can be concluded from the manuscripts, Huygens actually made at least one of the projected devices and drew the curve with it [15]. He was very concerned about the mechanical precision of the process; he discussed possible ways of making the surface exactly horizontal (even working out a method to gauge the water-level [16] - then a recent invention) so that no sideways component of gravity would interfere with the dragging process. His concern for the horizontality of the surface went so far that he explicitly discussed a possible objection, namely that, because the earth is spherical, there is only one point on a flat surface where the action of gravity is precisely perpendicular [17]. This objection does not apply to the surface of a fluid, hence Huygens' interest in dragging floating bodies.



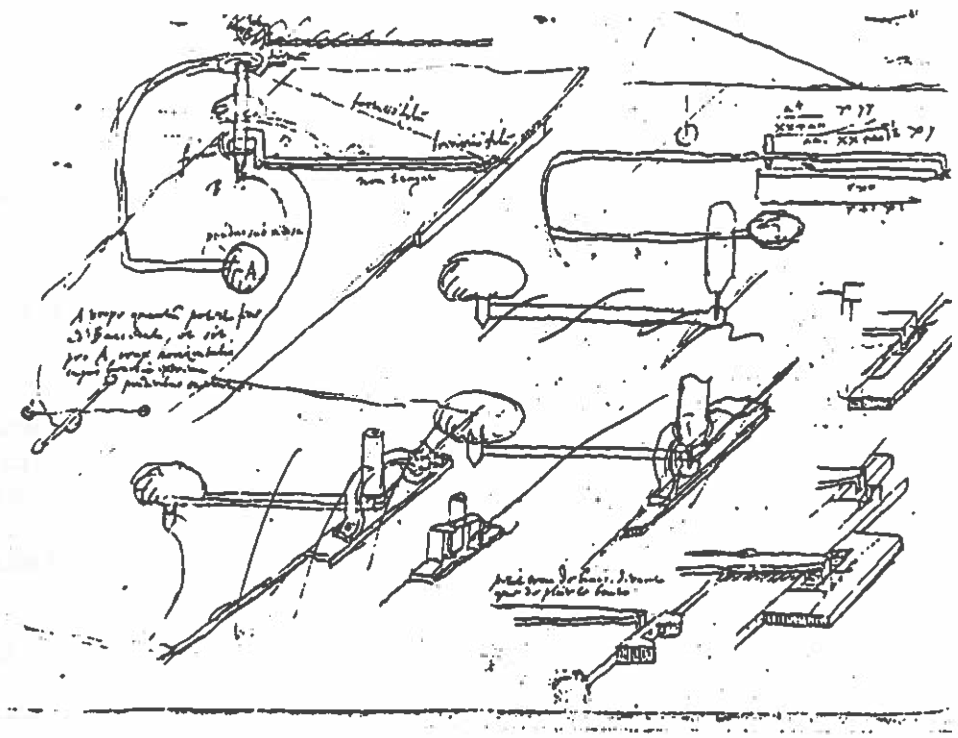


Figure 4.2

While sketching the designs, Huygens repeatedly recalled the aim of his endeavour. For instance beside his first sketches of the cart and the floating body that is dragged over the surface of a fluid, Huygens noted that the latter arrangement "will solve the problem of the hyperbolic quadrature" [18], and:

> A little cart or a boat will serve to square the hyperbola. If a boat in the shape of a rather flat spherical shell is made to float on water, with a stick placed in it in equilibrium, and if one drags one of the ends in a straight line, the point of the axis of the spherical shell will exactly describe the Tractoria - syrup instead of water - The dragging should be at a fixed point in the centre of the shell, and slow. [19]

It should be emphasised that it was the precision of tracing by motion that Huygens sought, not the precision of pointwise construction. In fact the relation implicit in the construction enabled him to calculate coordinates of points on the curve by means of logarithms; he actually made that calculation (equivalent to Formulas (4.3-4) in his manuscripts [20]. This numerical approach would enable him to construct the curve pointwise with much greater precision than any tracing process could achieve. But, significantly, he presented these calculations not as a means to draw the curve but as a means to check whether tracing by tractional motion was sufficiently precise.

Huygens was well aware that by introducing the tractrix as means of construction he was challenging the Cartesian view of geometry. He noted:

> 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; for he was well aware that the properties and uses of such curves could also be investigated by geometrical methods. [21]

In the published article [1693a] Huygens did not describe his instrument, but he did stress that the curves could be traced easily and accurately by an instrument. After giving the construction discussed above, as well as a number of properties of the curve, he made it clear once more that the main motivation for his study was the legitimation of the tractrix, which was required to justify its use in the proper geometrical solution of problems dependent on the quadrature of the hyperbola:

> But it is not because of all that I have mentioned so far about this line that I propose it here, but for another reason, which is that a means can be found to describe it by a rather simple machine, and thereby to reduce the hyperbola to a square, which seems worthy of the consideration of geometers. (-) 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, such as among others the determination of points on the <u>Catenaria</u> or Catenary, and the logarithms. [22]

Huygens' investigations may well strike the modern reader as a curious and ineffective combination of foundational study in geometry and a down-to-earth practical (indeed do-it-yourself) approach to the mechanics of motion. But Huygens took his subject very seriously; for him, evidently, these studies

on the instrumental foundation of geometry were meaningful and important. And, although he stressed the instrumental implementation more strongly than others did, Huygens was not the only one to have this interest. To show this I shall deal in the following sections with the various responses evoked by his article on tractional motion. These responses, as well as Huygens' own studies, reveal a characteristic view of the nature and aim of the study of curves, which was shared by most mathematicians at the time. That view was influential in the research on curves at the time, but it was later forgotten and therefore it is now very unfamiliar.

5. BERNOULLI'S PROBLEM

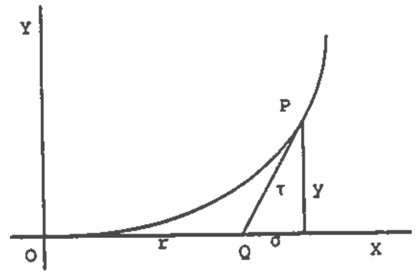
Huygens' article on the tractrix inspired Jakob and Johann Bernoulli to formulate a problem that became known as "Bernoulli's problem". Bernoulli's problem may well have been the most complicated inverse tangent problem that was studied in the period around 1700. It will be useful first of all to collect the most important mathematical facts about the problem and its solution.

The problem is to find the tractional curve with straight base and chord length varying proportionally to the segment along the base from the origin to the end of the chord. Thus taking the X-axis as base (and considering the tractional motion to be performed in the upper half plane), we have (see Figure 5.1)

(5.1) $\tau = pr$.

where τ = PQ and r = OQ, and p : 1 is a given ratio.

Figure 5.1



Using elementary geometry one derives the differential equation of the curve:

(5.2) $$y\sqrt{1+(\frac{dx}{dy})^2} = p(x - y\frac{dx}{dy})$$

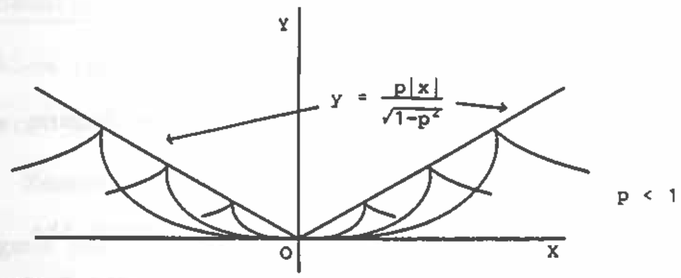
or

(5.3) $$y\sqrt{dx^2+dy^2} = p(xdy - ydx)$$

The differential equation is homogeneous and non linear. The trajectories are similar with respect to the origin O. When p > 1, two trajectories pass through each point in the upper half plane. When p < 1, the trajectories lie between the X-axis and the half-lines

(5.4) $$y = \frac{p}{\sqrt{1-p^2}}|x| .$$

Every point in that region lies on two trajectories; the trajectories have cusps on the half-lines. In the case where p = 1 circles through O with their centres on the Y axis satisfy the conditions of the problem, and so do their horizontal top tangents (as degenerated cases). Figure 5.2 shows some examples of the trajectories in the various cases.



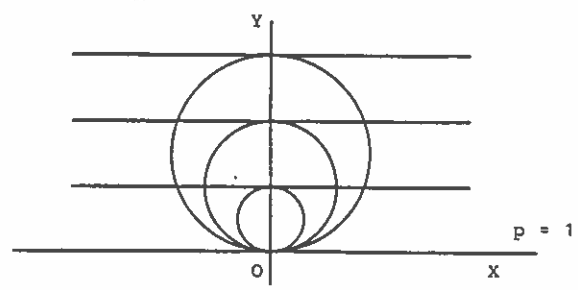
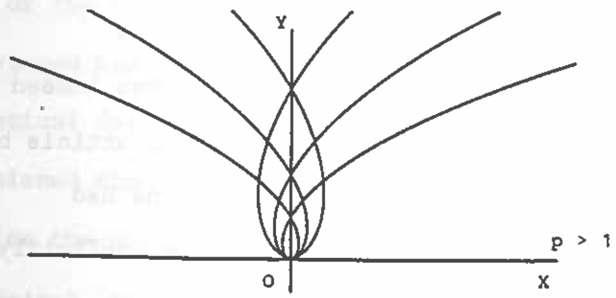


Figure 5.2



A convenient way to solve the differential equation is to take $\chi = \measuredangle PQX$ as parameter. Then $\frac{dy}{dx} = \tan\chi$, $x = y\frac{p\cos\chi+1}{p\sin\chi}$, and by calculating $\frac{dx}{d\chi}$ in two ways one derives

(5.5) $\frac{dy}{y} = \frac{p+\cos\chi}{\sin\chi} d\chi$.

This differential equation can be solved:

(5.6) $y = \gamma \sin\chi (\tan\frac{\chi}{2})^p$,

whence

(5.7) $x = \gamma(\cos\chi + \frac{1}{p})(\tan\frac{\chi}{2})^p$.

Or, in terms of $t = \tan\frac{\chi}{2}$:

(5.8) $y = \gamma \frac{2t}{1+t^2} t^p$, $x = \gamma \left(\frac{1-t^2}{1+t^2} + \frac{1}{p}\right) t^p$.

We see that the resulting curves are algebraic for rational p and transcendental for irrational p. The other variables, expressed in t, are:

(5.9) $\tau = \gamma t^p$ and $\sigma = \gamma\, t^p \frac{t^2-1}{t^2+1}$.

***

"Bernoulli's problem" was proposed at the end of an article by Johann Bernoulli in the May 1693 issue of the Acta Eruditorum [23]. Mathematicians were invited to determine the curve (or curves) through the origin O with the property (see Figure 5.1) that at each of its points P the length of the tangent PQ (with Q on the X-axis) had a fixed ratio p to the segment OQ along the axis.

Bernoulli remarked that such curves could be traced easily by a continuous motion. He had tractional motion in mind; the problem was indeed inspired by Huygens' article on the Tractrix. This appears from an article by Jakob Bernoulli in the June issue of the Acta. Pointing out that he had discussed the problem with his brother before it was proposed, he explicitly acknowledged indebtedness to Huygens:

> This is an elegant problem; that we [sc. he and his brother, HB] hit upon it was due to the publication recently of certain Huygensian results in the Rotterdam Acta. [24]

He explained a tractional device that could indeed produce the required curve (I shall discuss the device below).

The problem attracted a gret deal of interest within the group of early practitioners of Leibniz' calculus and led to a series of articles in the Acta Eruditorum. In June 1693, a month after his brother had proposed the problem, Jakob Bernoulli published a construction of the curve (but not the analysis by which he had found the construction) [25]. The July issue contained a short article by Leibniz with comments on the problem and a suggested generalisation [26]. In September l'Hôpital's solution of the problem was published (again without the analysis) [27]. Comments by Huygens [28] and Leibniz [29] appeared in the October issue. In May 1694 the Acta carried another solution by l'Hôpital [30], translated into Latin from a French version that had appeared in December 1693 in the Mémoires Mathématiques [31]. Finally in September 1694 Huygens' solution of the problem (again without analysis) was printed in the Acta [32], with an excerpt from his letter to Leibniz [33] and a further comment by Leibniz [34].

Meanwhile the problem and the curves were also discussed by letter. Huygens and l'Hôpital exchanged their analyses and constructions and commented on the various publications [35]. In earlier letters to Huygens, l'Hôpital had clarified several points about the new calculus, and the Bernoulli problem was one of the first to which Huygens successfully applied the calculus. It convinced him of the power of the new method, about which he had been sceptical for a long time. Still Huygens' style was decidedly more old-fashioned than that of the others. Huygens seems to have been the first to notice the occurrence of the cusp (in the case $p < 1$); he pointed this out to l'Hôpital. He also wrote about various ways in which the curves could be traced by instruments. Johann Bernoulli and l'Hôpital discussed the problem in their letters [36]; l'Hôpital sent a solution and asked Bernoulli to translate it and send it on to the Acta; Bernoulli criticised the solution but finally did as he was asked [37]. l'Hôpital informed him about the cusp; that topic was then discussed at length in later letters. For some time the Bernoulli problem featured in the correspondence between Leibniz and Huygens [38]; they discussed it particularly in connection with the geometrical status of transcendental curves and the foundational aspects of tracing curves by motion.

As can be seen from the above, those who published a solution of the problem gave a construction of the curve or curves, but they left it to their readers to check whether these curves did indeed satisfy the conditions and to wonder how they were found. The first analysis of the problem which was actually published was apparently that of Jakob Bernoulli; it occurs in his Opera of 1744 [39].

It will be instructive to give examples of the analyses and constructions, The most suitable examples are Jakob Bernoulli's analysis of the problem and Huygens' construction. The others are either too lacunary or too involved to be usefully presented here.

Jakob Bernoulli analysed the problem as follows. He started by considering (see Figure 5.1) the variables τ = PQ and σ; so OQ = r = τ/p, $y = \sqrt{\tau^2-\sigma^2}$ and $x = \sigma + \tau/p$. He then expressed dy and dx in τ, σ, $d\tau$ and $d\sigma$, and evaluated the quotient

(5.10) $$\frac{dy}{dx} = \frac{y}{\sigma} ;$$

this yielded the differential equation

(5.11) $$\sigma d\tau = \tau d\sigma + \frac{\tau d\tau}{p} - \frac{\sigma^2}{p\tau}d\tau .$$

The substitution

(5.12) $$z = \frac{\sigma}{\tau}$$

led to

(5.13) $$\frac{d\tau}{p\tau} = \frac{dz}{z^2-1} .$$

In this differential equation the variables are separated. Bernoulli wrote the

right-hand side as a sum of partial fractions and integrated directly:

(5.14) $$t^{1/p} = \sqrt{\frac{1+z}{1-z}}$$

(He did not write down the intermediate step $\frac{\log\tau}{p} = \frac{1}{2}\log\frac{z+1}{z-1}$, nor did he consider a constant of integration, as a result he obtained only one curve.) It followed that

(5.15) $$\tau^{2/p} = \frac{1+z}{1-z} , \quad \text{so} \quad z = \frac{\tau^{2/p}-1}{\tau^{2/p}+1}$$

whence, as $z = \frac{\sigma}{\tau}$,

(5.16) $$\sigma = \tau\frac{\tau^{2/p}-1}{\tau^{2/p}+1} .$$

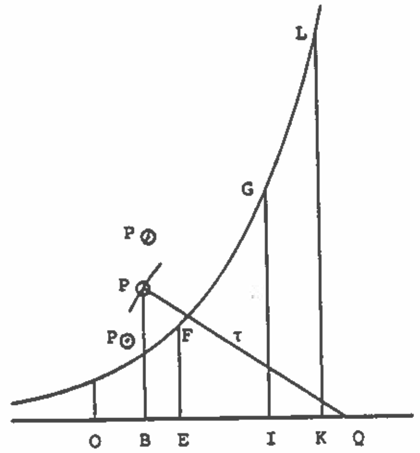
Thereby Bernoulli had expressions in τ for σ and r (r = τ/p). From σ, r and τ it was easy to construct the corresponding point P on the curve. To arrive at a (pointwise) construction of the curve, the only step left was to translate Formula (5.16) into a construction of σ from τ. In his published construction Jakob Bernoulli practically left that problem to his readers; he himself probably used the logarithmica.

Huygens' construction provides a better illustration of constructional practice than does Jakob Bernoulli's. It is a pointwise construction assuming a logarithmica as given:

Construction [40] (see Figure 5.3). (1) Take OE on the axis and EF perpendicular such that EF : OE = p : 1. (2) Draw an arbitrary logarithmica through F with OE as axis. (3) Take any point Q on the axis. (4) Determine length τ such that τ : OQ = p : 1; draw a circle with radius τ and centre Q. (5) Determine the ordinate IG of the logarithmica such that IG = τ. (6) Determine K on the axis such that IE : EK = p : 2; draw the ordinate KL of the logarithmica. (7) Determine B on the axis such that (KL+EF) : (KL-EF) = τ : DB. (8) Draw a perpendicular in B, it intersects the circle in P; P is on the required curve. (9) Repeat the construction from (3) onwards to find more points on the curve. (10) Repeat the construction from

(1) onwards, taking diffent lengths OE in order to find other curves satisfying the property. ∎

Figure 5.3



In the Acta Eruditorum article Huygens gave neither analysis nor proof for his construction. Thus his readers did not get more than the set of instructions summarised above. This way of presenting results or solutions was very common then; Huygens' construction is a characteristic example of the representation of a new curve occurring as solution of an inverse tangent problem. Huygens' analysis of the problem is too complicated to be given here. It is, however, instructive to check the construction in order to see how the relationships implicit in the problem occur in the construction. I follow the numbering of the steps in the construction:

(1) Call EF = γ, hence OE = γ/p. (2) With coordinates u (taken along the axis from E) and v, the logarithmica has the equation $v = \gamma e^{\lambda u}$ for some λ. (3) Call OQ = r. (4) Hence τ = pr. (5) Call EI = u, then we have τ = IG = $\gamma e^{\lambda u}$.

(6) Call EK = u', then u : u' = p : 2, so u' = 2u/p, hence KL = $\gamma e^{\lambda u'} = \gamma e^{2\lambda u/p}$.

(7) So

$$(5.17) \qquad QB = \tau \frac{KL-EF}{KL+EF} = \gamma\, e^{\lambda u} \frac{\gamma e^{2\lambda u/p}-1}{\gamma e^{2\lambda u/p}+1}$$

We can now compare the construction with the Formulas (5.6-9); we see that $\gamma e^{\lambda u} = \tau = \gamma t^p$, hence $e^{\lambda u} = t^p$, so that

$$(5.18) \qquad QB = \gamma\, e^{\lambda u} \frac{\gamma e^{2\lambda u/p}-1}{\gamma e^{2\lambda u/p}+1} = \gamma\, t^p \frac{t^2-1}{t^2+1},$$

which indeed corresponds to the value of σ in (5.9).

***

I now turn to the methodological discussions to which the Bernoulli problem gave rise. From the beginning, the problem was considered of interest primarily because in this case the tractional motion produced both algebraic and non-algebraic curves. Thus the resulting curves were very diverse, whereas the motions that traced them were the same; the only thing that differed was the ratio p : 1. This was a most striking phenomenon because it upset the basic distinctions of Cartesian geometry. For Descartes the motions producing non-algebraic curves were fundamentally different and indeed inferior to those producing "geometrical" (i.e. algebraical) curves. Moreover, algebraic curves of higher degree were seen as generated by more complex motions than those of lower degree. Tractional motion plainly belied these distinctions. Without changing the simplicity of the motion in any way, it produced circles (in the case p = 1), algebraic curves of any degree and non-algebraic curves. When proposing the problem Johann Bernoulli already stressed this aspect of the problem, writing as follows:

> For whatever the ratio of M to N [i.e. the ratio p : 1, HB] might be, the curve ABC can always be described with equal ease by some continuous motion, despite the fact that the curve would turn out to be more or less complex depending on the ratio of M to N; indeed in the case of equality ratio, it is immediately evident that the curve ABC is a circle; in the other cases the curve will still be geometrical if M is to N as number to number; but otherwise it is transcendental. [41]

Huygens was struck by the phenomenon too, he called it "mirabile" [42]. Jakob Bernoulli articulated a further consequence: The curves would be of utmost use ("eximium usum") in the "construction of equations", solving with equal ease problems that led to equations of greatly different degree (and thus breaking down the Cartesian classification of problems). He wrote:

> Whence it is clear that if such constructions are to be deemed geometrical and accurate, then an infinity of equations of whatever high degrees can be constructed in the same way as the simplest ones, with an ease exceeding almost all belief. [43]

Huygens reacted to this statement in a letter to Leibniz [44]. He wondered if Bernoulli, because he wrote "if such constructions are to be deemed geometrical and accurate", doubted the geometrical acceptability of tractional motion. He asked Leibniz' opinion on the matter. Leibniz answered [45] that algebraic curves should indeed be seen as a subclass of all curves traced by continuous motions, algebraicity occurring more or less as an exceptional case, in the same way as rationality occurs as an exceptional case among algebraic relationships. Somewhat later Leibniz expressed these ideas more precisely in an Acta Eruditorum publication. Commenting on a passage in an article by Huygens [46] in which the term "geometrical" was used in its Cartesian sense, he wrote that one of the best features of his infinitesimal calculus was

> ...that in that way one gets general solutions, which by their nature extend to transcendental quantities, but which in certain cases, as may happen, lead to ordinary quantities. I could be surprised that apparently he [sc. Huygens, HB] calls geometrical only those which are covered by equations of a certain degree, but I think that, when he writes about those "which are called geometrical", he follows rather than approves common usage. My opinion is that certain classical writers are rightly reproached for denying anything to be sufficiently geometrical that could not be performed by compass and ruler, and that therefore one should not at present condone the mistake of those who restrict geometry to the graded equations of algebra alone, whereas rather anything should be geometrical that can be exactly constructed by continuous motion. If he does not admit this, he does injustice to his own brilliant inventions, as he himself has been foremost in extending geometrical constructions, for the invention of evolutes, which we owe to Huygens, is invaluable, and now he has for the first time publicly presented tractional constructions. [47]

Thus for Leibniz curves in geometry were traced by motions; the only criteria for these motions were continuity and exactness; he did not mention

restrictions of instrumental or practical feasibility and simplicity. We shall see in the next section how Huygens (and indirectly l'Hôpital) reacted to this extreme position.

***

As the Bernoulli curves were considered important in connection with their use in constructions, mathematicians naturally thought of how they could actually be traced. Jakob Bernoulli sketched a device for that purpose in the article in which he gave the solution to the problem:

Construction [48]. (1) Draw a right-angled triangle EOF with OF along the X-axis (see Figure 5.4) and OE : OF = p : 1. (2) Take a system SQT of two perpendicular rulers and a chord GQP of length OE. At the end P of the chord a weight is placed, which, while being drawn by the chord over the horizontal plane, is subject to friction. (3) Move the ruler system to the left while keeping the end G on the line EF. During this motion the weighted end P of the chord is dragged over the surface, always moving in the direction PQ. (5) So, at any position, we have PQ : OQ = ET : TG = EO : OF = p : q, as required. (Bernoulli did not give technical details nor did he discuss the possible initial positions of the rulers and the chord.) ⊠

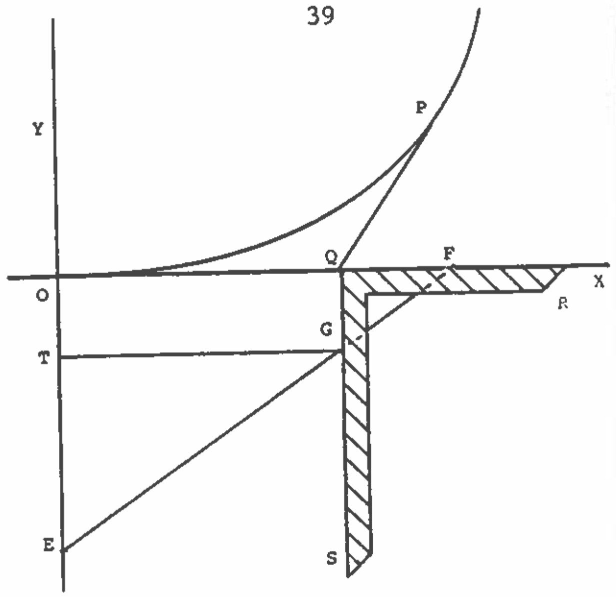


Figure 5.4

Huygens studied the problem of the actual tracing of Bernoulli's curve in more detail, much in the same style as his studies on the tractional instrument discussed in Section 4. In a letter to l'Hôpital he explained the machines he had devised. As an example, I discuss his suggestion for the case where $p > 1$.

Construction [49]. Let (see Figure 5.5) OB be the axis and O the origin of the required curve. Near B there is a system of two connected wheels E and F on one vertical axis; the ratio of the radii of the wheels is $r_F : r_E = (p-1) : 1$. There are two chords: QBE wrapped around the one wheel, and PQBF around the other. At the end P of chord PQBF a weight is attached which performs the tractional motion. Q is a ring to which the end of chord QBE is fixed while chord PQBF can pass freely through it; at B there is a ring through which both chords pass freely. Now ring Q is moved to the left along OB. By its motion the wheel system is turned, whereby the other chord is wound up around its wheel, so that its other end-point P is drawn over the horizontal surface, always moving in the direction of Q until it reaches the axis at O. During this motion P will trace Bernoulli's curve. [50] ⊠

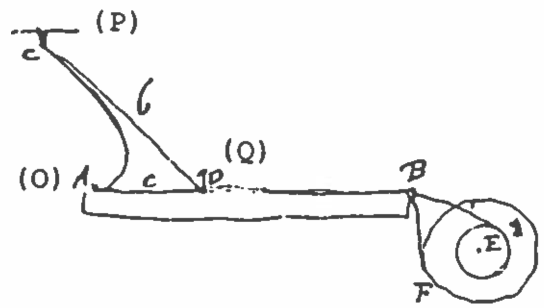


Figure 5.5

Huygens noted that if $p = 2$, one wheel suffices because in that case $(p-1) : 1 = 1$, i.e. $r_F = r_E$. He also sketched instrumental implementations of the motion in the case where $p < 1$.

6. LEIBNIZ' "SUPPLEMENT TO MENSURAL GEOMETRY"

Meanwhile Leibniz had expounded his ideas on the use of motion in geometry and the legitimation of non-algebraic curves in an Acta Eruditorum article [51] that appeared in September 1693. Its long title announced a "supplement to mensural geometry"; that supplement was construction by tractional motion. Again it had been Huygens' article on the tractrix that had prompted Leibniz to publish his own ideas on tractional motion.

The article opened with a long general argument on the justification of the use of physical processes such as tractional motion in geometry. Leibniz introduced a distinction between what he called "geometria determinatrix" (determinatory geometry) and "geometria dimensoria" (mensural geometry). The former aimed at the solution of what I have called zero-dimensional problems as practised within Cartesian geometry. Leibniz explained that determinatory geometry concerned the determination of points in the plane satisfying certain properties with respect to other points that were given. The required points were found, that is, constructed, by the intersection of straight lines, circles and other curves. The proper means to analyse these problems was algebra. The quantities involved in this algebraic analysis were in general irrational. Irrationality occurred because of what Leibniz called the "ambiguity" of the problems; they often admitted more than one solution. In such cases the analysis of the problem led to an equation of higher degree, and in general the roots of such equations involved radicals or other irrationalities; only in exceptional cases would the quantities turn out to be rational.

Mensural geometry, in contrast, dealt with what I have called one-dimensional problems, and transcended the Cartesian restriction to algebraic relationships. This part of geometry, according to Leibniz, concerned the measurement of arc lengths, areas, volumes and the like. Such problems were mostly reduced to quadratures of curves. The quantities necessary for the analysis of these problems were in general transcendental, although algebraic relations occurred as special cases [52]. Algebra, therefore, was insufficient for the analysis of these problems; the proper analysis for transcendental geometry was the "scientia infiniti", by which Leibniz meant his infinitesimal calculus. Transcendental quantities occurred because the problems of mensural geometry involved the infinite, namely the infinities of infinitesimal parts that make up the curves.

Leibniz then discussed how this distinction between geometrical fields related to the physical means on which geometry relied to generate objects and solve problems. In determinatory geometry these means were: ruler, compass and various other instruments or processes for tracing the curves by whose intersection the problems are solved. In transcendental geometry various new means had been adopted, such as for instance the evolution (rolling off) of curves, by which the cycloid was defined, and by which also classical curves such as the spiral and the quadratrix of Dinostratus could be generated. Leibniz recalled that he had earlier suggested similar processes, for example motion in a resisting medium, or suspension of a flexible chain; both of these would provide the quadrature of the hyperbola and logarithms. Another possibility was the use of light rays as a means of geometrical construction; such optical solutions could also be used in determinatory geometry.

Clearly Leibniz here contradicted the basic conceptions of Cartesian geometry. To a Cartesian geometer these motions (to say nothing of the use of light rays) were "mechanical" and thereby not geometrical. Descartes himself had explicitly excluded the process of rolling off, arguing that the ratio between curved and straight could never be known exactly. Leibniz did not

accept this. According to him any physical process is acceptable in geometry as long as it is exact:

> For if a method of construction is exact, then it is accepted in the theory of geometry; if it is easy and useful, it may be accepted in practice. For if motion is performed according to hypotheses that are certain, then it is also subject to geometrical study, as for example the centre of gravity. [53]

Leibniz took exactness in a very general sense, allowing any process that could be imagined to proceed according to determined mechanical laws. If the process was also easily performed, it was useful for practice, but the ultimate criterion for geometrical acceptability was not practical ease but exactness in the abstract.

After this theoretical preparation Leibniz provided the required legitimation of tractional motion in geometry. Tractional motion was exact because it was governed by clear mechanical laws. Moreover, tractional motion was eminently suited for transcendental geometry; it related directly to the tangents of the curve, because the direction of these tangents is given everywhere by the position of the tractional chord. Tangents were the directions of the infinitesimal straight parts that make up a curve. Hence tractional motion was intimately related to what characterised transcendental geometry, namely infinitude:

> This is the reason why such motion is wonderfully suited for transcendental geometry, because it is immediately related to the tangents or the directions of the line, and therefore to the elementary quantities, infinite in number but inassignable or infinitely small as regards magnitude. [54]

Thus Leibniz rejected the Cartesian demarcation of geometry and, by adopting a very abstract interpretation of the criterion of exactness, came to a very broad conception of the field. In a later article he wrote that geometry concerned "anything that can be exactly constructed by continuous motion" [55]. Exactness meant, as quoted above, that the motions were performed according to "certain hypotheses", clear rules or laws. Actual physical and practical feasibility was not a requirement; as long as the rules governing the motions could be clearly formulated, the curves that arose were geometrical. The instruments, the physical implementation of the motions, were little more than the means to imagine the rules that govern the motions. Thus Leibniz' position came near to circularity; in effect he said that geometrically acceptable motion was motion that can be defined mathematically. As we will see, his contemporaries reacted with some caution and reserve to this extreme position. Their reaction was based less on Leibniz' theoretical arguments summarised above than on his proposal for using tractional motion to solve the general quadrature problem. In its technical impracticability this proposal revealed even more strongly Leibniz' very abstract conception of the motions that may be used to produce geometrical curves and solve geometrical problems.

***

After the theoretical arguments on the legitimation of tractional motion Leibniz gave his own solution of the tractrix problem (I discussed it in Section 3 as an example of construction by quadrature). He took the opportunity to mention Perrault's question and to claim that he had been the first to solve the problem and realize the advantages of tractional motion for solving inverse tangent problems - a remark that rather irritated Huygens [56]. He then described an instrument for solving all quadrature problems by means of tractional motion. It is based on a general relation between quadratures and tractional motion. I shall not follow Leibniz' rather involved explanation of that relation but shall summarise it in modern terms.

Consider tractional motion with straight base (along the X-axis) and variable chord length τ. As in Formula (4.1), the differential equation of the tractional curve is

$$(6.1)\qquad \frac{dy}{dx} = \frac{-y}{\sqrt{\tau^2-y^2}}$$

or

$$(6.2)\qquad dx = -\frac{\sqrt{\tau^2-y^2}}{y}\,dy\ .$$

Hence if τ is a function of y we can integrate

$$(6.3)\qquad ax = -a\int\frac{\sqrt{\tau^2-y^2}}{y}\,dy\ .$$

The resulting tractional curve depends on the quadrature of the curve

$$z = a\,\frac{\sqrt{\tau^2-y^2}}{y}\ .$$

Conversely, the quadrature of an algebraic curve $z = z(y)$ will depend on a tractional curve with

$$(6.4)\qquad \tau = \frac{y}{a}\sqrt{z^2+1}\ .$$

This relation between τ and y is also algebraic. Therefore the general quadrature problem can be considered solved if it is possible to implement tractional motion with a straight base along the X-axis and chord length τ depending on y according to any given algebraic relation. This is precisely what Leibniz' instrument is meant to do. It consists of (see Figure 6.1, Leibniz' original figure from the Acta) a horizontal plane ATRH and a vertical plane MTRL intersecting along TR. The vertical plane is placed under the horizontal one; the planes can move with respect to each other. In the vertical plane a curve EEE is marked representing, with respect to the axes RT and RL, the relation between y and $a-\tau$, with $\tau=\tau(y)$ as in Formula (6.1), and $z=z(y)$ the curve whose quadrature has to be found. Thus if RT=y, then TE=$a-\tau(y)$. AT and AH are rectangular axes in the horizontal plane. TG is a vertical cylinder in which the pointed weight F can move up and down. F is fixed to a chord FTC of constant length a. At the end C a pointed weight is attached which, if dragged by the chord, traces a curve on the horizontal plane. The cylinder is moved along AT; thereby the horizontal part TC of the chord drags the weight C over the horizontal plane. During that motion C carries with it the line HR which (by a device indicated by the rectangle at H) remains perpendicular to AH. The cylinder TG is connected to the vertical plane in such a way that that plane always passes through the axis of the cylinder. Moreover, at R there is a device (indicated by a rectangle) ensuring that throughout the motion the line RL in the vertical plane intersects the line HR and that TRH remains a right angle. Further it is ensured that during the motion the point F of the weight follows the curve EEE marked on the vertical plane. When the top T of the cylinder is now moved along AB, the weight C is dragged over the horizontal surface, causing the vertical plane to follow the motion and ensuring that FT = a - τ(RT) = a - τ(y). Hence TC = a - FT = τ(y), so C performs the required tractional motion.

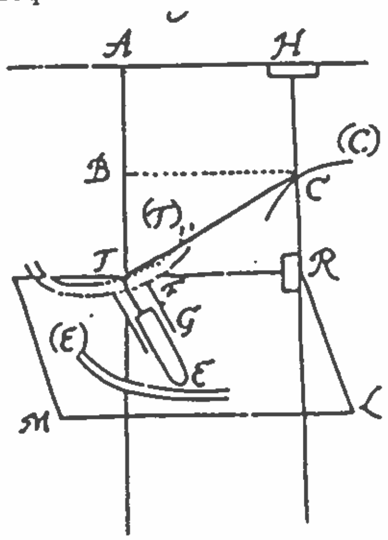


Figure 6.1

It should be stressed that Leibniz gave no explanation at all of how the various linkages and connections between the moving parts should be implemented so as to ensure the motion was performed in the way he described; he merely assumed that this could be done. For instance when dealing with the line HR moving with C, he wrote:

> This propulsion does not prevent the advancing point C from being moved solely by the traction of the chord, and therefore from keeping the direction of that chord during its motion. [57]

The marking of the curve EE was assumed to be possible too since this relation was given:

> whose nature and description on the tablet LRM is therefore provided by ordinary geometry. [58]

At the end of the article Leibniz suggested that tractional motion with curved base could be used for solving inverse tangent problems in general. But he did not elaborate this idea in any detail. He also referred to the case where the length of the chord varies as a function of the distance OQ along the (straight) base. This is, in fact, a generalization of the Bernoulli problem (τ=f(r) rather than τ=ar). He suggested how his instrument could easily be adapted to trace these kinds of tractional curves: the vertical plane on which the curve z=a-f(r) is traced is now fixed along the base such that, when the point F follows the curve, we have TC = a - TF = f(AT), and thus the required curve will be traced.

The article, characteristically Leibnizian, covered and generalized the kinds of tractional motion that had been discussed by others. But the generalisations completely disregarded practical feasibility. This disregard aroused criticism.

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When Leibniz' article appeared he was already engaged (as we have seen) in a discussion by letter with Huygens about the acceptability of physical processes in geometry. Huygens had written:

> I would like to know your opinion about my Tractoria for the quadrature of the hyperbola (-). What is remarkable there is that, according to the laws of mechanics, supposing the plane to be horizontal, the description must be perfect, and therefore also this quadrature by means of it. [59]

Leibniz answered:

> When one asks whether this construction [sc. by tractional motion HB] is geometrical, it is necessary to agree on the definition. In my way of speaking I would say that it is. I also think that the description of the cycloid, or of your curves by evolution, is geometrical. And I do not see

> why one restricts geometrical curves to those whose equation is algebraic. But among the geometrical constructions I prefer not only those that are the simplest, but also those that serve to reduce the problem to another, simpler problem and that contribute to enlighten the mind. For example I would wish to reduce the quadratures or the dimensions of areas to the dimensions of curved lines. [60]

Thus Leibniz moved on to new criteria of simplicity for curves and constructions in geometry. For instance he advocated dimensional simplicity preferring reduction to arc lengths to reduction to quadrature because arc length is one-dimensional and therefore simpler than two-dimensional quadrature. Huygens did not agree; he could not accept Leibniz' disregard for the criterion of instrumental simplicity and practical feasibility. On the letter in which Leibniz wrote to him Huygens had noted, beside a passage where Leibniz explained the motions of a complicated tractional process, "that would be difficult" [61], and the instrument for quadratures particularly aroused his disapproval. He first vented his criticism in correspondence with l'Hôpital. He found the title of Leibniz' article "very pompous",

> As if he were giving a universal method for tangents, better than any other. I would like to know your opinion of it; for my part, I find nothing poorer and more useless, considering the cumbersome and fully impractical methods of tracing he proposes. For it is almost impossible to construct with some exactness the simple Tractoria that I have given, which he pretends to have recognized before me (one could doubt that) as the quadratrix of the hyperbola. [62]

l'Hôpital entirely agreed:

> I have found that it corresponded so little to its ostentatious title that I hardly had the patience to read it, for his machine is so very complicated and so cumbersome that it cannot be of any use in practice, and what is more, this sheds no new light whatever on the inverse of tangents. There are people who wish they know everything, and as soon as others have published something new they want to claim the invention for themselves. [63]

The indignant tone in these reactions indicates how strongly Huygens and l'Hôpital felt about the matter. For them Leibniz' merely imagined motions did not solve problems; to claim otherwise was pompous, and irritating.

Meanwhile Leibniz had asked Huygens' opinion about his proposals [64]. Huygens gave it, less bluntly than in his letter to l'Hôpital, but clear enough:

> As to your application of the Tractoriae to the quadrature of curves, I must admit that I cannot find the advantage in it which you promise, for these descriptions are very cumbersome, and incapable of any exactness. It is almost impossible to trace with some precision the first and simplest one that I have proposed; those of Mr Bernoulli are already much more difficult (-). It is true, as you say, that every curve is a Tractoria, but I don't see at all that that makes it worthwhile considering others than the ones I mentioned. [65]

Leibniz defended his position stressing that practicality had never been a criterion for allowing or rejecting means of construction in geometry:

> As to my general construction of quadratures by traction, I think it suffices for science that it is exact in theory, even if it is not suitable for execution in practice. Almost all the most geometrical constructions are of that nature when they are composite. For instance the rulers of the instrumental Mesolabum of Mr Descartes would not be able to work exactly when larger numbers of them are required. And although Mr Descartes has proposed to construct equations of the fifth and sixth degree by motion of a material parabola, I believe that it would be quite difficult to perform such a construction exactly, to say nothing about higher degrees. [66]

Leibniz here referred to Descartes' construction of the roots of fifth and sixth degree equations by the intersection of a circle and a particular third degree curve, which was later called the Cartesian parabola. This curve was traced by the intersection of a parabola and a straight line moving with respect to each other. However, it seems that Leibniz hesitated to take his theoretical conception of exactness in its full consequence, for he also added a defence of the practicality of his instrument, thereby rather weakening the conviction of his argument:

> However, the general construction of all quadratures is infinitely more difficult, and nevertheless I think that the difficulties can be sufficiently reduced in practice if one uses a good pressure. For despite all the apparent obstacles, when the pressure does its job, the line of traction could not but be tangent to the curve. The younger Mr Bernoulli, having studied my description attentively, has recognised and admired its truth, although he too believes that it would be difficult to execute it well. [67]

The partners in the discussion did not convince each other. It is remarkable that some months later Leibniz no longer seemed so convinced of the value of tractional motion for the quadratures of curves; in a discussion of the problem he mentions the possibility of constructing by tractional motion as an aside:

> To reduce problems about transcendental curves to quadratures is certainly a great preparation for the solution, but I must admit (leaving aside my general tractional construction) that it is better to reduce the matter to rectifications of curves that are already constructed, something that indeed I have done and will do, whenever necessary. [68]

7. THE LEGITIMATION OF TRANSCENDENTAL CURVES - AN APPRAISAL OF THE DEBATE

The theme of tractional motion recurred several times in eighteenth century mathematics, and it appears that the preoccupation with the legitimacy and acceptability of procedures of solution, which characterised the studies discussed above, continued for a considerable time. However, these later studies did not add essentially new arguments to the discussion and I shall therefore not deal with the eighteenth century part of the mathematical history of tractional motion [69].

As we have seen in the preceding sections, tractional motion gave rise to fundamental discussions concerning the geometry of curves. We can now survey the themes in these discussions and thereby characterise the underlying view of geometry.

The first theme is the critique on the Cartesian paradigm. The confrontation with transcendental curves led mathematicians to question the basic Cartesian assumption, namely that the motions generating algebraic curves were geometrically acceptable whereas those generating non-algebraic curves were not. Tractional motion, being eminently simple and producing both algebraic and non-algebraic curves, contradicted this assumption and thereby the Cartesian demarcation of geometry.

Moreover, because curves were considered to be means for constructing problems, the confrontation with transcendental curves also undermined the Cartesian classification of problems, which depended on the degrees of the constructing curves.

Mathematicians noted and stressed the untenability of the Cartesian demarcation and classification, but they did not formulate consistent alternatives. Only Leibniz dared to formulate a new answer: all motions that were exact and continuous produced acceptable curves. This answer disregarded practical feasibility and gave no criterion of simplicity (on which a new classification of problems could be built), and we have seen that even Leibniz was somewhat hesitant to accept the full consequences. Other mathematicians just could not entirely dismiss simplicity of the motions (interpreted as instrumental simplicity) as a criterion. But they did not work out new demarcations and classifications on the basis of an alternative codification of simplicity.

If no new answers could apparently be found, why was the demarcation question not dismissed? The fact that the interest in the question remained alive is an indication of the strength of the practical mechanical imagery in the contemporary thinking about curves. Several of the machines for tractional motion which have been described above were not meant to be actually built; rather they have to be understood as aids to the mechanical imagination needed to help the mind to accept and understand the curves.

But there was also a more direct reason for the interest in the problem, namely that there was not (yet) an accepted analytical apparatus to represent transcendental curves simply by their formulas. Such formulas were hardly developed and they carried insufficient explanatory force for mathematicians to accept them as satisfactory means to represent a curve. Therefore, whenever mathematicians had to give the solution of a problem that involved a transcendental curve, that curve had to be represented by some sort of construction (by motion, by quadrature, by reduction to a curve assumed given etc.). Several examples of such constructions have been given above. Obviously mathematicians were frequently confronted with the foundational questions about curves, because these constructions had to be elaborated and formulated time and time again.

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Thus the discussions about tractional motion reveal a conception of the study of curves, predominant around 1700, which can be characterised as follows. The main aim of the study of curves was to solve problems. The solution of a problem consisted of a construction, that is, a sequence of steps taken from a canon of accepted standard constructions. The main foundational or methodological questions that interested or worried mathematicians in this field did not concern the rigour of the proofs or the existence of the objects of study, but concerned the legitimation and justification of these procedures of construction. These questions were considered important because in the minds of mathematicians the images of geometrical objects such as curves were strongly mechanical and because the procedures of construction served as means to represent the curves.

The discussions of these fundamental issues broke up the earlier Cartesian paradigm but they remained ultimately inconclusive; no clear-cut and functional new answers were found (nor is it, with hindsight, likely that such answers could have been found). However, the construction-centred view of the study of curves had a considerable influence on the direction of research in the field.

It should be noted that this characterization of the study of curves around 1700 is not based solely on the writings about tractional motion. One meets the same ways of thinking and arguing when one reads the studies on famous problems such as the catenary, the brachistochrone and the elastica, or those on exponential curves or the early studies on elliptic integrals [70]. But these works fall outside the scope of the present article.

The characterisation applies to a field of study that was in a phase of transition between the Cartesian paradigm and that of Eulerian analysis. Two features of this transition process are clearly reflected in the studies on tractional motion. The one is a process of <u>habituation</u> to new objects, namely transcendental curves. The confrontation with these curves was the main stimulus behind the changes in the field. The discussions of the proper construction or representation of transcendental curves, intense and inconclusive as they proved to be, may well be seen as evidence of a deeper process in which the mathematicians gradually became used to the new curves. Early on they needed all the support that their construction-centred view of geometrical objects could provide, later they could skip these procedures, and rely merely on the analytic expressions.

Secondly there was a change in the conception of the aim of geometry. The early modern view of geometry as the art and science of solving geometrical problems by construction was replaced by a conception of geometry as a study, by analytic means, of the properties and mutual relations of geometrical figures.

NOTES

[1] [Leibniz 1693b], pp. 296-297; Claude Perrault (1613-1688) was the brother of Charles Perrault (1628-1703), the famous author of the Mother Goose fairy tales.

[2] Cf. [Huygens Oeuvres] vol. 10, p. 517, note 5.

[3] See note [69]

[4] [Euler 1741], Riccati 1752], cf. note [69].

[5] For a more detailed discussion see my papers [1981] and [1984]

[6] Descartes presented it in his [1637]; it became a recognizable mathematical paradigm through the Latin edition with commentary and related treatises by van Schooten, [Descartes 1649] and [Descartes 1659]

[7] Keeping close to Leibnizian usage I shall write $\int f(x)dx$ to mean $\int_a^x f(t)dt$ for some suitably chosen a.

[8] [Leibniz 1693b] pp. 296-297.

[9] The manuscripts [Huygens ms 1692] have not been published in their entirety; the editors of the <u>Oeuvres</u> decided only to summarise their content and to quote some characteristic passages.

[10] [Huygens 1693a]

[11] [Huygens 1693a] pp. 408-409.

[12] "D'où l'on voit reciproquement comment on peut trouver les points de cette courbe, en supposant la quadrature de l'Hyperbole." [Huygens 1693a] p.409.

[13] "On doit avouer que ma courbe estant supposée ou donnée, on a la quadrature de l'Hyperbole. Si je trouve donc quelque moien de la decrire aussi exactement qu'avec un compas ordinaire on decrit un cercle, n'auray je pas trouvé cette quadrature? Qu'y a-t-il plus a dire a ma construction qu'a celle d'une ligne moienne proportionelle entre deux droites donnees? Il est vray que j'ay besoin du parallelisme d'un plan a l'horizon; mais cela est possible, non pas dans la derniere justesse, mais comme la droiture d'une regle. Pour le reste je decris ma courbe presque aussi facilement qu'un cercle et la machine que j'emploie approche fort la simplicité du compas." Huygens [ms 1692] fol. 62^r, [Oeuvres] vol. 10 p. 412, note.

[14] [Huygens ms 1692] fols. 59^r, 64^r

[15] [Huygens ms 1692] fol. 66^r

[16] [Huygens ms 1692] fols. 64^v-65^v

[17] [Huygens ms 1692] fol. 62^r

[18] "resoudra le problème de la quadrature hyperbolique " [ms 1692] p. 64^r, [Oeuvres] vol 10, p. 411, note.

[19] "Une charrette, ou un batteau servira a quarrer l'hyperbole. Si on fait nager sur l'eau un baquet en portion de sphere assez platte, avec un baton posè dessus en equilibre. et que l'on traine l'un des bouts en ligne droite, le point de l'axe de la portion spherique decrira la Tractoria exactement - sirop au lieu d'eau - Il faut tirer une pointe fiche au centre de la nacelle, et lentement." [ms 1692] fol. 64^r.

[20] [ms 1692] fol. 59^v [Oeuvres] vol 10, p. 420.

[21] "Non bene Cartesius e geometria sua rejiciebat curvas quarum naturam aequatione exprimere non poterat. Melius agnovisset geometriam suam hac parte mancam quod ad earum tractationem non attingeret non enim nesciebat talium quoque curvarum proprietates atque usus geometricis rationibus investigari. " [mss 1692] fol. 60^v.

[22] "Mais ce n'est pas pour tout ce que je viens de raporter touchant cette ligne que je la propose icy; mais pour une autre raison; qui est qu'on peut trouver moyen de la decrire par une machine assez simple, et par là reduire l'Hyperbole au quarré, ce qui m'a semblé digne de la consideration des Geometres. (-) Si cette description, qui par les loix de la Mechanique doit etre exacte, pouvoit passer pour Geometrique, de meme que celles des sections de Cone qui se font par les instrumens l'on auroit par elle, avec la quadrature de l'Hyperbole, la construction parfaite des Problemes qui se reduisent à cette quadrature; comme sont entre autres la determination des points de la Catenaria, ou Chainette, et les Logarithmes." [Huygens 1693a] pp. 409-412.

[23] [Joh. Bernoulli 1693], p. 66.

[24] "Elegans est hoc Problema, in quod incidimus occasione Hugenianorum quorundam, quae nuperrime in Actis Roterodamensibus comparuere." [Jak. Bernoulli 1693] p. 574.

[25] [Jak. Bernoulli 1693]

[26] [Leibniz 1693a]

[27] [l'Hôpital 1693a]

[28] [Huygens 1693b]

[29] [Leibniz 1693c]

[30] [l'Hôpital 1694]

[31] [l'Hôpital 1693b]

[32] [Huygens 1694a]

[33] [Huygens 1694b]

[34] [Leibniz 1694]

[35] See [Huygens Oeuvres] vol. 10, index V s.v. "Courbe de Bernoulli".

[36] See [Joh. Bernoulli Briefw.], index IV (pp. 515-517) s.v. "P_{37}".

[37] [l'Hôpital 1693a]

[38] See [Huygens Oeuvres] vol. 10, index V s.v. "Courbe de Bernoulli".

[39] [Jak. Bernoulli Opera] vol. 2 pp. 1082-1084.

[40] [Huygens 1694a] pp. 673-674.

[41] "In quacunque enim ratione sit M ad N, curva ABC semper eadem facilitate motu quodam continuo describi potest, non obstante, quod curva, pro ratione M ad N, magis vel minus composita evadat; in casu quippe rationis aequalitatis, illico patet curvam ABC esse circulum: in reliquis, si M ad N est ut numerus ad numerum, erit quidem curva geometrica; secus autem transcendentalis est. [Joh. Bernoulli 1693] p. 66.

[42] [Huygens 1693b] p. 513.

[43] "Unde patet, si constructiones ejusmodi censendae sunt geometricae et accuratae, aequationes infinitas altissimorum graduum, pari cum simplicissimis omnemque pene fidem excedente facilitate, construi posse." [Jak. Bernoulli 1693] p. 575.

[44] Huygens to Leibniz 17-9-1693, [Huygens Oeuvres] vol 10, pp. 509-512, here p. 510.

[45] Leibniz to Huygens 11-10-1693, [Huygens Oeuvres] vol 10, pp. 538-543, here p. 539.

[46] [Huygens 1693b] p. 513.

[47] "...quod ita solutiones generales habeantur, quae sua natura porriguntur ad quantitates transcendentes, in certis autem casibus, ut fieri potest, ad ordinarias ducunt. Mirarer, quod solas illas, quae aequationibus certi gradus subjacent, Geometricas vocare adhuc videtur, nisi judicarem, sequi magis vulgi morem ea in re, quam probare, dum de iis ait, quae Geometricae vocantur. Ego putem, ut veteres quidam recte reprehensi sunt, quod Geometricum satis esse negarent, quicquid circulo aut regula effici non posset; ita nec illorum hodie errori favendum esse, qui Geometriam solis aequationibus Algebrae gradariis metiuntur, cum Geometricum potius sit, quicquid motu continuo exacte construi potest. Quod si ille non admittit, suis ipse praeclaris inventis injuriam facit, cum ipsemet inprimis auxerit Geometricas constructiones: nam evolutionum inventum, quod Hugenio debemus, quantivis pretii est, et nunc tractorias constructiones protraxit in publicum primus." [Leibniz 1693c] pp. 290-291

[48] [Jak. Bernoulli 1693] p. 575.

[49] Huygens to l'Hôpital 5-11-1693 [Huygens Oeuvres] vol. 10 pp. 549-554, here pp. 550-551.

[50] That this arrangement indeed produces a curve with $PQ : OQ = p : 1$ can be seen as follows: During the motion, the wheel turns over a total angle of $W = OB/r_E$. Now let the situation drawn in Figure 5.5 correspond to an angle of w. We then have $OQ = OB - BQ = OB - r_E w = r_E(W-w)$. Also
$PQ + QB - r_F(W-w) = OB$, whence
$PQ = OB - QB + r_F(W-w) = OQ + r_F(W-w) = (r_E+r_F)(W-w)$, so that throughout the motion $PQ : OQ = (r_E+r_F) : r_E = p : 1$ (because $r_F:r_E=(p-1):1$) as required.

[51] [Leibniz 1693b]; for a detailed study of Leibniz' efforts to legitimate transcendental relations and quantities in mathematics, see [Breger 1986].

[52] Note that, as was mentioned in Section 3, Leibniz and his contemporaries restricted themselves almost exclusively to problems in which the data and the required properties of the solution could be expressed algebraically. Thus in studying quadratures and arc lengths they would generally take the curve to be algebraic, and in dealing with inverse tangent problems they assumed the given property of the tangents to be algebraically expressible. In other words, they dealt primarily with integrals of algebraic integrands and with differential equations whose terms were algebraic.

[53] "Et quidem si exacta sit construendi ratio, recipitur in Geometriae theoriam; si facilis sit utilisque, potest recipi in praxin. Nam et motus secundum certas hypotheses factus Geometricae est tractationis, exemplo centri gravitatis." [Leibniz 1693b] p. 295.

[54] "Hinc autem fit, ut talis motus mire sit accomodatus ad Geometriam transcendentem, quia immediate refertur ad lineae tangentes, vel directiones, adeoque ad quantitates elementares, numero quidem infinitas, magnitudine autem inassignabiles seu infinite parvas." [Leibniz 1693b] p. 296.

[55] "quicquid motu continuo exacte construi potest" [Leibniz 1693c] p. 290.

[56] Huygens to l'Hôpital 24-12-1693, p. 579, quoted below, note [62], cf. Leibniz [Schr. Br.] vol. (3) 1, p. LXXI, note 370.

[57] "quae protusio non impedit, quo minus protrudens punctum C sola tractione fili moveatur adeoque ejus directionem in motu servet." [Leibniz 1693b] p. 300.

[58] "cujus proinde natura et descriptio haberi potest in tabula LRM per geometriam ordinariam " [Leibniz 1693b] p. 300.

[59] "Je voudrois bien scavoir vostre jugement touchant ma <u>Tractoria</u> pour la quadrature de l'Hyperbole (-). Où il y a cela de remarquable, que suivant les loix de la Mechanique, supposè le plan horizontal, la description doit estre parfaite, et par consequent cette quadrature par son moien." Huygens to Leibniz, 17-9-1693, [Huygens Oeuvres] vol. 10 pp. 509-512, here p. 510.

[60] "Lorsqu'on demande si cette construction est Geometrique il faut convenir de la definition. Selon mon langage je dirois qu'elle l'est. Aussi crois ie que la description de la cycloide, ou de vos lignes faites par l'evolution, est Geometrique. Et je ne vois pas, pourquoy on restreint les lignes Geometriques à celles dont l'equation est Algebrique. Mais entre les constructions Geometriques ie prefere non seulement celles qui sont les plus simples mais aussi celles qui servent à reduire le probleme à une autre probleme plus simple et contribuent à éclairer l'esprit. Par example ie souhaiterois de reduire les quadratures ou les dimensions des aires aux dimensions des lignes courbes." Leibniz to Huygens 11-10-1693, [Huygens Oeuvres] vol. 10 pp. 538-543, here p. 541.

[61] "Cela serait difficile" Leibniz to Huygens 11-10-1693 [Huygens Oeuvres] vol. 10 pp. 538-543, here p. 540.

[62] "...fort pompeux, comme s'il donnait une methode universelle et meilleure que nulle autre pour les Tangentes. J'en apprendrai volontiers vostre sentiment, car pour moy je ne trouve rien de plus pauvre ni de plus inutile, vu les descriptions embarassées et tout à fait impracticables qu'il apporte. Car à peine pourroit on construire avec quelque exactitude cette simple Tractoria que j'ay donnée, laquelle il prétend avoir reconnue devant moy, (de quoy on pouroit douter) pour la quadratrice de l'Hyperbole." Huygens to l'Hôpital 24-12-1693, [Huygens Oeuvres] vol. 10 pp. 577-579, here pp. 578-579.

[63] "...j'ai trouvé qu'il repondoit si peu au titre fastueux, qu'a peine ay-je eu la patience de le lire, car sa machine est si fort composée, et tellement embarassée qu'elle ne peut estre d'aucun usage dans la pratique, et de plus cela ne donne aucune vue nouvelle pour l'inverse des tangentes, ce sont des gens qui veullent tout savoir et qui d'abort que les autres ont fait paroittre quelque chose de nouveau s'en veullent attribuer l'invention,..." l'Hôpital to Huygens 18-1-1694, [Huygens Oeuvres] vol. 10, pp 579-581, here p. 580.

[64] Leibniz to Huygens 11-10-1693, [Huygens Oeuvres] vol. 10, pp. 538-543, here p. 540.

[65] "Touchant l'application que vous avez faite des <u>Tractoriae</u> à la quadratures des Courbes, j'avoue que je n'y puis trouver cet avantage que vous promettez, car ces descriptions sont tres embarassées, et incapables d'aucune exactitude. A peine peut on tracer avec quelque justesse cette premiere et plus simple que j'ay proposée, celles de Mr. Bernouilli estant desia beaucoup plus difficiles, (-) Il est vray, comme vous dites, que toute courbe est <u>Tractoria</u>, mais je n'en vois point qu'il vaille la peine de considerer que celles dont je viens de parler. " Huygens to Leibniz 29-5-1694, [Huygens Oeuvres] vol. 10, pp. 609-615, here p. 611.

[66] "quant à ma construction Generale des Quadratures par la Traction, il me suffit pour la science qu'elle est exacte en theorie quand elle ne seroit pas propre à estre executée en practique. La plus part des constructions les plus Geometriques, quand elles sont composées, sont de cette nature. Comme par example les regles du Mesolabe organique de M. des Cartes ne sçauroient operer exactement, lors qu'elles doivent estre un peu multipliées. Et quoyque M des Cartes ait proposé de construire les Equations du 5.me ou 6.me degré par un mouvement de la parabole materielle, je crois qu'on auroit bien de la peine à faire une telle construction avec exactitude pour ne rien dire des degrés plus

hauts." Leibniz to Huygens 22-6-1694, [Huygens Oeuvres] vol. 10, pp. 639-646, here p. 642.

[67] "Cependant la construction generale de toutes les quadratures est infiniment plus difficile, et neantmoins je crois que les difficultés pourroient estre assez diminuées en practique en se servant d'une bonne appression. Car non obstant tous les embarras apparens, l'appression faisant son devoir, la ligne de la traction ne sçauroit manquer de toucher la courbe. Monsieur Bernoulli le cadet, ayant consideré attentivement ma description, en a reconnu et admiré la verité, quoyqu'il croye aussi qu'il seroit difficile de la bien exécuter." Leibniz to Huygens 22-6-1694, [Huygens Oeuvres] vol. 10,pp. 639-641, here p. 642.

[68] "Problemata curvarum transcendentium ad quadraturas reducere, magna quidem ad solutionem praeparatio est; fateor tamen (seposita mea generali constructione tractoria) praestare rem reduci ad linearum jam constructarum reductiones, quod et ego quoties opus feci faciamque." Leibniz [1694] pp. 293-294 [interpreting "reductiones" as a misreading or miswriting for "rectificationes"]

[69] The story of tractional motion in the eighteenth century can be summarised as follows:

Early in the century two articles on the tractrix appeared. One was [Bomie 1714] (see also [Fontenelle 1714]) which gave proofs of the theorems about the curve presented by Huygens in his [1693a]. The other was [Perks 1715] which, independently, also gave the proofs and added designs of instruments for tracing the tractrix and related curves. Regarding these works see [Pedersen 1963]; Pedersen reports on reconstructions of Perks' instruments made at the Institute for History of Science at Aarhus University; these models, although requiring a little practice from the operator, work well.

Independently of these studies, but inspired by Huygens' original article, Giovanni Poleni designed and constructed, in 172#, an instrument for tracing the tractrix. His aim was to legitimate the curve by providing an instrument that was as precise as the then current instruments for tracing conics. Poleni instigated a discussion by letter on the instrument and its practical and legitimating merits. He received reactions from G. Manfredi, J. Riccati and A. Conti. He published his findings together with the comments in the form of a letter to J. Hermann [Poleni 1729].

The theme of tractional motion resurfaced in the 1740's, this time not so much in connection with justification of the tractional curves but relating to their use in solving differential equations. In his [1741] Euler discussed the differential equation $ds + s^2dz = Z(z)dz$, of which the Riccati equation $ds + s^2dz = z^mdz$ is a special case. Noting that in general separation of the variables was not possible, so that no analytical expression for the solution could be found, Euler had recourse to older canons of solution, in this case construction by tractional motion. He showed that, given $Z(z)$, it was possible to construct (pointwise) a curve which, if used as the base of tractional motion with constant tangent, provided a tractional curve, out of which, again pointwise, the solution curve of the differential equation could be constructed. When Clairaut later encountered the differential equation in a study of a mechanical problem [Clairaut 1745], he noted that nobody had been able to separate the variables and gave Euler's construction, without further proof. Vincenzo Riccati read Clairaut's article around 1750, worked out the proofs himself (apparently he was unable to trace Euler's original article), wrote about it to M. Agnesi in 1750 (the letter was published later as [V. Riccati 1753]) and wrote a book about the use of tractional motion in constructing (i.e. solving) differential equations [V. Riccati 1752]. The substance of that book was published once more in the textbook on analysis ([V. Riccati/Saladini, 1765] vol. 2, pp. 470-487).

[70] Cf. my paper [1974].

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