The Inverse Method of Tangents: A Dialogue between Leibniz and Newton (1675–1677)

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Introduction

Following the creation of Analytic Geometry, several procedures for determining tangent lines to certain classes of functions given by a relation between two variables x and y had been described. The converse of these direct methods of tangents is the problem of deriving the equation of the function itself, given only the knowledge of a certain characteristic property of its tangent lines. This inverse problem of tangents had been studied in a special case by DESCARTES himself in 1638/1639.

When LEIBNIZ began to develop his calculus, he soon recognized it to be of the utmost importance and closely related to the problem of quadrature. A number of manuscripts from the period of his early mathematical studies give evidence of the significant rôle of this problem in LEIBNIZ' thoughts. This is even more strongly emphasized by the fact that he repeatedly touched upon inverse tangent problems in his correspondence with NEWTON, via OLDENBURG.

NEWTON's opinion on the subject is mainly found in his second letter for LEIBNIZ, the so-called *Epistola Posterior*[1]. It is, to a certain degree, backed up by some of his published tracts, the history of whose composition, however, is only incompletely known at present. A final evaluation therefore has to be postponed until his mathematical manuscripts, now being prepared for publication by Dr. D. T. WHITESIDE at Cambridge, will be generally available.

Further comments on the inverse method of tangents are contained in the famous *Commercium Epistolicum D. J. Collins et aliorum de analysi promota* (London 1712/13); they are not considered here. The whole question proved to be one of great importance in the mathematical discussions of the following decades.

1. Leibniz: The creation of his calculus and his first thoughts on the inverse method of tangents

It is well known that since 1673 LEIBNIZ had been looking for a method of handling infinitesimal problems. He was searching for a formalism, a calculus, suitable to express the variations of functional relationships as they occur in questions of

[1] All dates are given in New (Gregorian) Style, which was ten days ahead of the Old (Julian) Style then still in use in England and some parts of the continent. — English translations of Latin source material are taken from NCT and C so far as available. — For abbreviations and sources of manuscripts and letters see the Appendix, p. 134.

this type. During a few days in October 1675[2], he made the decisive steps: the introduction of the symbols dx $\left(\text{first as } \frac{x}{d}\right)$ and $\int f(x)\,dx$ into the study of infinitesimal problems, and the establishment of the basic rules for the new notation.

At once a number of known results were checked by means of the new "calculus", and its power was tested by applying it to further problems. Among those, the so-called "inverse tangent problems" deserve special consideration.

Several methods had been invented for determining the tangent lines of certain classes of functions, but nobody had yet made known similar procedures for the solution of the inverse problem. "Given that the tangent line of a curve has a certain property, how can one determine the curve itself?" — this was the question.

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LEIBNIZ had a clear idea of this problem as early as 1673. In fact, while he indulged himself in the study of the important mathematical works of his time, he had made some progress along this line. To a paper of August 1673[3], of which only excerpts have been published so far, he gave the significant title, *Methodus tangentium inversa, seu de functionibus*, after having first named it *Methodus nova investigandi tangentes linearum curvarum ex datis applicatis, vel applicatas ex datis productis, reductis, tangentibus, perpendicularibus, secantibus.*

While it is not my intention to repeat MAHNKE's penetrating analysis[4] of this piece, I have to mention some points which concern us here. LEIBNIZ stated the problem thus: "To find the locus of the function, provided the locus [or law] which determines the subtangent is known," and, later in the same manuscript, he spoke of the "regress from the tangents or other functions to the ordinates" where "other functions" mean such expressions as normals, subtangents, subnormals, *etc.* He continued:

> The matter will be most accurately investigated by tables of equations; in this way we may find out in how many ways some one equation may be produced from others, and from that, which of them should be chosen in any case. This is, as it were, an analysis of the analysis itself, but if that be done it forms the fundamental of human science, as far as this kind of things is concerned[5].

This reveals that it was LEIBNIZ' idea to prepare a set of tables of functions together with their derivatives, as we should say to-day. It would tell which functions appear as derivatives of the common curves such as circle, ellipse, hyperbola, parabola, cycloid, tractrix, *etc.*, and would, at the same time, serve as an integral table useful for dealing with quadratures, rectifications and inverse tangent problems. So LEIBNIZ, who had just set foot on new ground and had hardly begun to learn to walk on it, was already conceiving the plan of surveying the new territorium.

This is, as it were, an analysis of the analysis itself.

He finally emphasized in this early manuscript

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[2] C.c. II, nos. 1089/92. See HEL, pp. 118/123.

[3] C.c. II, no. 575.

[4] M, pp. 43/59.

[5] C, p. 60.

... that almost the whole of the theory of the inverse method of tangents is reducible to quadratures[6].

Moreover, he had gained support for his belief by a detailed investigation of a few examples which had led him to consider infinite series. The subtangent s of a function is to its ordinate y as the infinitely small unit dx (LEIBNIZ used 1 for it) to the difference of two neighboring ordinates (dy), $s/dx = y/dy$ (see Figure 1). The problem, hence, is to determine the ordinate y itself (as a function of x) from the difference dy of such ordinates, this difference being (because of the special choice of $dx=1$) equal to the ratio of the ordinate y to the subtangent s. Considering the parabola, for which $y/s = y/2x$, LEIBNIZ introduced $x_0=1$, $x_1=2$, ..., $x_n=n+1$ and derived for the corresponding y_n the series

$$y_n = y_0 + \frac{y_0}{2\cdot 1} + \frac{y_0}{2^2\cdot 1\cdot 2} + \\ + \frac{y_0}{2^3\cdot 1\cdot 2\cdot 3} + \cdots.$$

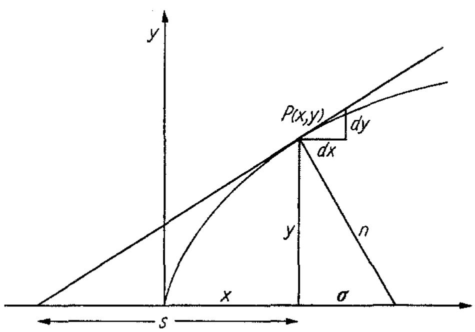

Fig. 1

He obtained a similar result for the circle and the hyperbola, and although his reasoning is not quite sound, as MAHNKE[7] has shown, LEIBNIZ was convinced he possessed a general method yielding an infinite series of rational numbers, about which he exclaimed:

This invention is of the greatest importance. By this means, the progress of the ordinates of any figure can be obtained geometrically by an infinite series of rational numbers. Thus we have a general method to effect arithmetical quadratures of perfect exactness, and mechanical ones, which come arbitrarily close to the geometrical ones.... An arithmetical quadrature is one in which the area of a figure is exactly and geometrically represented by an infinite series of rational numbers. Geometrical and completely perfect is a quadrature if the area can be represented exactly by another quantity. But it is mechanical if the area can be represented by another quantity whose difference from the true one is so small that it can be neglected in practical problems. Nobody before me has given an arithmetical quadrature of the circle[8].

In the last sentence, of course, he was thinking of his famous series

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + - \cdots$$

which he had found shortly before[9]. Yet it was a very general method that he had arrived at: Inverse tangent problems could, in principle at least, be reduced to the summation of infinite series.

In a manuscript of the following year[10] LEIBNIZ ascertained that

... the quadratures of all figures follow from the inverse method of tangents, and thus the whole science of sums and quadratures can be reduced to analysis, a thing that nobody even had any hopes of before[11].

[6] C, p. 60.
[7] M, pp. 52/55.
[8] M, p. 54 (my translation).
[9] HEL, pp. 34/36.
[10] C.c. II, no. 791: *Schediasma de methodo tangentium inversa ad circulum applicata.*
[11] C, pp. 60/61.

A further manuscript from the fall of 1674[12] shows LEIBNIZ' continued concern with infinite series, but, unlike NEWTON, he did not concentrate so much on this particular direction of mathematical research.

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There still exist many more manuscripts of LEIBNIZ' investigations during those creative years 1673 to 1676, but only a few have been published so far[13]. In a group of papers from November, 1675, we can find further remarks about our problem, some of a more general nature, others dealing with specific examples.

On November 11[14], for instance, LEIBNIZ was concerned with the following five problems. Supposing first that the subnormal

$$\sigma = \frac{y\,dy}{dx}$$

(see Fig. 1) be inversely proportional to the ordinate y, $\sigma = a^2/y$, he found easily $y^3/3 = a^2 x$, so that the cubic parabola has the given property. (LEIBNIZ did not add a constant since he considered all curves to begin in the origin.) Next he studied $\sigma = a^2/x$ giving $y^2/2 = a^2 \int (dx/x)$, "which cannot be determined without the help of the logarithmic curve." Hence the figure which satisfies the condition $\sigma = a^2/x$ has as ordinate y the square root of the logarithm of the abscissa x. This is one of the transcendentals, LEIBNIZ concluded.

The following two examples start from similar conditions: $x + \sigma = a^2/y$ and $x + \sigma = a^2/x$, where $x + \sigma$ represents the distance from the origin to the x-intercept of the normal. They give $(x^2 + y^2)/2 = a^2 \int (dx/x)$ and $(x^2 + y^2)/2 = a^2 \int (dx/x)$, respectively. LEIBNIZ, however, did not yet use the differential dx under the integral sign, and so was mistaken in considering $\int (dx/y)$ as the logarithm of y.

His final example in this note may have been inspired by the results of the previous two, for he put $\sigma = \sqrt{x^2 + y^2}$, which leads to $y\,dy = \sqrt{x^2 + y^2}\,dx$, $y^2/2 = \int \sqrt{x^2 + y^2}\,dx$. Since this integral is inaccessible, LEIBNIZ attempted to find an approximate solution[15]. He started with $x = 1$ in the equation $y^2 = \sqrt{x^2 + y^2}$, obtained $y^4 = 1 + y^2$, solved it wrongly as $y = \sqrt[4]{5}/\sqrt{2}$; then he substituted back again, obtaining $y^2 = \sqrt{1 + (\sqrt[4]{5}/\sqrt{2})^2}$, as a better approximation.

It is interesting to observe this step in the right direction, although LEIBNIZ was not successful at this time. It is far more essential to realize the ease and clarity of the work which led to the solution or to the integral from which the solution is to be obtained. No clumsy descriptions are necessary, no special geometric transformations have to be carried out in order to find the results. The individual geometric investigation of old has been replaced by the general and formal method of the calculus — a new kind of algebra, specially developed for the study of infinitesimal problems. Much was left to be done, of course, but the foundation for further work had been laid and had stood the first test.

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[12] C.c. II, no. 775: *Schediasma de serierum summis.*

[13] See HW, footnote (= f) 3 and HEL, pp. 228/230 in general — HEL, p. 77, f 286; pp. 122/124, ff 619/621; p. 125, f 624; p. 155, f 769; p. 158, ff 785/787; p. 160, f 811 in particular.

[14] C.c. II, no. 1120: *Methodi tangentium inversae exempla.*

[15] See HEL, p. 124.

In a manuscript of November 22, 1675[16], LEIBNIZ introduced a new idea. It is taken from the direct method of tangents, with which he was continually concerned. In order to generalize the results obtained by DESCARTES and SLUSE in their attempts to describe procedures for determining the tangent lines to given functions, LEIBNIZ had begun to tackle the problem from the following point of view. The given equation $f(x, y)=0$ is paired with another function $g(x, y)=0$. Both functions will normally intersect each other a number of times; but when in solving them together (by eliminating one of the two variables) a multiple root is obtained, they will touch each other in a point $P(a, b)$, say. Now, if $g(x, y)=0$ is chosen such that its tangent is known from previous investigations, then in $P(a, b)$ this tangent line will be identical with the tangent of $f(x, y)=0$ which was sought. DESCARTES, for $g(x, y)=0$, had used a circle, SLUSE a straight line; LEIBNIZ recognized that neither choice was compulsory, and he remarked:

> Hence I go on to say that not only can a straight line or circle, but any curve you please, chosen at random, be taken, so long as the method for drawing tangents to the assumed curve is known; for thus, by the help of it, the equations for the tangents to the given curve can be found. The employment of this method will yield elegant geometric results that are remarkable for the manner in which long calculation is either avoided or shortened, and also the demonstrations and constructions. For in this way we proceed from the easy curves to more difficult cases. ... Hence I fully believe that we shall derive an elegant calculus for a new rule of tangents[17].

In a similar way LEIBNIZ hoped to solve the inverse problem, too, as is evident from the following phrases:

> Now this very general and extensive power of assuming any curve at will makes it possible, I am almost sure, to reduce any problem to the inverse method of tangents or to quadratures ... .
>
> The whole thing, then, comes to this; that, being given the property of the tangents of any figure, we examine the relations which these tangents have to some other figure that is assumed as given, and thus the ordinates or the tangents to it are known. The method will also serve for quadratures of figures, deducing them one from another; but there is need of an example to make things of this sort more evident; for indeed it is a matter of most subtle intricacy[18].

Behind all this one easily detects the plan to construct an extensive table of functions and derivatives or integrals; this would be, LEIBNIZ expected, the key to the new analysis and the link between direct and inverse method of tangents. But he still hoped to be able to complete the construction by means of auxiliary functions — presumably he thought a direct investigation by means of differences or sums was needed only for a few basic functions, and all the rest could be deduced from their properties by comparison.

There exists another paper by LEIBNIZ, dated only five days later[19], in which he dealt with the same sort of problems. He tried in fact to supply an example of the kind he had called for, but made an unfortunate choice[20]. Yet it is clear

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[16] C.c. **II**, no. 1125.

[17] C, pp. 112/113.

[18] C, p. 113.

[19] C.c. **II**, no. 1131: *Pro methodo tangentium inversa* ... GERHARDT dated this paper wrongly Nov. 21 instead of Nov. 27 — see HW, f 3.

[20] See C, pp. 105/107.

that he was still aiming at the elimination of one of the unknowns in $f(x, y)=0$ by the help of the auxiliary function $g(x, y)=0$ in order to be able to integrate term by term, for he said in a final note in this manuscript of November 27, 1675:

Whenever the formula for the one unknown that is left in shackles is such that the unknown is not contained in an irrational form or as a denominator, the problem can always be solved completely; for it may be reduced to a quadrature, which we are able to work out; the same thing happens in the case of simple irrationals or denominators. But in complex cases, it may happen that we obtain a quadrature that we are unable to do. Yet, whatever it may come to, when we have reduced the problem to a quadrature [*i.e.* to the form $dy/dx=f(x)$], it is always possible to describe the curve by a geometrical motion; and this is perfectly within our power, and does not depend on the curve in question. Further, this method will exhibit the mutual dependence of quadratures upon one another, and will smooth the way to the method of solving quadratures[21].

In these words LEIBNIZ has summarized most of his ideas concerning the inverse tangent method as they had developed by the end of 1675. He aimed at the construction of tables by means of auxiliary functions in such a way that ultimately all problems could be reduced to the form $dy/dx=f(x)$. He had attempted to deal with a few problems explicitly, but, as far as we can judge from the material available, did not get very far. Yet his new algebraic symbolism had proved to be far superior to the traditional geometric methods then in common use. He even had approached the method of approximate solution in a case where he could not immediately succeed otherwise.

What he did not mention here is the method of infinite series which, as we have seen, he occasionally had employed — in the fall of 1673 and again in October 1674. Had it perhaps lost some of its attractiveness during the past months? If so, then not for much longer. For HOFMANN[22] reports an investigation which LEIBNIZ carried on during the early summer of 1676 concerning

$$y=x+\frac{x^2}{2!}+\frac{x^3}{3!}+\cdots.$$

LEIBNIZ found that for this function

$$\int_0^x y\,dx=y-x,$$

and he knew that the corresponding differential equation would be satisfied only by the function $x=\log(1+y)$. But whatever the rôle of the method of infinite series finally may have been in LEIBNIZ' structure of the new analysis, it seems fair to say that in the years up to 1676 it did not occupy the central position, notwithstanding the fact that he had obtained some very interesting results by it.

2. Leibniz on Debeaune's problem

After all that has been said it will not be surprising that a little later the subject was hinted at in the correspondence which LEIBNIZ kept up with OLDENBURG, the secretary of the Royal Society in London. This, at least, is the inter-

[21] C, p. 108.
[22] HEL, p. 155.

pretation given by the editor of *The Correspondence of Isaac Newton* [NCT] to the following passage in LEIBNIZ' letter of December 28, 1675 [23]:

> But to another geometrical problem * too, hitherto regarded as well-nigh impossible of solution, I have recently discovered a successful means of approach: about this I shall speak more at length when I have leisure to complete it .... From these you will recognize, I believe, not only that problems have been solved by me, but also that new methods ** (for this is the one thing that I value) have been disclosed [24].
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> * NCT editor: This is perhaps a reference to the problem of de Beaune and Descartes, a foretaste of a solution of a differential equation: to find the curve for which the subtangent is constant. It was a problem on which Leibniz had recently been at work.
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> ** NCT editor: The infinitesimal calculus, rather than the methods and theories of algebraical equations, is meant.

The reference to DEBEAUNE and DESCARTES just quoted points to the first source of this type of problem. It had been raised by FLORIMOND DEBEAUNE (1601—1652), a French councillor at Blois who was one of the first scholars to read thoroughly the famous *Géométrie* of DESCARTES published in 1637. The treatment of the method of tangents had caused DEBEAUNE to formulate the inverse tangent problem in full generality, and in order to inaugurate its study he had publicly invited the French mathematicians in the fall of 1638 to submit solutions to several examples suggested by him [25]. FERMAT [26], ROBERVAL, BEAUGRAND and DEBEAUNE himself do not seem to have been very successful, as far as we know; only DESCARTES' attempt to give a complete solution of one of them has been preserved in a condensed version [27].

LEIBNIZ' attention had already been drawn to the letter of DESCARTES, in which his result is given, by HUYGENS [28], and in July 1676, a few months after he had coined his new symbolic calculus, LEIBNIZ looked up the work of DESCARTES

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[23] LEIBNIZ to OLDENBURG, 28 XII 1675.

[24] NCT I, p. 402.

[25] See S.

[26] Besides DEBEAUNE, FERMAT seems to have recognized the existence of the inverse tangent problem at about the same time. In the explanation of his method of finding extreme values and tangent lines of a function, given in French in about 1638, he said: "One could in succession search for the converse of this proposition and, given the property of the tangent, look for the curve to which this property shall fit. To this question lead those about burning-glasses which have been suggested by Descartes. But this deserves a separate discourse." FERMAT in fact suggested in the following line carrying on such a discussion with his correspondent DESCARTES, but nothing came of it as far as we know.

This French piece is mentioned by CANTOR, *Vorlesungen über Geschichte der Mathematik* ²**2** (1900), pp. 857/858 and 864, who dated it at 1638 from internal evidence. It was first published in CH. HENRY, *Recherches sur les manuscrits de Pierre Fermat* ..., Bull. Bibl. (Boncompagni) **12** (1879). CANTOR quotes p. 663, but the copy of the Bodleian Library at Oxford has this passage on p. 713, the whole article comprising pp. 477—568 and 619—740. There seems to exist an offprint with independent page-numbering for CANTOR actually gives "p. 189 (**12**), 663." — The sentences quoted are found in the last but one paragraph of No. XIX: "Méthode des Maximis expliquée et envoyée par Fermat à Descartes" in the second part of HENRY's article. — The French piece was later included in FO **2** (1894) as No. XXXI, pp. 154—162, where the passage quoted above is printed on p. 162.

[27] DESCARTES to DEBEAUNE, 20 II 1639.

[28] See HEL, p. 158.

again — though not very carefully. DEBEAUNE's statement of the example in question is equivalent to the differential equation

$$\frac{dy}{dx} = \frac{x-y}{n} \tag{2.1}$$

(n constant)[29].

LEIBNIZ[30] made a slip in setting up the ratios and started from

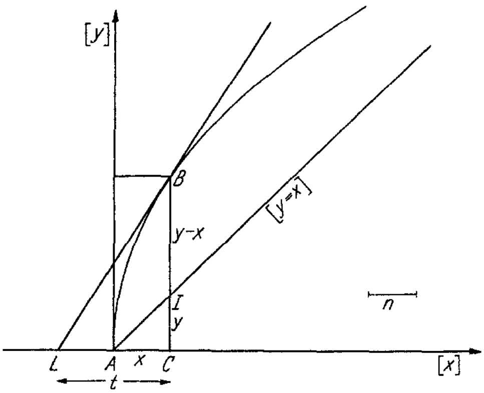

Fig. 2

$$\frac{t}{y} = \frac{n}{y-x} \tag{2.2}$$

where t is the subtangent, $t = \frac{y\,dx}{dy}$, so that his equation becomes

$$\frac{dy}{dx} = \frac{y-x}{n}. \tag{2.3}$$

(See Figure 2. LEIBNIZ' figure, which is reproduced here, departs from his habit in that the position of the axes agrees with the modern usage!) He then gives

$$\frac{n}{t} = \frac{y-x}{y} = 1 - \frac{x}{y}, \qquad \frac{x}{y} = 1 - \frac{n}{t} = \frac{t-n}{t}, \tag{2.4}$$

but

$$\frac{t}{y} = \frac{dx}{dy}. \tag{2.5}$$

Therefore

$$\frac{dx}{dy} = \frac{n}{y-x}, \qquad dx \cdot y - x \cdot dx = dy \cdot n.$$

Hence

$$\int dx \cdot y - \int x \cdot dx = n \cdot \int dy. \tag{2.6}$$

Now

$$\int dy = y, \qquad \int x \cdot dx = \frac{x^2}{2},$$

and $\int dx \cdot y$ is the area ACB. Thus a curve is sought for which this area ACB be equal to

$$\frac{x^2}{2} + ny \quad \text{or} \quad \frac{\overline{AC}^2}{2} + n \cdot \overline{BC}. \tag{2.7}$$

When the area $x^2/2$ of the triangle ACI is subtracted, the remaining area AIB must equal the rectangle $n\,y$.

So far LEIBNIZ. The progress he has made here is very small: From the differential equation

$$\frac{dy}{dx} = \frac{y-x}{n} \tag{2.3}$$

he has derived the integral equation

$$\int y\,dx = \frac{x^2}{2} + ny. \tag{2.7}$$

[29] See S, p. 410, Eq. (4.1).
[30] C.c. **II**, no. 1483.

He did not think of expanding y into an infinite series as he had done before. Instead, he turned back to the starting point of DESCARTES: to the fact that the subtangent, referred to the asymptote which he had found, is constant. This DESCARTES had given without proof; he had made use of it in his construction by basing it on a new coordinate system. LEIBNIZ now did the same. He referred the following work to a new figure adapted from DESCARTES (Figure 3; only the essential lines are drawn here). The asymptote BC becomes the X-axis and forms an angle of 45° with the Y-axis BA. Hence the curve AVX he is looking for will no longer start at the new origin B, but at A. BAC is an isosceles right triangle.

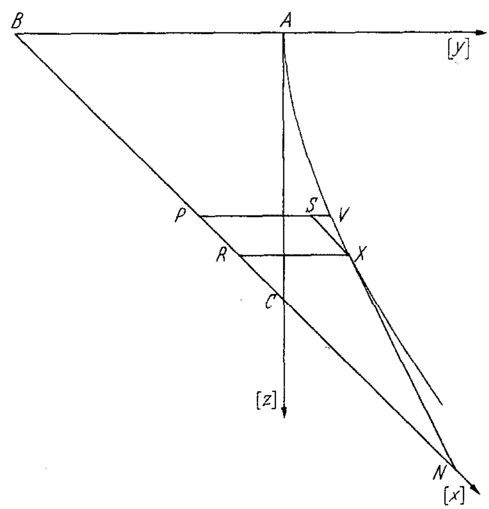

Fig. 3

From a point X on the assumed curve (with $y=RX$, $x=BR$) he proceeded to a neighboring point V with coordinates

$$y+dy=\overline{RX}+dy=\overline{PS}+\overline{SV}$$

and

$$x-dx=\overline{BR}-\overline{PR}=\overline{BR}-\overline{SX}.$$

If the tangent line is drawn at X, intersecting BC in N, then

$$t=\overline{RN}=\overline{BC}=c \qquad (2.8)$$

according to DESCARTES' remark. Using similarity of the infinitesimal triangle SVX and the triangle RXN, LEIBNIZ obtained

$$\frac{dy}{dx}=\frac{y}{t}=\frac{y}{c}. \qquad (2.9)$$

(The left hand term should be $-\frac{dy}{dx}$, since dx and dy are of opposite sign.) Next he replaced x by its projection onto $\overline{AC}=f$:

$$z=\frac{f}{c}x, \qquad (2.10)$$

and, after considering briefly the moment of a certain area, he returned to

$$\int\frac{dy}{y}=\frac{1}{f}z, \qquad (2.11)$$

saying that, unless he is mistaken, the integral is in our power. (Actually, about eight months before, he had studied the integral $\int\frac{a^2}{y}dy$ and recognized its connection with the logarithm.) To support his memory, he now jotted down: Figures of the kind

$$\frac{dy}{y},\quad \frac{dy}{y^2},\quad \frac{dy}{y^3}$$

must be determined the same way as

$$y\,dy, \quad y^2 dy, \quad \text{etc., and} \quad d\sqrt{ay} = \frac{1}{2\sqrt{ay}} \quad -$$

and then it came back to him: $\int (dy/y)$ appertains to the logarithm.

Filled with joy about this result he exclaimed optimistically:

Thus we solve all the problems on the inverse tangent method which are contained in the letters of Descartes.

Inserting the missing minus sign, returning to $x = \frac{c}{f} z$, and noting that the curve has to pass through $A(0, c)$, for reference purposes we complete formula (2.11) as

$$\int \frac{dy}{y} = -\frac{z}{f} = -\frac{x}{c} \tag{2.12}$$

to give

$$x = -c \log \frac{y}{c}. \tag{2.13}$$

It should be noted that this solution refers to the coordinate system formed by the Y-axis BA and the asymptote BC of Figure 3, and not to Figure 2.

About a month later, in his letter to OLDENBURG of August 27, 1676[31], LEIBNIZ again boasted of the success he supposedly had achieved, with the following words:

... problems of the inverse method of tangents, which even Descartes admitted to be beyond his power .... This curve neither Descartes nor de Beaune nor anyone else, so far as I know, has found. I myself, however, on the day, indeed in the hour, when I first began to seek it, solved it at once by a sure analysis. Yet I admit that I have not yet attained to everything of this sort which can be desired, though I know it to be of the greatest importance[32].

3. From Newton's Epistola Prior to his Epistola Posterior

The letter of LEIBNIZ to OLDENBURG just quoted was only one of several in his official correspondence with the Royal Society on recent discoveries in mathematics and the sciences[33]. The congenial partner, for whom its contents, above all, were determined, was of course ISAAC NEWTON at Cambridge. He had already composed a letter for LEIBNIZ[34], almost exclusively devoted to the method of infinite series; but there are a few sentences which are relevant in our context.

In OLDENBURG's copy of this letter for LEIBNIZ[35], dated August 5, 1676, this passage reads as follows:

From all this is to be seen how much the limits of analysis are enlarged by such infinite equations: in fact by their help analysis reaches, I might almost say, to all problems, the numerical problems of Diophantus and the like excepted. Yet the result is not altogether universal unless rendered so by certain further methods of developing infinite series. For there are some problems in which one cannot arrive at infinite series by division or by the extraction of roots either simple or affected. But how to proceed in those cases there is now no time to explain; nor time to report some other

[31] LEIBNIZ to OLDENBURG, 27 VIII 1676.

[32] NCT II, p. 71.

[33] HEL, esp. p. 151.

[34] NEWTON to OLDENBURG, 23 VI 1676 (= *Epistola Prior*).

[35] OLDENBURG to LEIBNIZ, 5 VIII 1676 (= *Epistola Prior*). It is an exact copy of NEWTON to OLDENBURG, 23 VI 1676; except for a short postscript in OLDENBURG's hand it was copied by his amanuensis.

things which I have devised, about the reduction of infinite series to finite, where the nature of the case has admitted it. For I write rather shortly because these theories long ago began to be distasteful to me, to such an extent that I have now refrained from them for nearly five years[36].

The reader may wonder why I present a passage in which nothing is to be found about the inverse problem of tangents. The answer lies in the subsequent exchange of letters between the two great mathematicians.

LEIBNIZ' response to the *Epistola Prior* is his letter of August 27, 1676 from which I have already quoted at the end of Section 2. Just before those words he, replying to NEWTON's passage above, had interjected:

What you and your friends seem to say, that most difficulties (Diophantine problems apart) are reduced to infinite series, does not seem so to me. For there are many problems, in such a degree wonderful and complicated, such as neither depend upon equations nor result from squarings [ut neque ab æquationibus pendeant, neque ex quadraturis], as for instance (among many others) problems of the inverse method of tangents which even Descartes admitted to be beyond his power[37].

It very surprising and not easy to explain why LEIBNIZ here not only has his doubts as to the power of the method of infinite series (this would perhaps be understandable on the grounds discussed at the end of Section 1), but also why he so wholeheartedly denies the dependence of inverse tangent problems on quadratures. More than once, as we have seen, had he emphasized just this close connection between both — why should he suddenly question it?

*

NEWTON's answer came in his second great letter for LEIBNIZ, through care of OLDENBURG[38]. There one again finds a comment on inverse tangent problems in direct reply to the Leibnizian remarks:

When I said that almost all problems are soluble I wished to be understood to refer specially to those about which mathematicians have hitherto concerned themselves, or at least those in which mathematical arguments can gain some place. For of course one may imagine others so involved in complicated conditions that we do not succeed in understanding them well enough, and much less in bearing the burden of such long calculations as they require.

Nevertheless — lest I seem to have said too much — inverse problems of tangents are within our power, and others more difficult than those, and to solve them I have used a twofold method of which one part is neater, the other more general. At present I have thought fit to register them both by transposed letters, lest, through others obtaining the same result, I should be compelled to change the plan in some respects. 5accdæ10effh11i4l3m9n6oqqr8s11t9v3x: 11ab3cdd10eæg10ill4m7n6o3p3q6r5s11t8vx, 3acæ4egh5i4l4m5n8oq4r3s6t4vaaddæeeeeeiijmmnnooprrrsssssttuu.

This inverse problem of tangents, when the tangent between the point of contact and the axis of the figure is of given length, does not demand these methods. Yet it is that mechanical curve the determination of which depends on the area of an hyperbola. The problem is also of the same kind, when part of the axis between the tangent and the ordinate is given in length. But I should scarcely have reckoned these cases among the sports of nature [ludus naturæ]*. For when in the right-angled triangle, which is formed by that part of the axis, the tangent and the ordinate,

* NCT **II**, p. 160, note (74): Newton must have received the misquotation "ludus naturæ" ("sport of nature") instead of (as Leibniz had written) "hujus naturæ" ("of this nature"). Leibniz noted the error in a letter to Conti, 9 April 1716.

[36] NCT **II**, p. 39.

[37] NCT **II**, p. 71.

[38] NEWTON to OLDENBURG, 3 XI 1676 (= *Epistola Posterior*).

the relation of any two sides is defined by any equation, the problem can be solved apart from my general method. But when a part of the axis ending at some point given in position enters the bracket, then the question is apt to work out differently[39].

NEWTON's anagram was later revealed to stand for

Una Methodus consistit in extractione fluentis quantitatis ex æquatiône simul involvente fluxionem ejus: altera tantum in assumptione Seriei pro quantitate qualibet incognita, ex qua cætera commodè derivari possunt, & in collatione terminorum homologorum æquationis resultantis, ad eruendos terminos assumptæ seriei[40].

The translation in NCT reads:

One method consists in extracting a fluent quantity from an equation at the same time involving its fluxion; but another by assuming a series for any unknown quantity whatever, from which the rest could conveniently be derived, and in collecting homologous terms of the resulting equation in order to elicit the terms of the assumed series[41].

In the passages quoted in this section we meet the core of the dialogue as far as our subject is concerned. NEWTON, who at first had only vaguely indicated the range of his new method of "infinite equations", at last accomodated himself to write a bit more fully. Nevertheless, he is still very reluctant, disguising his principal results in an unsolvable anagram, giving a few details on the case where the triangle that is similar to the Leibnizian characteristic one appears, and yet immediately retracting when somewhat more elaborate explanations would become necessary. To a careful reader, the spirit of these three hundred words reflect that of the whole letter: NEWTON is not really interested to continue the correspondence — might he not be drawn into another controversy that will steal him his time and bring nothing but anger and useless but unending discussions? The claim for his inventions is stated in the anagram — what more can he desire? In fact, the short covering letter to OLDENBURG of the same day[42] has this postscriptum:

I hope this will so far satisfy M. Leibnitz that it will not be necessary for me to write any more about this subject. For having other things in my head, it proves an unwelcome interruption to me to be at this time put upon considering these things.

Yet two days later we see NEWTON writing to OLDENBURG in a somewhat different mood[43]. Obviously afraid he might have been too harsh on LEIBNIZ, he asked OLDENBURG "to mollify" what he may consider to be expressed too severely. NEWTON even went so far as to say:

I believe M. Leibnitz will not dislike ye Theorem towards ye beginning of my letter pag. 4 for Squaring Curve lines Geometrically. Sometime when I have more leisure it's possible I may send him a fuller account of it: explaining how it is to be ordered for comparing curvilinear figures wth one another, & how ye simplest figure is to be found wth wch a propounded curve may be compared[44].

But the time where NEWTON had more leisure for such things did not come, and the hopes of his partner, who was eager to continue this exchange of ideas, were soon to be disappointed.

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39 NCT II, p. 148.
40 WO III, p. 645. NCT II, p. 159.
41 NCT II, p. 159.
42 NEWTON to OLDENBURG, 3 XI 1676; NCT II, p. 110.
43 NEWTON to OLDENBURG, 5 XI 1676.
44 NCT II, p. 163.

4. Comments on Newton's Epistola Posterior

In this section documentary background material for the claims raised by NEWTON in his *Epistola Posterior* is presented, and the marginal notes entered by LEIBNIZ on his copy of the Newtonian letter are discussed.

NEWTON's reply may conveniently be divided into two parts. Up to and including the anagram, he spoke about his general methods for dealing with inverse problems of tangents and related questions. Afterwards, he turned to a few particular examples. Both sections require some attention.

In the general part, NEWTON claimed to have a twofold method. This, according to the solution of the anagram, consisted

(i) in extracting a fluent quantity $[x, y]$ from an equation at the same time involving its fluxion $[\dot{x}, \dot{y}]$;

(ii) in assuming a series whose coefficients were to be determined from the conditions of the problem.

It is pointed out in NCT[45] that both parts are elaborated in NEWTON's *Methodus fluxionum et serierum infinitarum*, which was published by HORSLEY under the title *Geometria analytica sive specimina artis analyticae*[46]. It is in Chapter 4 of this work that he dealt with the doctrine of fluxions. Its first part has to do with differentiating functions; its second part, which concerns us here, is devoted to the problem: To find the relation between certain fluents [quantities] provided an equation involving their fluxions [derivatives] is given, *i.e.* in modern terms, to solve certain differential equations of the first order in x, y, $\dot{y}/\dot{x}$. NEWTON distinguished between three cases:

(1) equations with x or y absent;

(2) equations with both x and y present;

(3) equations involving more than two fluxions.

Case (1) permits rewriting the given equation in the form $\dot{y}/\dot{x} = f(x)$ (or $\dot{x}/\dot{y} = g(y)$); several examples are worked out to show that $y = \int f(x)\,dx$ (or $x = \int g(y)\,dy$) will be the solution. (An example is given on p. 127 below.) Case (2), where $\dot{y}/\dot{x} = f(x, y)$, is solved in an ingenious way by setting up a table to be filled in step by step with the aim of obtaining an infinite series for y. NEWTON's fourth example may indicate how the method works even when fractional exponents occur[47].

Given is the differential equation

$$\frac{\dot{x}}{\dot{y}} = \frac{1}{2}\,y - 4y^2 + 2y\,x^{\frac{1}{2}} - \frac{4}{5}\,x^2 + 7y^{\frac{5}{2}} + 2y^3. \qquad (4.1)$$

NEWTON writes down all the terms that do not contain x in the top row, the others, arranged according to increasing powers of x, in the left-hand column. The places in the rectangular field thus outlined will later be filled in one by one; the following row is reserved for the vertical sums to be formed in the process; from it will be obtained the sequence for x by integration. There are

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[45] NCT II, pp. 159/160, note (72).

[46] NH I, pp. 389/518. This treatise grew out of an older one, the *Analysis per aequationes numero terminorum infinitas* (1669). NEWTON said that Chapter 4 was written in 1671. *Cf.* HSV, pp. 49/55 and f 201, f 206.

[47] NH I, p. 422.

	$+\frac{1}{2}y$	$-4y^2$	$+7y^{\frac{5}{2}}$	$+2y^3$			
$+2y\,x^{\frac{1}{2}}$	*	$+y^2$	*	$-2y^3$	$+4y^{\frac{7}{2}}$	$-2y^4$	&c.
$-\frac{4}{5}x^2$	*	*	*	*	*	$-\frac{1}{20}y^4$	&c.
Summa	$+\frac{1}{2}y$	$-3y^2$	$+7y^{\frac{5}{2}}$	*	$+4y^{\frac{7}{2}}$	$-\frac{41}{20}y^4$	&c.
$x =$	$\frac{1}{4}y^2$	$-y^3$	$+2y^{\frac{7}{2}}$	*	$+\frac{8}{9}y^{\frac{9}{2}}$	$-\frac{41}{100}y^5$	&c.
$x^{\frac{1}{2}} =$	$\frac{1}{2}y$	$-y^2$	$+2y^{\frac{5}{2}}$	$-y^3$	&c.		
$x^2 =$	$\frac{1}{16}y^4$	&c.					

two more rows, one for $x^{\frac{1}{2}}$ and one for x^2, since these occur as factors in the left-hand margin.

The aim is to obtain a series in y for x (since $\dot{x}/\dot{y}$ was given); there is not yet anything else to be filled in in the first column; thus the sum is $y/2$, its integral $x=y^2/4$, its square root $y/2$, its square $y^4/16$. When these newly found values are substituted for $x^{\frac{1}{2}}$ and x^2, respectively, in the left-hand margin, y^2 and $-y^4/20$ result and are entered in the appropriate places of the rectangular field.

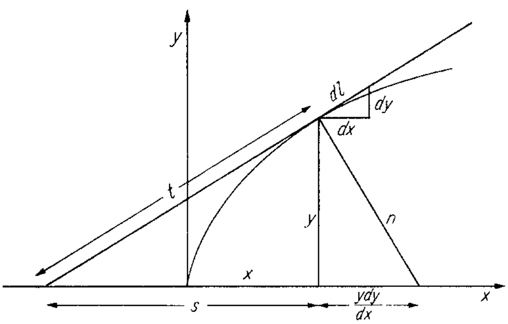

Fig. 4

Next the sum of the second column has to be formed, $-3y^2$, which on integration yields $-y^3$. Hence one now has $x=y^2/4-y^3$; therefore $x^{\frac{1}{2}}$ will begin as $y/2-y^2$ — NEWTON originally determined the root by a long division. x^2 would begin as $y^2/16-y^5/2$ but is no longer written down — he did not intend to carry the computation so far. The term $2y\,x^{\frac{1}{2}}$ then yields $-2y^3$, so nothing is added to the entrance of the third column, $7y^{\frac{5}{2}}$, and this value is integrated to give $2y^{\frac{7}{2}}$. In this way the procedure has to be carried on *ad infinitum* unless a general pattern discloses itself, which would permit writing down the law of the terms in the final series.

NEWTON gives many more examples for each of his three cases. Hence there is no reason to doubt that the interpretation in NCT of the general part in NEWTON's answer, *i.e.* of the anagram, is the correct one.

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Turning now to the second part of NEWTON's passage, we first express his specific examples in mathematical language. From Figure 4 it is obvious that

$$t=\frac{y\sqrt{dx^2+dy^2}}{dy} \quad \text{or} \quad dx=\frac{\sqrt{t^2-y^2}\,dy}{y} \tag{4.2}$$

and

$$s=\frac{y\,dx}{dy} \qquad \text{or} \qquad \frac{dy}{y}=\frac{dx}{s}. \tag{4.3}$$

In NEWTON's symbols, (4.3) is

$$\frac{\dot{x}}{\dot{y}}=\frac{s}{y},$$

which he had discussed (with x and y being interchanged) as an example of case (1)[48] (see p. 125). To solve it, he had suggested replacing y by $b+y$ (b an arbitrary constant), dividing

$$\frac{\dot{x}}{\dot{y}}=\frac{s}{b+y}=\frac{s}{b}-\frac{s\,y}{b^2}+\frac{s\,y^2}{b^3}-\frac{s\,y^3}{b^4}+-\cdots$$

and integrating term by term:

$$x=\frac{s\,y}{b}-\frac{s\,y^2}{2b^2}+\frac{s\,y^3}{3b^3}-+\cdots. \tag{4.4}$$

NEWTON had left the result in this form, without hinting at the fact that a logarithmic function turns up here.

Now, in his letter, he stated that both problems [equations (4.2) and (4.3)] do

> ... not demand these methods for it is that mechanical curve[49] the determination of which depends on the area of an hyperbola.

In short, he claimed to be able to integrate these equations directly without the help of series.

In NCT[50] it is noted that "the former [problem, my equation (4.2)] belongs to type III of the table given by Horsley (*I*, 378)". This is slightly misleading; what is meant is not the table *on* p. 378, but the one that is inserted *between* p. 378 and 379. The former is a table of integrals which can be evaluated in algebraic form, the latter contains functions whose quadratures depend on those of conic sections. And the curve with a constant tangent was known to be the tractrix, one of the transcendentals.

As type III,1 NEWTON has tabulated:

The form of the curve	The conic section: abscissa	The conic section: ordinate	The area of the curve
$y=\frac{d}{z}\sqrt{e+f\,z^\eta}$	$\frac{1}{z^\eta}=x^2$	$\sqrt{f+e\,x^2}=v$	$\frac{4de}{\eta f}\left(\frac{v^3}{2e\,x}-s\right)$ where $s=\int v\,dx$.

With the substitutions

$$d=1, \quad e=t^2, \quad f=-1, \quad \eta=2, \quad z=y=\frac{1}{x}, \quad v=\sqrt{t^2x^2-1},$$

$$\frac{d\sqrt{e+f\,z^\eta}}{z}\to\frac{\sqrt{t^2-y^2}}{y},$$

[48] NH I, pp. 417/418, no. 37.

[49] The distinction between geometric [= algebraic] and mechanical [= transcendental] curves is due to DESCARTES.

[50] NCT II, p. 160, note (73).

and the expression for the area becomes

$$\int \frac{\sqrt{t^2-y^2}}{y}\,dy = -\int \frac{\sqrt{t^2x^2-1}}{x^2}\,dx = -2t^2\left(\frac{(t^2x^2-1)^{\frac{3}{2}}}{2t^2x} - \int \sqrt{t^2x^2-1}\,dx\right). \tag{4.5}$$

This clearly exhibits how the problem "depends on the area of an hyperbola."

In fact, added to the table are four diagrams which serve to represent the areas (*i.e.* integrals) whenever possible. In the particular case in question, NEWTON'S Figure 3 (shown here in a simplified version as Figure 5), representing the hyperbola

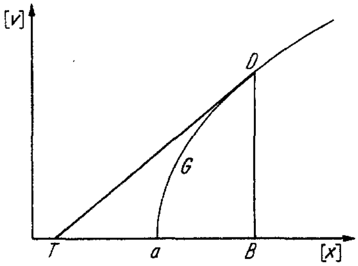

Fig. 5

$$v = \sqrt{t^2x^2-1} \quad \text{or} \quad \frac{x^2}{1/t^2} - v^2 = 1,$$

is applicable. It gives the branch G of this hyperbola in the first quadrant, beginning at the vertex a $[=1/t]$. At a point $D(x, v)$ the tangent line is drawn intersecting the X-axis at T. Then, according to NEWTON, the resulting area is equal to

$$\frac{4de}{\eta f} \quad \text{in} \quad aGDT,$$

i.e. minus $2t^2$ times the trilineum bounded by the hyperbola, its tangent line and the X-axis. But, if BDT is a right-angled triangle,

$$\begin{aligned} aGDT &= BDT - aGDB \\ &= \frac{1}{2}\,\frac{t^2x^2-1}{t^2x}\,v - \int v\,dx = \frac{(t^2x^2-1)^{\frac{3}{2}}}{2t^2x} - \int \sqrt{t^2x^2-1}\,dx, \end{aligned}$$

since the x-coordinates of B and T are x and $1/t^2x$, respectively. *Q.e.d.*

NEWTON in all probability constructed type III of the table (as well as many of the other eleven types given therein) by "working backwards" from the results obtained by the procedure of differentiating. In the example at hand, this would mean beginning with the last integral

$$2t^2 \int \sqrt{t^2x^2-1}\,dx = 2t^2 \int v\,dx$$

and applying integration by parts (observing $v\,dv = t^2x\,dx$):

$$2t^2 \int v\,dx = 2\int (v^2/x)\,dv = 2v^3/3x + \tfrac{2}{3}\int (v^3/x^2)\,dx.$$

Now

$$v^3/x^2 = v\,(t^2 - 1/x^2),$$

therefore

$$\tfrac{2}{3}\int (v^3/x^2)\,dx = (2t^2/3)\int v\,dx - \tfrac{2}{3}\int (v/x^2)\,dx.$$

On combining, this gives

$$2t^2 \int v\,dx = v^3/x - \int (v/x^2)\,dx$$

as was desired in (4.5).

In a more modern way, the characteristic property of the tractrix is most conveniently derived from its parametric equations

$$\begin{aligned} x &= a(u - \tanh u), \\ y &= a \operatorname{sech} u. \end{aligned} \tag{4.6}$$

Then

$$dx/du = a(1 - \operatorname{sech}^2 u) = a \tanh^2 u,$$
$$dy/du = -a \operatorname{sech} u \tanh u,$$
$$dl/du = \sqrt{(dx/du)^2 + (dy/du)^2} = a \tanh u,$$

and

$$t = \frac{y\,dl}{dy} = -a. \qquad (4.7)$$

Elimination of u in (4.6) yields

$$x = \sqrt{t^2 - y^2} - \log \frac{t - \sqrt{t^2 - y^2}}{y},$$

which agrees with the result of integration of (4.2).

*

Finally, after NEWTON had rebuffed the misquotation "ludus naturae," he pointed again to the triangle with sides y, b, and s, claiming that any problem involving two of its sides could be solved apart from his general method. But it would change the picture if some other part of the axis should enter the equation.

If we take these words at their face value, they will soon be seen to involve certain difficulties. The original Latin reads:

> Nam quando in triangulo rectangulo quod ab illa axis parte [pars axis inter tangentem et ordinatim applicatam] & tangente ac ordinatim applicata constituitur, *relatio duorum quorumlibet laterum* [my italics] per æquationem quamlibet definitur, Problema solvi potest absque mea methodo generali, sed ubi pars axis ad punctum aliquod positione datum terminata ingreditur vinculum tunc res aliter se habere solet[51].

NEWTON, speaking first of a *relatio* and not only of a *ratio*, must have had in mind the three cases

N. (1) $f(y, s) = 0$ or $s = \dfrac{y\,dx}{dy} = f(y)$, i.e. $dx = \dfrac{f(y)\,dy}{y}$;

(2) $f(y, t) = 0$ or $t = \dfrac{y\sqrt{dx^2 + dy^2}}{dy} = f(y)$, i.e. $dl = \sqrt{dx^2 + dy^2} = \dfrac{f(y)\,dy}{y}$;

(3) $f(s, t) = 0$ or $s = f(t)$ or $t = f(s)$.

If so, then he went too far in saying that he could handle all three cases without the help of his general methods — certainly the last case is a very delicate one! But how else are we to interpret his words?

Then, where he referred to relations when a part of the abscissa different from the subtangent s *ingreditur vinculum* — enters the expression — he must have had in mind the idea of replacing the segment s by expressions such as x, $s - x$, may be even $s + y\,dy/dx$ *etc.* (*cf.* Fig. 4). Hence we should have to consider two more classes of functions:

N. (4) $f(s - x, y) = 0$, $f(s + y\,dy/dx, y) = 0$, ...,

(5) $f(x, t) = 0$, $f(s - x, t) = 0$, $f(s + y\,dy/dx, t) = 0$, ...

In these cases, so much is evident, "the matter is apt to work out differently."

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[51] NCT II, p. 129.

Let us now compare this with the marginal notes which LEIBNIZ entered on his copy of the Newtonian letter[52]. Obviously trying to understand NEWTON's scanty remarks, he jotted down what amounts to the following relations (the numbering again is mine):

L. (1) $s=f(y) \quad - \quad x=\int \frac{f(y)}{y}\, dy;$

(2) $t=f(y) \quad - \quad \int \frac{f(y)}{y}\, dy = \int \sqrt{dx^2+dy^2};$

(3) $s=f(x) \quad - \quad \int \frac{dy}{y} = \int \frac{dx}{f(x)}.$

The editor of NCT wondered whether LEIBNIZ tried the fourth case which suggests itself here:

T. (1) $t=f(x) \quad - \quad \left[\frac{y\sqrt{dx^2+dy^2}}{dy} = f(x)\right],$

"a much more difficult one — which, in Newton's words ... would be 'apt to work out differently?'"[53] As I have pointed out above, I believe NEWTON's "fourth" case really consisted of N.(4) and N.(5), the latter one comprising of course T.(1). On the other hand, L.(3) does not seem to fit into the picture at all involving, as it does, two segments both taken from the horizontal coordinate axis. After all, it does not matter too much, since L.(1) and L.(2) do agree with N.(1) and N.(2), respectively, and yet it would be interesting to know whether LEIBNIZ understood NEWTON better than he had expressed himself, or whether the German mathematician was reading into this passage of his English correspondent the meaning which he expected to find in it.

*

Finally a word about the promise concerning the quadrature of curved lines in NEWTON's letter to OLDENBURG[54] written two days after the great *Epistola Posterior* (*cf.* p. 124). NEWTON here referred to theorems on integration and to a list of functions squarable by comparison with conic sections, such as he had given in his previous report (without, however, stating the integrated form). There he had summed up the matter in the words:

> But when any curve of this kind cannot be squared geometrically there are other theorems at hand for comparison of it with conic sections, or at any rate with other figures of the simplest kind with which it can be compared[55].

It has been shown that this statement pointed at such material in NEWTON's possession as can be found in the treatise *De Quadratura*. These ideas must have sounded very familiar to LEIBNIZ, who had himself conceived the plan of a systematic arrangement of tables of functions and their integrals. In fact, he did return to it again in the following letters.

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[52] LMG I, pp. 145/146; NCT II, pp. 209/212 (with a correction).
[53] NCT II, p. 212, note (6).
[54] NEWTON to OLDENBURG, 5 XI 1676.
[55] NCT II, p. 136.

5. The conclusion of the dialogue: Leibniz' answers

LEIBNIZ replied immediately[56] to the *Epistola Posterior*, which he had received only late in June of the following year[57]. He opened a long paragraph on the inverse tangent problem with the remark that, when NEWTON said these problems were in his power, he obviously meant by means of infinite series. But he, LEIBNIZ, desires a solution exhibiting the curves geometrically — at least, if their quadratures are assumed to be given. To quote an example: HUYGENS had found that the cycloid is its own evolute. Now to solve the problem, to describe a curve which is its own evolute, would have been difficult; yet this is an example of the class of the inverse method of tangents. Another one would be, LEIBNIZ continued, to find an analytical curve whose arc-length is equal to the area under a given analytical curve. To do the opposite has long been known.

In the following sentences LEIBNIZ referred to the special cases which NEWTON had mentioned. He stated:

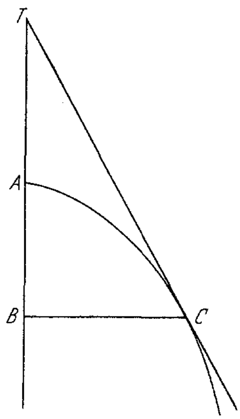

Fig. 6

When Newton says that the discovery of the curve does not require these methods because the tangent, or the intercept taken on the axis between the tangent and the ordinate, is a constant right line, he hints, I suppose, that he understands the general inverse method of tangents to be in his power by means of methods of series, [or] approximations, but that in this special case there is no need of series. But I was looking for a method which would accurately exhibit the desired curve, on the supposition, of course, of the squarings, and by the help of which we should be able to find its equation, if it has one, or another primary property. His assertion that problems, in which the relation between two sides of a triangle TBC is given, can always be resolved, is true[58], and it flows from my procedures also, and it can often be supplied by a simple analytical operation, without even bringing in squarings. For instance, if BC be assumed to be x, and $TB = b\,x + c\,x^2 + d\,x^3 + \cdots$, the question is what is the kind of curve that has this property of tangents; that is, what is the equation expressing the relation between AB (or y) and BC (or x)? I assert that it will be $y = b\,x + \frac{1}{2}\,c\,x^2 + \frac{1}{3}\,d\,x^3 + \cdots$. Had TB been equal to $a + b\,x + c\,x^2$, the squaring of the hyperbola would have been required to find the desired curve[59].

Moreover, in general, in whatever way the relation is given between two sides of this triangle which I am in the habit of calling "characteristic" (because of its numerous uses), always, supposing the squarings of analytical figures, the desired curve can be obtained. Yet I do not know if anyone besides Newton is likely to supply this result. By my method, the thing is effected and proved in the calculation of a single little line .... But when the most distinguished Newton asserts that the matter does not proceed in the same way if there is given the relation of the term TB to part of the axis, that is, to AB or y, my reply to this is that for me it is just as easy to find the nature of the curve or its equation if the relation of TB to AB is given, as it is if the relation to BC is given, as he requires. But we do not yet, as far as I know, possess the general inverse method of tangents[60].

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[56] LEIBNITZ to OLDENBURG, 1 VII 1677.

[57] *Cf.* HEL, p. 179.

[58] Here the draft contained the following passage marked for exclusion when copied for NEWTON: "If, for instance, $TB = a + b\,x + c\,x^2$, or $dy/dx = (a + b\,x + {} + c\,x^2)/x$, it is true, ..., giving $y = \int (a/x)\,dx + b\,x + c\,x^2/2$."

[59] The last remark refers to the excluded passage, *viz* to the integral $\int (a/x)\,dx$. [For this and the previous footnote, see LMG **I**, p. 159, and LBG, pp. 245/246.]

[60] NCT **II**, pp. 223/224.

The examples quoted in this paragraph can be verified immediately by noticing that, in LEIBNIZ' notation, $TB:BC=dy/dx$, i.e. $TB=x\,dy/dx$ [LEIBNIZ has interchanged x and y] (Fig. 6). He had included another example (after the passage "a single little line") which I omit since it contains some evident errors and does not introduce important new ideas. But whatever else he said is in complete agreement with his marginal notes. He still takes the last case to be L.(3) (*cf.* p. 130), and therefore is entitled to say that "It is just as easy," namely "the thing is effected and proved in the calculation of a single little line": his

$$\int \frac{dy}{y} = \int \frac{dx}{f(x)} \quad -$$

"always supposing the squarings of analytical figures."

What LEIBNIZ is really claiming, then, is that he has a full insight into the relation between inverse tangent problems and quadratures. Under certain conditions, they are reducible to problems of integration. Others, however, more complicated, cannot easily be reduced to integrations: "we do not yet, as far as I know, possess the general inverse method of tangents."

*

Some days later LEIBNIZ was prompted to send another letter to OLDENBURG[61]. Having succeeded in finding the method for inverting an infinite series $z=ay+by^2+cy^3+\cdots$ into the form

$$y=\frac{z}{a}-\frac{bz^2}{a^3}+\cdots$$

for which he had asked in his previous letter, he did not wish NEWTON to think any longer that he, LEIBNIZ, would not be able to do it — in fact, he emphasized that he once had used it himself but had then neglected it.

In the following paragraph, constituting almost a third of this letter, he returned for yet another time to the inverse method of tangents:

> In addition to what I have noted in a previous letter, namely, the inverse geometrical method of tangents (granted, of course, the squarings of analytical curves), and to other matters of that kind, we need to be able to know for certain as regards squarings whether the squaring of some proposed figure cannot be reduced to the squaring of the circle or the hyperbola. For it has been possible to square most of the figures so far handled by the aid of the one or the other. But if it can be proved, as I think it can, that some figures are not squarable either by means of the circle or of the hyperbola, it remains for us to establish some other higher primary figures, to whose squaring all others may be reduced, when it is possible to do this. So long as it is not done, we are stuck here, and we often seek by means of a particular infinite series what could be reduced to the squaring of the circle or the hyperbola, or to some other squaring of a better known figure. Gregory had believed that the measurement of the curves of the hyperbola and the ellipse did not depend on the squaring of the circle or the hyperbola. But I have discovered a certain form of hyperbolic curve, which I can measure from the given squaring of the same hyperbola. As for the rest of them, it is not yet clear to me[62].

LEIBNIZ here is explaining further what he had said in his previous reply: the inverse problem of tangents depends on quadratures; it can be solved in so far as the squarings can be obtained. But integrations, this he had realized from

[61] LEIBNIZ to OLDENBURG, 22 VII 1677.
[62] NCT II, p. 233.

the beginning when he first thought of tables of derivatives and integrals, fall into a number of classes. Many can ultimately be reduced to the squaring of the circle, *i.e.* to an integral $\int \sqrt{a^2 - x^2}\, dx$, others to the area of the hyperbola $\int (dx/x)$ or $\int \sqrt{a^2 + x^2}\, dx$. And yet, it seems doubtfult whether the solution of all such problems can finally be obtained from the circle and hyperbola alone. More likely than not, some other figures will be needed. In the meantime we often use particular infinite series to solve such problems — does this not sometimes hide a reduction to circle or hyperbola integrals in cases where it might be possible? To compile a list of "primary figures" is thus a thing of great importance.

It is so for another reason, too, for measurements of curves, *i.e.* rectifications, also reduce to quadratures. LEIBNIZ believed in particular he had found a method of rectifying an hyperbola based only on its squaring, and thus that he had a counter-example to a result of GREGORY, but this was due to a computational error, as HOFMANN has discovered[63]. "As for the rest of them, it (was) not yet clear" to him.

*

This is the last piece in the correspondence under discussion. OLDENBURG replied once more[64] only in order to say that NEWTON is presently engaged otherwise, and that further letters from him are not to be expected in the near future. And with OLDENBURG's sudden death[65] shortly afterwards LEIBNIZ lost the mediator who had kept up for him the connection with the scholars in England. Thus another link between the English mathematicians and those on the continent was broken, and the gap between them gradually became more and more visible.

* * *

Perhaps the most striking impression one gets from studying these papers and letters is the similarity of NEWTON's thought and LEIBNIZ' concerning the construction of integral tables. For both of them, these form a major tool of the new analysis, a means of surveying the vast new field which they have opened up. Both recognize the ultimate dependence of quadratures on (hopefully) a few basic functions — NEWTON probably went farther in actually preparing such tables.

Here as well as in the discussion about specific inverse tangent problems the geometric point of view is still very strong: integration, for instance, is not yet an abstract procedure but a formalized geometric operation — wherever possible, the result of it is exhibited as a certain area.

The same is true, as the name itself reveals, for the inverse method of tangents. It seems to be this very fact, this emphasis on the geometric origin of the problem, that prevented both LEIBNIZ and NEWTON from expressively embedding it into the wider class of problems nowadays called differential equations. (Of course, there was as yet no theory of such equations apart from a few individual manipulations such as solving for dy/dx, using infinite series, *etc.*) But both seem to foresee a development in this direction when they speak about inverse tangent problems in general terms.

63 HEL, p. 118.
64 OLDENBURG to LEIBNIZ, 19 VIII 1677.
65 NCT II, p. 235, note (3).

Can we imagine how the new analysis might have flourished had NEWTON and LEIBNIZ continued their correspondence in competing partnership?

A considerable part of this paper was written while I was a member of the Department of Mathematics at the University of Toronto.

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B. Catalogue of Letters and Manuscripts

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For a manuscript, the author is named; for a letter, both sender and recipient are given.

Besides works where published pieces may be found, further books or articles containing comments on or extracts from unpublished pieces are given for reference purpose. (No attempt at completeness has been made.)

Date	Letter or Manuscript	Publications	Comments
1639			
20 II	DESCARTES to DEBEAUNE	DCA **III**, pp. 184/194; DO **II**, pp. 510/523	S, pp. 410/416
1673			
VIII	LEIBNIZ (C.c. **II**, no. 575)		C, pp. 59/60; G 1848, pp. 20/22; G 1855, pp. 56 et seq.; HEL, pp. 44, 124; HW, pp. 278/279, f 3; M, pp. 52/55
1674	LEIBNIZ (C.c. **II**, nos. 840, 844—846, 849)		HEL, p. 77, f 286
X	LEIBNIZ (C.c. **II**, no. 791) (Sched. de meth. tang. inv.)		C, pp. 60/61; G 1848, p. 22; G 1855, p. 57; HEL, p. 77
X	LEIBNIZ (C.c. **II**, no. 775) (De serierum summis)		C, pp. 61—62
XII	LEIBNIZ (C.c. **II**, nos. 820, 823—824, 832—833, 839)		HEL, p. 158, f 785; HW, pp. 278/279, f 3
1675	LEIBNIZ (C.c. **II**, no. 1208)		HEL, p. 160, f 811
I	LEIBNIZ (C.c. **II**, nos. 903—906)		HEL, p. 122, f 619
VI	LEIBNIZ (C.c. **II**, nos. 1469—1470, 1475)		HEL, p. 160, f 811
25, 26 29 X	LEIBNIZ (C.c. **II**, nos. 1089—1092)	G 1855, pp. 117/127; LBG, pp. 147/156	C, pp. 62/83; HEL, pp. 118/123
11 XI	LEIBNIZ (C.c. **II**, no. 1120)	G 1848, pp. 32/40; G 1855, pp. 132/139; LBG, pp. 161/167	C, pp. 93/102; HEL, pp. 123/124
21 XI	LEIBNIZ: → 27 XI		
22 XI	LEIBNIZ (C.c. **II**, no. 1125)	G 1848, pp. 46/48	C, pp. 111/113; HEL, p. 160
27 XI	LEIBNIZ (C.c. **II**, no. 1131)	G 1848, pp. 41/45 (wrongly dated 21 XI; → HW)	C, pp. 104/109; HEL, p. 125; HW, pp. 278/279, f 3
14 XII	LEIBNIZ (C.c. **II**, no. 1157)		HEL, p. 160, f 811
22 XII	LEIBNIZ (C.c. **II**, nos. 1165—1166)		HEL, p. 125, f 624

Date	Letter or Manuscript	Publications	Comments
28 XII	LEIBNIZ to OLDENBURG	LBG, pp. 143/147; LMG **I**, pp. 83/87; NCT **I**, pp. 396/403; WO **III**, pp. 620/622	HEL, p. 114 *et passim*
1676 I	LEIBNIZ (C.c. **II**, no. 1277)		HEL, p. 160, f 811
27 V	LEIBNIZ (C.c. **II**, no. 1428)		HEL, p. 155, f 769
V	LEIBNIZ (C.c. **II**, no. 1430)		HEL, p. 155, f 769
23 VI	NEWTON to OLDENBURG (for LEIBNIZ and TSCHIRNHAUS) [*Epistola Prior*] (*Cf.* 5 VIII 1676)	NCT **II**, pp. 20/47; WO **III**, pp. 622/629	HEL, pp. 146/151
VI	LEIBNIZ (C.c. **II**, nos. 1456, 1461 to 1462, 1475)		HEL, p. 155, f 769
VII	LEIBNIZ (C.c. **II**, no. 1483)	G 1848, pp. 51/54; LBG, pp. 201/203	C, pp. 118/122; HEL, p. 158
VII	LEIBNIZ (C.c. **II**, no. 1485)		HEL, p. 155, f 769
5 VIII	OLDENBURG to LEIBNIZ (copy of 23 VI NEWTON to OLDENBURG) [*Epistola Prior*]	LBG, pp. 179/192; LMG **I**, pp. 100/113	HEL, pp. 146/151
27 VIII	LEIBNIZ to OLDENBURG (for NEWTON)	LBG, pp. 193/200; LMG **I**, pp. 114/122; NCT **II**, pp. 57/75; WO **III**, pp. 629/633	HEL, pp. 151/162
3 XI	NEWTON to OLDENBURG (covering letter to the „Epistola Posterior")	NCT **II**, p. 110	HEL, p. 178
3 XI	NEWTON to OLDENBURG (for LEIBNIZ) [*Epistola Posterior*]	LBG, pp. 203/225; LMG, **I** pp. 122/147; NCT **II**, pp. 110/161; WO **III**, pp. 634/645	HEL, pp. 167/181
5 XI	NEWTON to OLDENBURG	NCT **II**, pp. 162/164	
1677 V/VI ?	LEIBNIZ (Marginal Notes to the "Epistola Posterior")	LBG, p. 224 LMG **I**, pp. 128/129, 145/146; NCT **II**, pp. 209/212	

Date	Letter or Manuscript	Publications	Comments
1 VII*	LEIBNIZ to OLDENBURG (for NEWTON)	LBG, pp. 240/248; LMG **I**, pp. 154/162; NCT **II**, pp. 212/231; WO **III**, pp. 648/651	HEL, pp. 179/180
22 VII	LEIBNIZ to OLDENBURG (for NEWTON)	LBG, pp. 248/249; LMG **I**, pp. 162/163; NCT **II**, pp. 231/234; WO **III**, p. 652	HEL, pp. 118, 179, 192
19 VIII	OLDENBURG to LEIBNIZ	LBG, pp. 253/255; LMG **I**, pp. 166/168; NCT **II**, pp. 235/237	HEL, pp. 179, 192

* Wrongly dated in NCT **II**; see HEL, p. 225.

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