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History

The myth of Leibniz's proof of the fundamental theorem of calculus

A paper by Leibniz from 1693 is very often cited as containing his proof of the fundamental theorem of calculus. However, argues Viktor Blåsjö in this article, when read in its proper context it becomes clear that Leibniz's argument is not at all a proof of this theorem but rather a recourse for the cases where the theorem is of no use.

What was Leibniz's take on the fundamental theorem of calculus? He was one of the creators of the field after all, so one is naturally curious. But if you go to the library to find the answer to this question you will be sold a bait-and-switch. You will be referred to his 1693 article [15], supposedly the only place where Leibniz explicitly stated and proved the fundamental theorem of calculus in print. The passage in question is reproduced in full in English translation in Struik [26, pp. 282–284], Calinger [4, pp. 354–356], Laubenbacher and Pengelley [13, pp. 133–135], discussed in full detail in Cooke [5, pp. 470–471], Hahn [11, pp. 125–128], Nitecki [25, pp. 292–293], Bressoud [3, pp. 101–102], Nauenberg [24], and cited in Katz [12, p. 529], Edwards, [8, p. 260], Volkert [27, p. 104], González-Velasco [9, p. 357], Grattan-Guinness [10, p. 55], Beyer [1, p. 163], et cetera, all on the supposition that this is Leibniz's proof of the fundamental theorem of calculus. (Leibniz's complete paper is available in German translations in [23] and [20], and a French translation in [22].) If you study the proof you will probably recognize it as a rather clunky way of saying $\int_a^b f(x)dx = F(b) - F(a)$ (where $F' = f$) in geometrical language. I shall argue that it is not. And this despite the fact that Leibniz clearly writes: "I shall now show that the general problem of quadratures can be reduced to the finding of a curve that has a given law of tangency" (p. 390). Today everybody reads this as shown in the box on this page.

Read through modern eyes in this manner, then, this looks like smoking-gun evidence that Leibniz is announcing his intention to prove the fundamental theorem. So it is not difficult to see how it came to be generally accepted as such in the literature. It is natural that scholars who know the centrality of the fundamental theorem of calculus in the modern conception of the field should go looking for its proof in Leibniz, and it is understandable that this passage would then catch their eyes. But I shall argue that this is an anachronistic reading that misses the point of the argument completely. When Leibniz's paper is understood in its historical context it becomes evident that it is meant to serve a different purpose.

The statement of Leibniz and its misleading translation into modern terms

The general problem of quadratures	The evaluation of a general integral $\int_a^b f(x)dx$
can be reduced to	can be reduced to
the finding of a curve	the finding of a function $F(x)$
that has a given law of tangency.	that satisfies $F'(x) = f(x)$.

Leibniz's calculus

Before delving into the forgotten historical context that explains what Leibniz is up to in his paper, we may ask ourselves: if this isn't it, then how *did* Leibniz think about the fundamental theorem of calculus? I believe that, if cornered to argue for this result, Leibniz would have argued essentially as follows.

$$\begin{aligned}\int_a^b y'dx &= \int_a^b \frac{dy}{dx}dx = \int_a^b dy \\ &= \text{sum of little changes in } y \text{ from } a \text{ to } b \\ &= \text{net change in } y \text{ from } a \text{ to } b \\ &= y(b) - y(a).\end{aligned}$$

For the other part of the theorem, just note from Figure 1 that if t increases by dt then

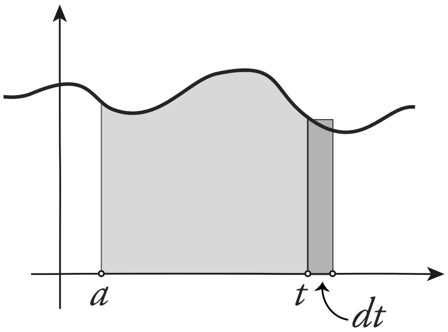

Figure 1 The integral $\int_a^t y\,dx$ and its differential.

the area $\int_a^t y\,dx$ increases by $y(t)dt$, whence

$$\frac{d\int_a^t y(x)dx}{dt} = \frac{y(t)dt}{dt} = y(t).$$

This mode of reasoning is very much in line with Leibniz's conceptions of integrals and differentials. Indeed, he would most likely not consider this a 'proof' of a 'fundamental theorem', but rather a somewhat tedious explication of the meaning of differentiation and integration. That is why he never put it in print. Instead he was satisfied with the casual statement that "as powers and roots in ordinary arithmetic, so for us sums and differences, or $\int$ and d, are reciprocal" [14, p. 297]. The comparison is an apt one not only procedurally but also foundationally: in neither case can there be a question of proof of the reciprocal relationship; rather it is built into the very meaning of the notions involved.

So much for the fundamental theorem, which, however, has nothing to do with the purpose of Leibniz's 1693 paper. To understand what Leibniz *did* intend in this paper, we must first understand its context. In the seventeenth century, Euclid's *Elements* was still the gold standard of mathematical rigour and method. One conspicuous aspect of this work is that Euclid speaks only of Figures he can *construct* using ruler and compasses. The scope of Euclid's construction tools was soo found too restrictive, but his emphasis on constructions was retained.

Descartes's construction method

The Euclidean requirement of construction as a prerequisite for knowledge was taken very seriously by Descartes, who was to have a great influence on Leibniz. Descartes taught the world coordinate geometry and the identification of curves with equations in his *La Géométrie* of 1637. In connection with this he also argued that the scope of mathematics should be extended to include all algebraic curves — to which his new method was especially suited — as opposed to being limited to the lines and circles of Euclid's *Elements* and the handful more complex curves studied in antiquity. However, Descartes did not present this as a radically new way of doing geometry, different in principle from that of Euclid. Rather he argued at great length that his method was really nothing but the Euclidean programme brought to its logical conclusion. In particular, he accepted curves represented by algebraic equations as legitimate mathematical objects only after he had found a way of constructing them in a Euclidean spirit.

Descartes's criterion for an acceptable construction is the following:

"To treat all the curves I mean to introduce here [i.e., all algebraic curves], only one additional assumption [beyond ruler and compasses] is necessary, namely, [that] two or more lines can be moved, one [by] the other, determining by their intersection other curves. This seems to me in no way more difficult [than the classical constructions]." [7, p. 43]

The key phrase is "one by the other": Descartes has no objections to assemblages of curves pushing one another in whatever fashion as long as all the motions are ultimately generated by one and only one primitive motion. You can build a curve tracing machine as intricate as you like as long as one single point needs to be moved to operate it. This single-motion criterion is the key to Descartes's division of curves into 'geometrical' (i.e., exact) and 'mechanical' (i.e., not susceptible to mathematical rigour).

Figure 2 shows an example of Descartes's construction method. It can be adapted to generate algebraic curves of higher and higher degree. For example, it is quite easy to see that replacing the line KNC by a circle produces a conchoid (Figure 3). And so it continues: once e.g. the conchoid has been generated it can be taken in place of the starting curve KNC to generate an even more complex curve, and so on.

These curve-tracing methods are what made algebraic curves legitimate geometry to Descartes. And they were so not in the sense of incidental or half-hearted attempts at justifying his new mathematics to obstinate colleagues stuck in old ways of thinking. Rather, these considerations formed the basis for his mathematical researches from the very beginning. Already in 1619, *before* he had the idea of a correspondence between a curve and an equation, Descartes was concerned with "new compasses, which I con-

Figure 2 Descartes's curve tracing method [6, p.321]. The triangle KNL moves vertically along the axis $ABLK$. Attached to it at L is a ruler, which is also constrained by the peg fixed at G. Therefore the ruler makes a mostly rotational motion as the triangle moves upwards. The intersection C of the ruler and the extension of KN defines the traced curve, in this case a hyperbola.

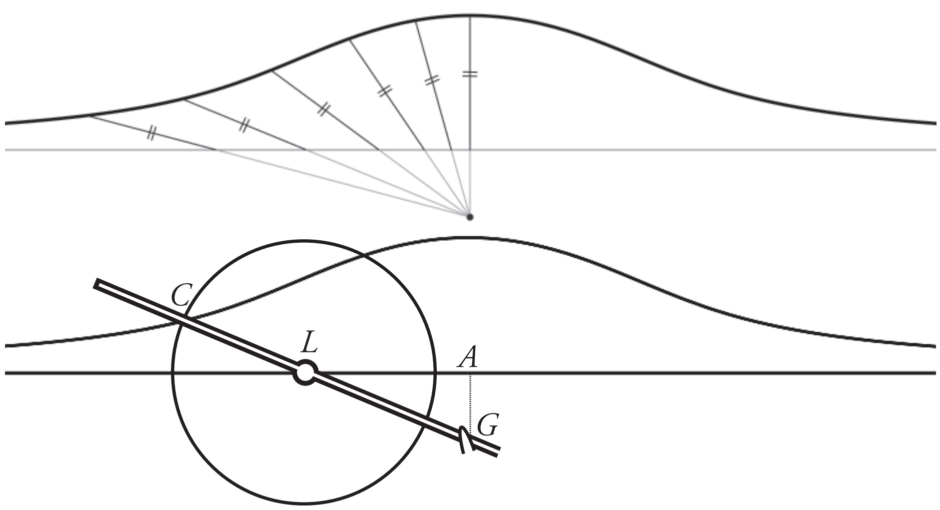

Figure 3 Top: The defining property of the conchoid, a famous algebraic curve studied in antiquity. Bottom: Construction of the conchoid using Descartes's method of Figure 2 with a circle in place of the line KNC.

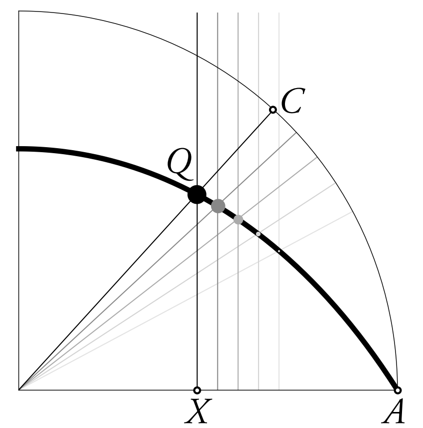

Figure 4 The quadratrix. C moves along the arc of a circle and X along its radius. Both points start at A and move at uniform speed in such a way as to reach the vertical axis at the same time, i.e., $\frac{d}{dt}AC=\frac{\pi}{2}\left(\frac{d}{dt}AX\right)$. The intersection Q generates the quadratrix.

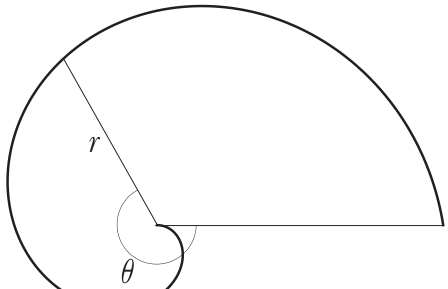

Figure 5 The Archimedean spiral $r=\theta$. The radial motion must be exactly coordinated with the rotational motion — a constructively impossible task, according to Descartes.

sider to be no less certain and geometrical than the usual compasses by which circles are traced" (quoted from [2, p. 232]). The key criterion for these 'new compasses', according to Descartes, was that they should trace curves 'from one single motion', contrary to the 'imaginary' curves traced by 'separate motions not subordinate to one another', such as the quadratrix (Figure 4) or the Archimedean spiral (Figure 5). The coordination of motions in both of these constructions involve π, which, since π is transcendental, is non-constructible (and hence unknowable) by Euclidean and Cartesian standards. As Descartes puts it in the *Géométrie*:

"The spiral, the quadratrix, and similar curves ... are not among those curves that I think should be included here, since they must be conceived of as described by two separate movements whose relation does not admit of exact determination, [...] since the ratios between straight and curved lines are not known, and I believe cannot be discovered by human minds, and therefore no conclusion based upon such ratios can be accepted as rigorous and exact." [7, pp. 44, 91]

By the time he published his *Géométrie*, Descartes had become convinced that his single-motion criterion included all algebraic curves (i.e., curves with polynomial equation of any degree), and nothing else. Convincing his readers of this — and thereby justifying the new algebraic methods in terms of the standards of classical, construction-based geometry — is one of the dominant themes of the *Géométrie*. (This is one of the main points of Bos [2], the definitive study of Descartes's geometry.)

Leibniz's construction method

These considerations form the direct background of Leibniz's 1693 article. He believed, as firmly as Descartes, that constructions are the bedrock of geometrical rigour. That is why he offered his own single-motion construction method, which can produce not only any algebraic curve but in fact any curve described by a differential equation of the form $dy/dx = f(x)$, where $f(x)$ can be any previously constructed function, just as the line KNC in Descartes's construction can be replaced by any previously constructed curve. This construction is what Leibniz's paper is all about.

Leibniz's construction goes as follows (Figure 6). The plane Π is a horizontal surface, say a table. On it is placed a weight at $C = (x, y)$ attached to a string TC. If we move the free end T of the string along the edge ABT of the table, the curve $(C)C$ generated by the moving weight would be the ordinary tractrix. But we shall modify this situation by having part of the string hang over the edge of the table. This end also has a weight attached to it, G, which ensures that it hangs straight down along the vertical plane Θ, until it hits the edge $E(E)$ protruding from this plane. Thus the fixed string length is $CT + TE$, and the length of the part TE hanging below the table is determined by the curve $E(E)$, which catches the weight at a point vertically below T. In fact, the length of TE is a funtion of the x-coordinate of the weight at C, for as C moves it pushes the 'ruler' HR and thereby the vertical plane Θ ahead of it, so that $E(E)$ is effectively the graph of a function with $RT = x$ as input and TE as output. The curve $(C)C$ is traced as T is moved along the edge of the table away from A. The motion of T thus inflicts two separate motions on the plane Θ: one in the y-direction resulting directly from the motion of T, and one along the x-direction resulting from the motion of C.

In this way we can generate a curve for which the length TC of its tangent is any given function of its x-coordinate. For if we seek a curve $C(C)$ for which $TC = \phi(x)$, say, then we can always choose the curve $E(E)$ so that TE is the total string length minus $\phi(x)$, which leaves just the required amount of string for the tangent TC. Thus if we write a for the total string length CTE, the required curve $E(E)$ is simply the graph of the function $a - \phi(x)$ plotted in the plane Θ with RT as x-axis and RL as y-axis.

Alternatively, we can generate a curve with a given slope $dy/dx = TB/BC$. This reduces to the above problem since $TC = \sqrt{TB^2 + BC^2}$ is a simple algebraic function of TB and BC. Thus if we want to generate the curve $C(C)$ with the given slope $dy/dx = f(x)$, we note that in this case $BC = x$ and $TB = xf(x)$, so that $TC = x\sqrt{f(x)^2 + 1}$. Once we have this expression for TC we can complete the construction of $E(E)$ as above.

In either case, then, since $\phi(x)$ or $f(x)$ are given, it takes only 'ordinary' Cartesian geometry to construct the required curve $E(E)$

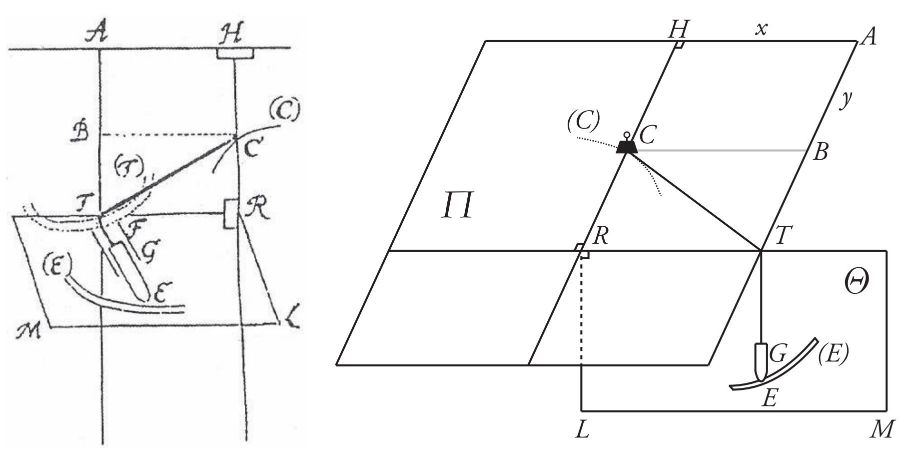

Figure 6 Leibniz's tractional-motion device for constructing the solution curve $C(C)$ of any inverse tangent problem. From [15], figure 3 (left), and my reproduction (right).

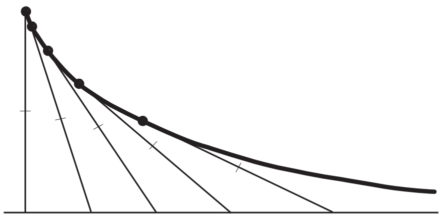

Figure 7 The tractrix is the curve traced by a weight dragged along a horizontal surface by a string whose other end moves along a straight line.

that will enable the curve $C(C)$ with the desired property to be traced. In particular, Leibniz's construction gives the solution to $dy/dx = f(x)$, where $f(x)$ is any previously constructed curve, while assuming nothing more than Cartesian geometry and a single-motion tracing procedure. In this way he enlarged the domain of constructible curves vastly beyond the algebraic curves admitted by Descartes, while still adhering very strictly to Descartes's requirement of single-motion tracing and to the Euclidean–Cartesian construction framework generally.

Construction of quadratures

Such was the purpose of Leibniz's paper. The confusion regarding the fundamental theorem arises from Leibniz's application to the problem of the construction of quadratures, i.e., the problem of constructing a line segment whose length equals a given area, or integral. This is quite clearly the guiding idea of the whole paper, whose title promises "a general construction of all quadratures by motion". In other words, Leibniz wants to clarify that his construction not only solves any differential equation $dy/dx = f(x)$ but also any integral $\int_a^b f(x)dx$. This problem readily reduces to the above as follows (Figure 8). Let $AF = x$ and let $f(x) = FH$ be the function whose integral is to be constructed. As above, construct a curve $C(C)$ such that its slope $dy/dx = TB/BC$ always equals $f(x)$. Then it follows that $FC = y = \int f(x)dx = AFHA$, so the quadrature has been constructed as a line segment, as required.

Since the tractional construction itself is prima facie concerned with constructing curves with given tangent properties, a casual reader of Leibniz's paper might have missed that it can also be used to find a line segment equal to a given integral had Leibniz not taken the trouble to spell out this application specifically and even note it in the title of the paper. This construction of quadratures was a major problem at the time, quite apart from differential equations, so it was certainly worth highlighting.

Leibniz's statement reevaluated

It is in the course of this explanation that we encounter Leibniz's sentence quoted above that seemed to be a statement of the fundamental theorem: "I shall now show that the general problem of quadratures can be reduced to the finding of a curve that has a given law of tangency." Now that we understand its context we see that to Leibniz this is a lemma linking the problem of quadratures to the tractional construction. It is not a fundamental theorem telling you to find an antiderivative F whenever you seek an integral $\int f dx$. Rather it is a specification of how the tractional motion needs to be set up to produce the values of $\int f dx$ as the y-coordinates of the tractional curve $C(C)$.

It is true that Leibniz's argument here concerns the relation between the differential equation $dy/dx = f(x)$ and the integral $\int_a^b f(x)dx$, and as such, to be sure, it is closely related to the fundamental theorem of calculus. But Leibniz's point is a much more specific one, and one very much specifically tailored to the setup of his tractional construction. It would be a big mistake, therefore, to forget about the context of the tractional construction and cut out the few lines relating to the fundamental theorem and study them as if they were meant as a proof of this general theorem. Yet this is precisely the mistake that occurs so often in the historical literature.

Leibniz would certainly consider it madness to apply his construction to an integral $\int f(x)dx$ for which an explicit antiderivative $F(x)$ can be found. Indeed, Leibniz says precisely this in a letter: "One cannot determine by this construction whether the sought quadrature can not also be carried out by common geometry; when this is possible one does not need the extraordinary route." [17, p. 694] In such cases he would simply go straight to $F(x)$, as he had done many times in print already before his 1693 paper.

Cases where $F(x)$ is algebraic had long been done and dusted, and logarithmic and trigonometric functions were also becoming common currency at this time. Certainly Leibniz would not spill ink in his 1693 paper on proving the fundamental theorem for use on such trivial cases.

The problem that interested him was integrals such as $\int \sqrt{1+x^4}dx$, or the corresponding differential equation $dy = \sqrt{1+x^4}dx$. Indeed, whenever Leibniz refers back to his paper it is certainly never with reference to the fundamental theorem, but rather always as "my general construction of quadratures by traction" [18, p. 127], i.e., as showing that the tractional device "serves to construct all quadratures by an exact and regular motion" [16, p. 665]. Again, Leibniz [19, p. 157] explains that "I wished for the tractional method to be applied to the inversions of tangents [i.e., solving differential equations] rather than to quadratures where we already have [a method, namely finding $F(x)$]."

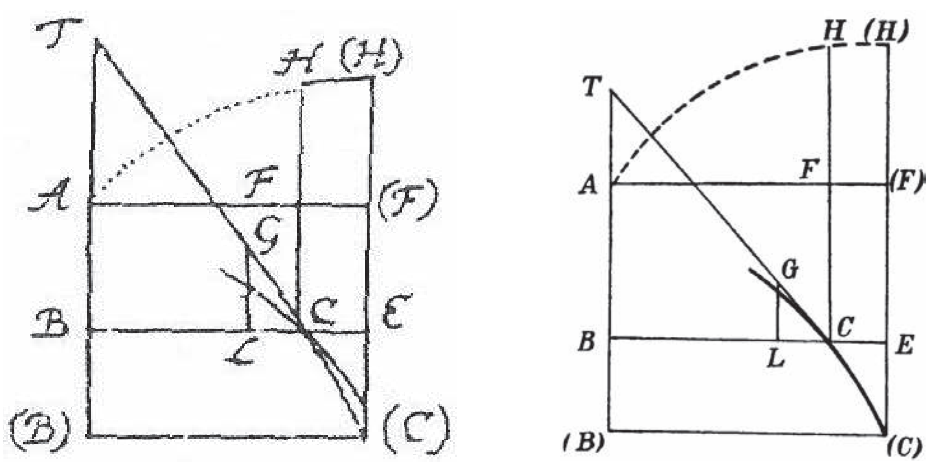

Figure 8 Leibniz's reduction of quadratures to rectifications. From [15], Figure 2, and as reproduced in [20, p. 31].

Conclusion

So, in conclusion, the irony of the story is that what is commonly referred to as Leibniz's proof of the fundamental theorem of calculus is actually his strategy for what to do when the theorem is of no use (in that one cannot find $F(x)$).

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