Available online at www.sciencedirect.com

ScienceDirect

Historia Mathematica 44 (2017) 134–149

HISTORIA MATHEMATICA

www.elsevier.com/locate/yhmat

On what has been called Leibniz's rigorous foundation of infinitesimal geometry by means of Riemannian sums

Viktor Blåsjö

Mathematical Institute, Utrecht University, Postbus 80.010, 3508TA Utrecht, The Netherlands

Available online 14 April 2017

Abstract

A number of scholars have recently maintained that a theorem in an unpublished treatise by Leibniz written in 1675 establishes a rigorous foundation for the infinitesimal calculus. I argue that this is a misinterpretation.

Zusammenfassung

Eine Reihe von Historikern haben vor kurzem behauptet, dass ein Satz in einer unveröffentlichten Abhandlung von Leibniz, die 1675 geschrieben wurde, eine strenge Grundlage für die Infinitesimalrechnung bildet. Ich behaupte, dass dies eine Fehlinterpretation ist.

MSC: 01A45

Keywords: Leibniz; Foundations of the calculus; Integration; Riemann sum

According to what is becoming a standard view among recent Leibniz scholars, an early manuscript by Leibniz, "published in its entirety only very recently," has "radically changed our views on the Leibnizian foundations of the calculus" (Rabouin, 2015, pp. 348–349). According to Knobloch:

> In 1675 ... Leibniz laid the rigorous foundation of the theory of infinitely small and infinite quantities ... In modern terms: Leibniz demonstrated the integrability of a huge class of functions by means of Riemannian sums. (Knobloch, 2002, pp. 59, 63)

Arthur quotes this assessment with approval, and elaborates:

E-mail address: V.N.E.Blasjo@uu.nl.

http://dx.doi.org/10.1016/j.hm.2017.03.007
0315-0860/

> Leibniz's method, in fact, is extremely general and rigorous; the same construction of elementary and complementary rectangles could be constructed for any curve whatsoever satisfying the three conditions ... continuity, no point of inflection, no point with a vertical tangent. (Arthur, 2008, pp. 24, 21)

Rabouin too agrees enthusiastically:

> We now possess crucial evidence that Leibniz did indeed *demonstrate* ... the equivalence between proofs using infinitesimal methods and proofs using finite quantities ... More than that, the general context of this translation was that of a "rigorous" foundation for the "method of indivisibles" (Leibniz's own terms!). (Rabouin, 2015, p. 364)

Levey is equally convinced:

> The demonstration of Prop. 6 articulates a general technique for finding the quadrature of any continuous curve that contains no point of inflection and no point with a vertical tangent. ... What Leibniz has demonstrated, then, is the integrability of a "huge class of functions." [Levey is quoting Knobloch] ... It goes without saying that his technical accomplishments in quadratures far outstrip the original reaches of the method of exhaustion; the technique of Riemannian integration by itself is an enormous advance, and for Leibniz it is not even particularly a showpiece of [the work in question]. (Levey, 2008, pp. 116, 119)

This interpretation is based on a single theorem: Proposition 6 of a treatise by Leibniz on the arithmetical quadrature of the circle.[1] The above authors all agree that the import of Proposition 6 is that it proves that a general curvilinear area can be approximated with arbitrary precision by rectangles, and that it hence establishes a fully rigorous foundation for integration in general. Let us call this Proposition $6'$.

I shall argue that the $6'$ interpretation is misguided. I say that, first of all, Leibniz's Proposition 6 is about one specific integration formula, not integrability in general, and secondly, that Leibniz didn't think of it as a foundational innovation but as a rather pedantic and basically routine way of applying what is essentially the ancient Greek method of exhaustion.

1. General arguments

My interpretation has considerable prima facie credibility. For if Leibniz had conclusively established the infinitesimal calculus on a fully rigorous foundation already in his twenties, then why did he never publish or refer to this work ever again? He lived for another forty years and had many occasions to write on the foundations of the calculus in print and correspondence, yet he never pointed to this work as establishing the definitive foundations of the calculus.[2] The obvious conclusion would seem to be that this work is not a great foundational masterpiece at all, as is indeed my contention.

The proponents of the $6'$ interpretation address this issue only unconvincingly. Arthur writes off the accumulated evidence of the remaining forty years of Leibniz's life as having "conspired to produce the impression that Leibniz developed his calculus without much attention to its foundations. But this impression is entirely mistaken." (Arthur, 2008, p. 20) He offers no explanation as to how or why so much evidence would have come to conspire to such a supposedly deceptive appearance. Knobloch is similarly unconvincing:

[1] Leibniz (1993, pp. 28–33), Leibniz (2012, pp. 527–533). The treatise was not published until Knobloch's edition (Leibniz, 1993). It has since been included in the Akademie-Ausgabe of Leibniz's complete works (Leibniz, 2012), and translated into German (Leibniz, 2016) and French (Leibniz, 2004).

[2] Knobloch in Leibniz (1993, pp. 11–14) cites a number of Leibniz's later mentions of this work, none of which have anything to do with Proposition $6'$.

> Leibniz originally wanted to submit it to the French Academy of Sciences in order to become a member of this institution. Hence, there can be no doubt that he rated it very high. (Knobloch, 2002, p. 59)

What about the fact that he didn't submit it, nor publish it later, nor reproduce its key results in his extensive subsequent correspondence on the foundations of the calculus? Can there really be "no doubt" about the significance of the work in light of these things? Moreover, it is well known that Leibniz was desperate to fashion a career for himself in intellectual circles at this time. The fact that he wanted to submit his work to the French Academy could very well be a reflection of this desire more than an assessment of the quality of the work, so this in itself proves nothing.

In fact, later in life Leibniz explicitly dismissed this treatise as insignificant. Referring to this treatise and related investigations from this period, he wrote:

> I found the greater part of my theorems anticipated [by others]. However I did not mind this very much, since I saw that these things were perfectly easy to the veriest beginner who had been trained to use them, and because I perceived that there remained much higher matters, which however required a new kind of calculus. Thus I did not think that my Arithmetical Quadrature, although it was received by the French and English with great commendation, was worth being published, as I was loath to waste time over such trifles while the whole ocean was open to me.[3]

In my view, Leibniz is exactly right: this treatise, including its Proposition 6, is, compared to Leibniz's later work, a "waste of time" work of "trifles" "not worth being published." On the $6'$ interpretation, meanwhile, it is puzzling to say the least why Leibniz would speak in such terms of the only treatise containing the definitive and perfectly rigorous foundations for his calculus.

Proponents of the $6'$ interpretation support their accounts by direct quotations from Leibniz which at first sight might seem like unequivocal proof of their interpretation. For instance:

> Leibniz summarized the importance of Theorem 6 by saying: "Hence the method of indivisibles which finds the area of spaces by means of sums of lines can be regarded as proven." (Knobloch, 2002, pp. 66)

Knobloch thus makes Leibniz out to say that Proposition 6 (or $6'$) has conclusively "proved" the method of indivisibles once and for all, and that one can now go on using it knowing that it rests on a firm foundation. But let us consider a fuller translation including the preceding sentence:

> Therefore if anything can be demonstrated, for a sum of lines or the area of a space formed by steps, in such a way that it holds regardless of to what extent the space formed by steps is brought forth, or it holds all the more when the intervals of the approximating space formed by steps are of sufficiently small size, then it will also be true for the curvilinear [space], or the error, if any can be committed, will be smaller than any assignable error. Whence it will be permissible to use the method of indivisibles proceeding by spaces formed by steps or by sums of ordinates as strictly demonstrated.[4]

[3] From a note intended as an appendix for a letter to Jacob Bernoulli, April 1703. "... magnam partem meorum theorematum praeceptam vidi. Parum tamen movebar, cum obvia esse viderem semel his imbuto tironi animadverteremque superesse multo altiora, sed quae novo calculi genere indigerent. Unde Arithmeticam meam Quadraturam similiaque licet magno plausu Galli Anglique excepissent nec editione digna putabam, pertaesus haerere in minutis, dum se Oceanus quidem aperiret." (Leibniz, 1855, p. 73), translation quoted from Child (1920, p. 20).

[4] "Ergo si quid de summa linearum sive area spatii gradiformis ita demonstrari poterit, ut locum habeat utcunque producatur spatium gradiforme, sive ut tum maxime locum habeat, cum spatii gradiformis applicatarum intervalla quantum satis est exigua sunt, id etiam de mixtilineo verum erit, sive error si quis committi potest, erit minor quovis errore assignabili. Quare methodo indivisibilium quae per spatia gradiformia seu per summas ordinatarum procedit, ut severe demonstrata uti licebit." (Leibniz, 2012, p. 533). Translations are mine unless otherwise noted.

This makes the conditional nature of Leibniz's assertion clearer. What he really says is that *if* one devises a proof *for a specific theorem* which holds for approximations that can be made arbitrarily good, then *that one theorem* has been strictly demonstrated. Leibniz himself has done precisely this (for the specific result of Proposition 7, as we shall see below), but other uses of indivisibles or infinitesimals would have to be likewise demonstrated by a specifically tailored argument.

This agrees with my interpretation that Leibniz considers himself to be giving little more than a routine explication of the method of exhaustion. Note well that any proof using the Greek method of exhaustion is tailored to one specific result. It makes no sense to speak of a "proof" establishing the rigour of the method of exhaustion in general. It only makes sense to speak of individual instances of this method devised for individual propositions. This parallels precisely my interpretation of Leibniz's treatise. Proposition 6 is one more proposition proved in the style of the method of exhaustion; it's one more addition to the pile of such examples already extant in classical Greek geometry, which is why Leibniz didn't consider his proof to be particularly innovative as far as foundations are concerned. Indeed, it is well-known that Leibniz in his mature years often claimed that the methods of the infinitesimal calculus were not lacking in rigour since its arguments could in principle be translated into method-of-exhaustion proofs. This was always a very plausible claim, and in this light the 1675 treatise doesn't tell us anything new as regards general foundational matters. It only shows that Leibniz once had the patience to work out the details of such a proof in one particular case.

Another key quotation for the $6'$ interpretation is this marginal note by Leibniz on Proposition 6:

> In it, it is demonstrated in fastidious detail that the construction of certain rectilinear and polygonal step spaces can be pursued to such a degree that they differ from one another or from curves by a quantity smaller than any given, which is something that is most often [simply] assumed by other authors. Even though one can skip over it at first reading, it serves to lay the foundations for the whole method of indivisibles in the soundest possible way.[5]

This quotation seems to sum up the two main points of the Knobloch interpretation: Proposition 6 is really Proposition $6'$, and it is the foundation for infinitesimal methods *tout court*. But what is the "it" that is "the foundations for the whole method of indivisibles" in the last sentence? Syntactically it could be Proposition 6, but "it" could also mean the strategy of proving that the approximations involved can be made arbitrarily good. Read in this second way, this marginal note is perfectly consistent with my view that Proposition 6 is a particular, not a general, result. In other words, Proposition 6 is not "the foundations for the whole method of indivisibles" in and of itself, but rather an instance, or at best a model example, of the technique that is this foundation.

When Leibniz speaks, in this quotation, of "other authors" who have merely "assumed" what he is proving, Knobloch (2002, p. 61) interjects that "Archimedes is meant." In my view Leibniz means precisely the opposite. The reference is to 17th-century authors using infinitesimals in a loose way. As I read him, far from claiming to improve upon Archimedes, Leibniz is very consciously trying to deal with polygonal approximations using Archimedean principles. Indeed, the Greek method of exhaustion comes down precisely to showing that the approximation differs by less than any assignable magnitude from what is being approximated, which is exactly what Leibniz himself is doing. And Leibniz himself says that he is "giving

[5] "Prop. 6. est spinosissima in qua morose demonstratur certa quaedam spatia rectilinea gradiformia itemque polygona eousque continuari posse, ut inter se vel a curvis differant quantitate minore quav[is] data, quod ab aliis plerumque assumi solet. Praeteriri initio ejus lectio potest, servit tamen ad fundamenta totius Methodi indivisibilium firmissime jacienda." (Leibniz, 2012, p. 521). Translation quoted from Arthur (2008, pp. 20–21). Also translated in Knobloch (2002, pp. 61–62).

way ... to the received opinions" in his demonstration of Proposition 6.[6] That is to say, he is proving it in the manner of the ancient method of exhaustion, the undisputed standard of rigour for such proofs.

Before giving his proof of Proposition 6, Leibniz offers the following preliminary remark:

> The reading of this proposition can be omitted if one does not want the highest rigour in the to-be-demonstrated Proposition 7. And it will be better that it is passed over in the beginning and only read once the whole matter has been understood, lest its scrupulosity keeps the prematurely fatigued mind from the remaining, far more beautiful things. For it brings about only this: that two spaces, of which one passes into the other if one carries the inscribing to infinity, approach each other to a difference less than one assigned however you please, even when the number of inscriptions remains only finite. Even those who profess to produce strict demonstrations are for the most part in the habit of accepting this as generally admitted.[7]

On my interpretation we can again take Leibniz's own words at face value: the proof is "only" a boring and tiresome explication of a well-known strategy, applied to a specific result (Proposition 7). Right after giving the proof, Leibniz again continues in the same vein:

> I would have willingly preferred to omit this theorem [Proposition 6], because nothing is more alien to my mind than these scrupulous details of some authors which imply more ostentation than utility. For they consume time, so to speak, on certain ceremonies, include more troubles than ingenuity, and envelop the origin of inventions in blind night which is, as it seems to me, mostly more prominent than the inventions themselves. I do not deny, however, that it is in the interest of geometry to have the methods themselves and the principles of inventions as well as some more outstanding theorems rigorously demonstrated. Hence, I believed that I had to give way a bit to the received opinion.[8]

Thus: Leibniz attaches very little value to his proof, derides excessive focus on such so-called rigour, and says he is only including it to "give way to received opinions." This agrees perfectly with my interpretation of Proposition 6. Note that he even explicitly states that it may be worthwhile to prove *some* theorems (meaning Proposition 7 in this case) with full rigour, not proving the foundations for everything in one go, whatever that could mean. My reading is also highly credible since it agrees perfectly with Leibniz's subsequent mathematical works, where his emphasis on discovery and his rather nonchalant attitude toward tedious rigour are well known and all-pervasive. It seems plausible that Leibniz would be especially inclined to "give way to received opinions" in this very early treatise, which was quite clearly written for the purpose of impressing others and making a name for himself, and that his later works, where he could speak more independently as an established scholar, represent his more genuine attitudes.

On the $6'$ interpretation it is very difficult to explain why Leibniz would so persistently depreciate this supposedly groundbreaking theorem. Perhaps the only plausible way to try to explain away the first of

[6] See Note 8.

[7] "Hujus propositionis lectio omitti potest, si quis in demonstranda prop. 7. summum rigorem non desideret. Ac satius erit eam praeteriri ab initio, reque tota intellecta tum demum legi, ne ejus scrupulositas fatigatam immature mentem a reliquis, longe amoenioribus, absterreat. Hoc unum enim tantum conficit duo spatia, quorum unum in alterum desinit si in infinitum inscribendo progrediare; etiam numero inscriptionum manente finito tantum, ad differentiam assignata quavis minorem sibi appropinquare. Quod plerumque etiam illi sumere pro confesso solent, qui severas demonstrationes afferre profitentur." (Leibniz, 2012, p. 527). All but the last sentence of this passage is quoted and translated in Knobloch (2002, p. 62). I give my own translation.

[8] "Hac propositione supersedissem lubens, cum nihil sit magis alienum ab ingenio meo quam scrupulosae quorundam minutiae in quibus plus ostentationis est quam fructus, nam et tempus quibusdam velut caeremoniis consumunt, et plus laboris quam ingenii habent, et inventorum originem caeca nocte involvunt, quae mihi plerumque ipsis inventis videtur praestantior. Quoniam tamen non nego interesse Geometriae ut ipsae methodi ac principia inventorum tum vero theoremata quaedam praestantiora severe demonstrata habeantur, receptis opinionibus aliquid dandum esse putavi." (Leibniz, 2012, p. 533). Translation quoted from Knobloch (2002, p. 67).

the two above quotations would be to argue that Leibniz's advice to skip the proof on first reading might be an attempt to accommodate readers who do not appreciate the importance of foundational rigour as much as Leibniz himself. But the last quotation undermines even this possibility, since Leibniz clearly states that he included the proof contrary to his own preferences, to accommodate the tastes of others. On the 6′ interpretation one would have to take this as an insincere rhetorical device, meant to apologetically introduce a foundational discussion which Leibniz himself finds interesting but expects his readers to find overly dry. Considering Leibniz's later neglect of these issues I think such a non-literal reading is hard to maintain.

2. Mathematical details of Leibniz's Proposition 6

Let us now turn to the mathematical details. First I shall give a summary of Leibniz's argument in slightly modernised terms. Leibniz uses only geometrical language, but I shall explain his results in terms of coordinate-system and calculus terminology, which will make certain points clearer.

Leibniz's purpose is to prove a theorem which in modern terms amounts to a special case of integration by parts. If we start with the general integration by parts formula

$$\int_a^b g'(x)f(x)dx = [g(x)f(x)]_a^b - \int_a^b g(x)f'(x)dx$$

and take $g(x) = x$, $a = 0$, and $f(0) = 0$, we obtain

$$\int_0^b f(x)dx = bf(b) - \int_0^b xf'(x)dx$$

which we can rewrite as

$$2\left(\int_0^b f(x)dx - \frac{1}{2}bf(b)\right) = \int_0^b f(x) - xf'(x)dx \tag{P7}$$

This is in effect Leibniz's Proposition 7. To understand Leibniz's geometrical formulation of this result, we refer to Figure 1. Given is a primary curve $f(x)$. For every point C on this curve, draw its tangent and find its intercept T with the y-axis, the y-coordinate of which is easily seen to be $f(x) - xf'(x)$. Then draw the horizontal through this point and mark its intersection D with the vertical through C. The set of all points D define a new curve, $d(x) = f(x) - xf'(x)$. The area under this curve is precisely the right hand side of (P7). The left hand side, meanwhile, is twice the area shaded in Figure 2. This geometrical way of stating (P7) is Leibniz's Proposition 7.[9]

Let us now consider the proof of this proposition. It is clear to us by integration by parts that it holds, but this is not how Leibniz proved it. Instead he proceeds by dissecting the areas into segments in a suitable way. If the curve $f(x)$ is approximated by a polygonal path, the area of Figure 2 is made up of triangles

[9] This theorem is an instance of the so-called "transmutation" method that was a central technical tool in Leibniz's early calculus or proto-calculus. See, e.g., Hofmann (1949, pp. 32–36), Child (1920, 42–44). Leibniz indeed praises the generality and usefulness of this theorem in the scholium following Proposition 7. For our purposes we are not interested in Proposition 7 in its own right, only insofar as it is it relevant to the interpretation of Proposition 6.

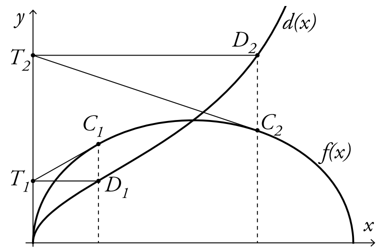

Figure 1. Leibniz's construction of a secondary curve $d(x)$ defined in terms of the tangents of a primary curve $f(x)$.

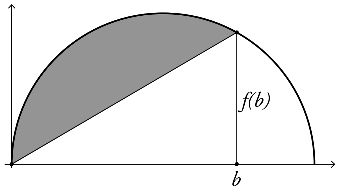

Figure 2. Geometrical interpretation of $\int_0^b f(x)dx - \frac{1}{2}bf(b)$.

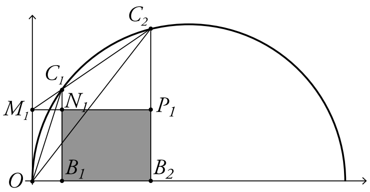

Figure 3. Rectangles approximating the area under $d(x)$ formed using secants.

such as OC_1C_2 of Figure 3. In the same figure, the secant C_1C_2 is extended to reach its y-intercept M_1, which defines the height of a rectangle (shown shaded in the figure) covering the horizontal space between C_1 and C_2. Rectangles defined in this way approximate the area under $d(x)$, because the secant line C_1C_2 approximates the tangent of the curve $f(x)$.

To reach the result of Proposition 7 it remains to show that these rectangles indeed have twice the area of the corresponding triangles. This is a simple matter of basic geometry:

$$2 \times \triangle OC_1C_2 = 2 \times \triangle OM_1C_2 - 2 \times \triangle OM_1C_1 = \square OM_1P_1B_2 - \square OM_1N_1B_1 = \square B_1N_1P_1B_2 \quad \text{(P1)}$$

Leibniz proves this in his Proposition 1. Thus Proposition 7 is now proved as far as the polygonal approximations are concerned.

It remains to infer the truth of the proposition for the actual curvilinear areas from these approximations. This is where Proposition 6, the all-important "rigour" proposition, comes in. Consider Figure 1 again. We want to estimate the area under $d(x)$ between the two verticals through C_1 and C_2. The biggest rectangle we could fit under the curve has a height going up to D_1. This rectangle is clearly smaller than, or "underestimates," the area. Conversely, a rectangle going all the way up to D_2 would overestimate the area. This will always be the case, because we are assuming that the curve $f(x)$ has the same concavity throughout, which means the tangents are always "turning in the same direction," so that $d(x)$ is strictly increasing.

But by choosing the points C_1 and C_2 very close together we can make the difference between the underestimate and overestimate as small as we please. And if we consider the whole area under $d(x)$ to be divided into rectangles in this manner the total difference between the sum of the overestimating rectangles and the underestimating rectangles will be obtained by multiplying their bases with the various differences in height between the lower and the higher rectangle. This sum of areas in turn can be overestimated by the total length along the x-axis of the entire interval we are considering times the maximum of all the differences in height between the lower and the higher rectangles at each subinterval. But by choosing the points of division C_1, C_2, etc. as closely together as needed, this maximum difference can be made as small as one likes, i.e., smaller than any given magnitude. Therefore the difference between the sum of the underestimating areas and the sum of the overestimating areas can be made as small as one likes. Therefore they must both approach the true area.

Furthermore, if we connect C_1 and C_2 and extend this secant to find its y-intercept M, and then make another approximating rectangle with height M, then this rectangle is greater than our previous underestimate and smaller than the overestimate. This again will always be the case because of the concavity assumption, for the secant approaches the two tangents at the endpoints when C_1 is brought toward C_2 or vice versa, and if the concavity remains the same this ensures that T_2 is higher than M_1 which in turn is higher than T_2. Therefore, rectangles whose height is determined by M_1, as in Figure 3, will always be in between the under- and overestimating rectangles determined by D_1 and D_2. And since the latter two could be made to converge to the true area, the M-rectangles must do so as well. Thus Proposition 7 holds for the curvilinear case since it holds for the polygonal case, and the polygonal approximation can be brought to within any specified error of the true area. This concludes the proof.

3. My view contrasted with the 6′ interpretation

Knobloch (2002), Arthur (2008), and Rabouin (2015) all reproduce Leibniz's elaborate proof of Proposition 6 in great detail, but treat it as if it were a proof of Proposition 6′. Indeed, neither Knobloch (2002) nor Arthur (2008) even mentions the result of Proposition 7, while Rabouin (2015) states it erroneously.[10] As far as the accounts of Knobloch (2002) and Arthur (2008) are concerned, it is a mystery why Leibniz's proof involves anything about secants and rectangles determined by the points M. Both authors reproduce all the details regarding these constructions in their account of the proof of Proposition 6, but since they have omitted Proposition 7 all of this serves no apparent purpose. They treat these constructions as if Leibniz's elaborately constructed M-rectangles were an innovation in terms of approximating the area as compared to the more immediate and obvious under- and overestimating triangles determined by the points D. But this makes little sense, since Leibniz's proof clearly shows that the D-rectangles could serve just as well as far as Proposition 6′ is concerned. Indeed, Leibniz discusses precisely this (that the limiting argument could be applied to the D-rectangles alone without any consideration of secants and M-rectangles) right after giving his proof of Proposition 6 (see Section 6). Knobloch (2002, p. 65), Arthur (2008, p. 24), and Rabouin (2015, p. 357) all note this and say something to the effect that Leibniz's method is more general. But none of them have given an explanation of why this generality is needed or desired, or why the simpler case would be insufficient.

Furthermore, if Proposition 6′ is what is at stake, and it is the area under $d(x)$ that is being approximated, what is the initial curve $f(x)$ doing in the proof? You would think a theorem about integrability of a more or less arbitrary function $d(x)$ would start with $d(x)$ as a given, not define it in terms of some auxiliary curve. Neither Knobloch (2002) nor Arthur (2008) offer an explanation for this.

In my reading of Proposition 6, everything associated with $f(x)$, secants, and M-rectangles has nothing to do with general rigour or a general foundation for the calculus. Instead, their only reason for being in the proof is that they are needed for the *specific* details of the demonstration of the *specific* result (P7) in Proposition 7. Leibniz is proving this theorem and this theorem only, and he is not pretending to do otherwise.

[10] Or at best misleadingly: "the area under the curve [$f(x)$] is half the area under the quadratrix [$d(x)$]" (Rabouin, 2015, p. 358), whereas, as we have seen, the proposition concerns the area in Figure 3 rather than the area under $f(x)$.

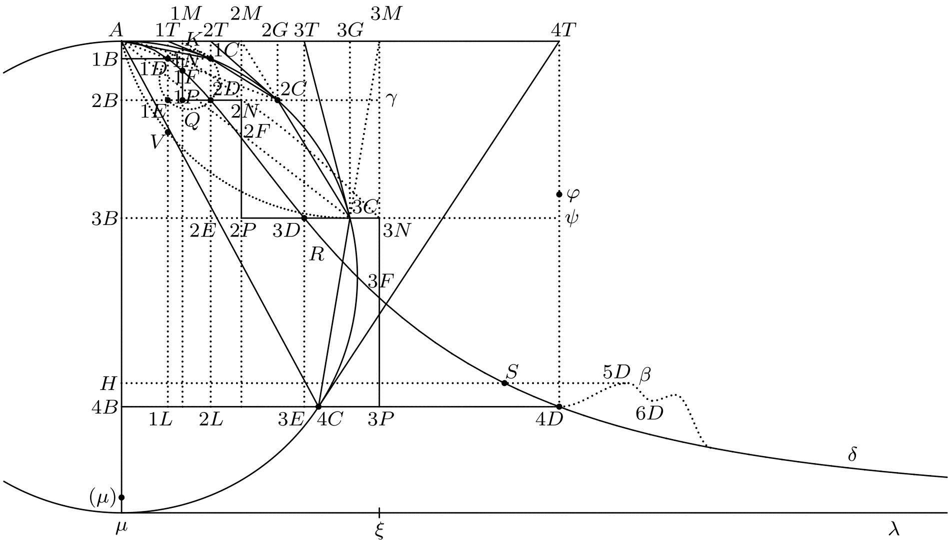

Figure 4. Leibniz's own figure for his proof of Proposition 6. From Leibniz (2012, p. 528).

4. Leibniz's own account of Proposition 6

We shall now consider in more detail the specifics of Leibniz's own account and formulations. Our purpose is twofold: first, this will verify our summary above, and second, it will help us analyse the precise conditions under which Leibniz's proof is valid—a point that plays a significant part in the $6'$ interpretation since Leibniz's remarks on this have been taken as testaments to the modernity and generality of his approach.

As we see in Leibniz's figure (Figure 4), our account above corresponds exactly to his setup. In our figures we rotated the diagram and introduced explicit coordinate axes and the notations $f(x)$ and $d(x)$, but the meanings of the various points denoted C, D, T, M, B, P, and N are precisely the same in our account and in Leibniz's.

On a given interval there are three key rectangles involved: those whose height are determined by T_1, M_1, T_2. The first is the underestimate discussed above (in Leibniz's figure: $1B2B1E1D$, $2B3B2E2D$, and $3B4B3E3D$), the second the actual estimate used in the theorem (in Leibniz's figure: $1B2B1P1N$, $2B3B2P2N$, and $3B4B3P3N$; in Figure 3: the shaded area), the third the overestimate (in Leibniz's figure: for example $3B4B4D\psi$). Leibniz calls the middle (M-based) type of rectangle an "elementary rectangle" and the difference between the overestimate and the underestimate a "complementary rectangle."

The key part of Leibniz's statement of Proposition 6 is:

> I say that, on the curves, points C between $1C$ and $4C$ and points D between $1D$ and $4D$ can be understood to be taken so close to one another and in such great number that the rectilinear step-shaped space $1N1B4B3P3N2P2N1P1N$ [i.e., the sum of the elementary rectangles] ... differs from the quadrilinear space $1D1B4B4D3D$ etc. $1D$ [i.e., the actual area under the curve $d(x)$] ... by a quantity less than any given.[11]

He specifies the following conditions for the theorem to hold:

[11] "ajo in curvis puncta C inter $1C$ et $4C$ et puncta D inter $1D$ et $4D$ tam sibi vicina tantoque numero assumta intelligi posse, ut spatium rectilineum gradiforme $1N1B4B3P3N2P2N1P1N$ ... ab ipso spatio Quadrilineo $1D1B4B4D3D$ etc. $1D$ ... differat quantitate minore quavis data." (Leibniz, 2012, pp. 528–529).

> It is, however, required that the curves, or at least the parts into which they are divided, be concave in the same direction, and without reversion points. Definition: I call reversion points those in which the ordinate coincides with the tangent, or in which the ordinate drawn from the axis touches the curve: such are, in the curve $1D2D$ etc. $4D5D6D$, the points $4D$. $5D$. $6D$. [where the ordinates $4B4D$, $H5D$, etc. are tangent to the curve].[12]

As Leibniz explains, if the curve has reversion points one can divide it into pieces (such as $1D \dots 4D$, $4D5D$, and $5D6D$) and still apply the theorem separately to each. The condition on reversion points really comes down to the requirement that the curves or functions $f(x)$ and $d(x)$ not have multiple y-values for a given x-value. In modern terms this assumed already under the notion of a function, so it would not need to be stated as a restriction of a theorem about integrability of functions. Nowadays this condition for a curve to be a function is sometimes called the "vertical line test." In Leibniz's orientation of the figure the line in question is horizontal rather than vertical, but I shall use the phraseology corresponding to modern conventions.

Leibniz's formulation of this condition is rather unsatisfactory from a modern point of view in that it seems to assume the existence of a tangent line at every point. If one of the reversion points $4D$, $5D$, $6D$ was a sharp corner there would be no tangent line there (at least according to the modern notion of tangent line), yet it is clear that Leibniz would still want to exclude such a curve, since the real problem is not the vertical tangent line per se but the multiple y-values for a given x-value.

The concavity condition is obviously intended as a restriction on the curve $f(x)$ (i.e., $1C2C3C4C$), not $d(x)$. In step (1) of his proof Leibniz states that M_1 lies between T_1 and T_2. This is so "by construction," he simply says, obviously relying implicitly on the assumption that $f(x)$ does not change concavity on this interval. The way he (rather loosely) states his theorem, the concavity condition applies also to $d(x)$, it would seem, but this serves no purpose in the proof.

Leibniz's proof proceeds in eight numbered steps. In the first three steps he is looking only at a specific subinterval $1B2B$. (1) Leibniz notes that, since M_1 lies between T_1 and T_2, the curve $d(x)$ must cut through the top of the elementary rectangle. His proof of this is that otherwise it would have to go around in the manner of paths such as $1DQ2D$ or $1DK2D$ indicated in Leibniz's figure, which would mean that it would have a reversion point, contrary to assumption. Here Leibniz is obviously assuming (without stating it) that the curve must be continuous, and arguably also that it has a tangent in every point.[13] (2) Leibniz considers the difference between the area under the curve $d(x)$ (on the given interval) and the area of the elementary rectangle. He claims that this is less than the area of the elementary rectangle. To prove this he splits the difference into two parts: the part of the area under $d(x)$ that sticks out above the elementary rectangle, and the part of the elementary rectangle that remains when its overlap with $d(x)$ is taken away. Clearly the difference between the area under the curve $d(x)$ and the area of the elementary rectangle consists of the first of these parts counted positively and the second of these parts counted negatively. (3) But each of these parts are contained in the complementary rectangle. Therefore the entire difference is less than the area of the elementary rectangle, as originally claimed.

Leibniz next uses this result to tackle the area under the curve on the whole interval. (4) Applying the same reasoning on the other subintervals ($2B3B$ etc.) and adding up, we see that the area under the curve $d(x)$ on the whole interval minus the area of the elementary rectangles is less than the area of the

---

[12] "Requiritur autem Curvas aut saltem partes in quas sunt sectae, esse ad easdem partes cavas, et carere punctis reversionum. Definitio: Puncta Reversionum voco, in quibus coincidunt ordinata et tangens, seu ex quibus ordinata ad axem ducta curvam tangit: talia sunt, in curva $1D2D$ etc. $4D5D6D$, puncta $4D$. $5D$. $6D$." (Leibniz, 2012, p. 529).

[13] Since reversion points are defined in terms of tangent lines, as discussed above. For suppose one considered, instead of the smooth paths $1DQ2D$ or $1DK2D$, a curve that switched directions abruptly in a non-differentiable point. Then one could go from D_1 to D_2 without crossing the top of the elementary rectangle, and without having a reversion point (at least according to the modern notion of tangent line), which is a possibility not admitted by Leibniz.

complementary rectangles. (5) The bases of the complementary rectangles add up to the total length of the interval in question, so the sum of the areas of the complementary rectangles is this length times the maximum of the heights of the complementary rectangles. (6) Combined with (4), this means that the latter estimate is greater than the area under the curve $d(x)$ on the whole interval minus the area of the elementary rectangles. (7) But the maximum of the heights of the complementary rectangles can be made smaller than any given magnitude, says Leibniz. This is asserted without proof. It follows that the same is true for the maximum times the interval length. (8) Combined with (6), this means that the difference between the area under the curve $d(x)$ and the area of the elementary rectangles can be made less than any given quantity. For "points on the curve can be taken at such small distances and such great number,"[14] we are told. Hence the proof is complete.

In steps (5)–(7), Leibniz is making an assumption essentially equivalent to the modern notion of uniform continuity. If we assume that we are integrating over a closed interval, and that $d(x)$ is continuous on this interval, then indeed it follows that the maximum of the heights of the complementary rectangles can be made as small as one likes. However, this would not be so if we integrated over an interval where $d(x)$ approached infinity, as would happen for instance in Leibniz's figure if we integrated all the way to μ instead of stropping at $4B$. One could argue that it is implied in Leibniz's argument that the interval of integration is closed and finite, and that $d(x)$ is everywhere continuous and finite on this interval. I believe this would be a fair characterisation of Leibniz's reasoning. But from the point of view of rigour it is problematic that he does not explicitly spell out the assumptions made and conditions imposed.[15]

5. Conditions on integrability

Let us sum up the conditions under which Leibniz's proof is valid. Leibniz himself imposes these restrictions:

(E1) The curves have a consistent concavity. (This is a restriction on $f(x)$ to ensure that any point M is between the corresponding T's.)
(E2) The curves do not have reversion points. (This is in effect a "vertical line test" ensuring that $f(x)$ and $d(x)$ have a unique y-value for any given x-value.)

He also makes a number of implicit assumptions, which can be characterised as follows:

(I1) The curve $f(x)$ is continuous and has a tangent line at every point. (This is assumed in the construction of $d(x)$.)
(I2) The curve $d(x)$ is continuous (assumed in step (1)) and has a tangent line at every point (assumed in the way (E2) is stated, and in step (1)).
(I3) The integration takes place over a closed, finite interval, on which the function $d(x)$ is defined and bounded, and hence uniformly continuous (i.e., if the interval of integration is divided into a finite number of subintervals smaller than δ then the Δd's on these subintervals are *all* less than some *one* upper bound $M(\delta)$ which goes to zero as δ goes to zero). (This is in essence what is assumed in steps (5)–(7).)

[14] "puncta in curva tam exiguo intervallo tantoque numero assumi possunt." Leibniz (2012, p. 532).

[15] To defend Leibniz, one may also try to argue that his explicitly stated condition that $d(x)$ cannot have a vertical tangent rules out the possibility of $d(x)$ growing to infinity, since it would then have such a tangent asymptotically. But I think it is clear that this is not Leibniz's intent. He clearly intended this condition in a sense corresponding to the modern "vertical line test" condition on the notion of a function, which is how he uses it. He does nothing to connect this to the issue of uniform continuity.

Leibniz seems to have considered it intuitively obvious that (I2) follows from (I1), and that (I3) holds. Of course Leibniz speaks of what I call $f(x)$ and $d(x)$ not as functions, but as curves. One may argue that his notion of a curve implies at least continuity and perhaps also the existence of its tangents. If this is granted we cannot fault Leibniz for not explicitly pointing out assumption (I1). However, (I2) remains problematic by modern standards of rigour. Since $d(x)$ is defined in terms of the tangent lines of $f(x)$, one cannot simply assume that $d(x)$ is a "curve" in this sense (i.e., (I2)); rather this must be derived as a consequence of (I1).

From the point of view of modern (i.e., 19th-century) integration theory, (I3) is perhaps the most essential. Considerable pitfalls might arise in these types of situations, and it is the central point of the 19th-century theory of integration to address them in a very precise manner (such as distinguishing continuity from uniform continuity). Leibniz does nothing to address such potential pitfalls but rather simply takes (I3) for granted. This makes it misleading to speak of a "rigorous" or "Riemannian" proof of the "integrability of a huge class of functions."[16] Leibniz's proof may be called "17th-century rigorous" but hardly "19th-century rigorous."

If one reads Leibniz as proving Proposition 6′, a general and rigorous result about the "integrability of a huge class of functions,"[17] then it is natural to construe Leibniz's (E1) and (E2) as a specification of this class, much like the precise conditions under which a theorem is valid is stated carefully in modern mathematics. Proponents of the 6′ interpretation tend to leave the reader with precisely such an impression.[18] But I don't think we should interpret (E1) and (E2) in this way. First of all, the curve being integrated is $d(x)$, and this curve is defined in terms of $f(x)$. So the very structure of the theorem precludes any possibility of stating it in the form one would expect in a modern treatment, i.e.: "Let $d(x)$ be a function satisfying the conditions ... Then $d(x)$ is integrable." Since Leibniz's entire approach starts with $f(x)$ instead of the function $d(x)$ actually being integrated, he cannot specify directly the "huge class of functions" $d(x)$ for which it holds; rather, if he wanted to prove that it applies for a certain class of functions $d(x)$, he would have to prove that these functions are indeed obtainable for some suitable choices of initial functions $f(x)$. But Leibniz does not do this.

But even this point aside, I believe it is clear that (E1) and (E2) are not really analogous to precise and rigorous specifications of conditions in the manner in which a modern theorem is stated. If in fact Leibniz really was trying to do this type of thing, one might reasonably expect him to include something along the lines of (I1)–(I3). Furthermore, (E1) and (E2) are obviously necessary conditions for Leibniz's construction to work and make sense even at an intuitive level, so one can just as well read them as commonsensical notes to assist the reader get the right general picture rather than as a precise and rigorous characterisation of the full class of functions for which the theorem holds. Thus for example (E2) is not so much about

[16] Knobloch (2002, pp. 59, 63), as quoted above.

[17] Knobloch (2002, p. 63).

[18] Knobloch (2002, p. 63), Arthur (2008, p. 24), Rabouin (2015, pp. 355–356, 364), and especially Scholtz (1933, 20, 40):

> For our purposes this Proposition 6 is the most important of all. For it is precisely here that Leibniz, with completely clear and distinct awareness, considers the problem of placing the conventional method of indivisibles on a new, entirely secure foundation. ... These considerations by Leibniz are very interesting because they constitute the first attempt at determining the particular conditions under which the integration can be safely carried out. (Für unsere Absichten ... ist eben dieser Satz 6 der wichtigste von allen. Denn gerade hier stellt sich Leibniz mit völlig klarem und deutlichem Bewußtsein die Aufgabe, die überkommene Indivisibelnmethode auf eine neue, ganz sichere Grundlage zu stellen. ... Diese Überlegungen Leibnizens sind sehr interessant, weil es die ersten Versuche sind, die besonderen Bedingungen festzustellen, under denen die Integration sicher ausführbar ist.)

precisely when the area under the curve $d(x)$ can be approximated with Riemann sums as it is about when it makes sense to speak of the area under the curve in the first place, even in a naive sense.

6. Leibniz's generalised Proposition 6

In his statement of Proposition 6, Leibniz includes not only the result and proof we discussed above (which we may call Proposition 6.1) but also the following generalisation, which we can call Proposition 6.2:

> And the same demonstration has a place for any other arbitrary mixtilinear and step-shaped space formed by continual application of a line to a certain axis. Hence the method of indivisibles, which finds areas of spaces by sums of lines, can be considered demonstrated.[19]

Following his proof of the main claim of Proposition 6, he returns to this generalised case:

> This proposition required a prolix demonstration, because ours differs not a little from the common method of indivisibles, at least in this case. If, however, in a case different from ours, any curve $1N2N3N$ passes through the same points $1N$ and $2N$ and $3N$ of the space formed by steps, as one is in the habit of doing in the common method of indivisibles, where curvilinear figures are only broken up into parallelograms, the demonstration would have been be far easier.[20]

Leibniz goes on to sketch the proof. Following our slightly anachronistic notation, we may call this curve $n(x)$. Leibniz claims that the area under this curve is underestimated by the same rectangles that were called elementary rectangles in the proof above. Clearly Leibniz is here assuming that $n(x)$ is an increasing (or monotone) function, though he does not say so.[21] We also have the same overestimating rectangle as before. So in this case elementary and underestimating rectangles coincide, and the difference between them and the overestimating rectangle can be taken as the new complementary rectangle. Thus is it obvious that the difference between the area under the curve $n(x)$ and the area of the elementary rectangles is smaller than the area of the complementary rectangles. In the above proof it was more elaborate to show this, as we saw in steps (1)–(3). Now we can instead go straight to (4) and complete the proof in the same way as above. Leibniz does precisely this, but in a self-contained manner.

Could Proposition 6.2 be equated with Proposition 6′? Clearly this is not what Knobloch (2002), Arthur (2008), and Rabouin (2015) had in mind, since their accounts are based on Leibniz's main proof, which they discuss in great detail. Furthermore, the very sloppy way in which Leibniz speaks of "any curve" $n(x)$—without even mentioning the obvious assumption that $n(x)$ is increasing (or monotone)—further strengthens my point at the end of Section 5 that Leibniz is not concerned with specifying precise conditions of validity for his theorems in anything like a modern sense.

It is true that Proposition 6.2 is quite general, but in what sense, if any, can it be considered "demonstration" of the "method of indivisibles"? Let us try to spell out its precise meaning. As Leibniz's conclusion of Proposition 6.2 shows, its implications can in turn be subdivided in two claims:

---

[19] "Et eadem demonstratio locum habet in quovis alio spatio mixtilineo et gradiformi continua rectarum ad quendam axem applicatione formatis. Adeoque methodus indivisibilium, quae per summas linearum invenit areas spatiorum, pro demonstrata haberi potest." (Leibniz, 2012, p. 529).

[20] "Haec propositio prolixiore indiguit demonstratione, quia non parum a communi indivisibilium methodo nostra in hoc quidem casu differt. Si vero, in casu alio a nostro, curva aliqua $1N2N3N$ per ipsa spatii gradiformis puncta, $1N$ et $2N$ et $3N$ transiisset, ut in communi methodo indivisibilium, ubi figurae curvilineae tantum in parallelogramma resolvuntur, fieri solet; longe facilior fuisset demonstratio." (Leibniz, 2012, p. 532).

[21] The function $d(x)$ was increasing as a consequence of the concavity of $f(x)$, but nothing forces $n(x)$ to have the same property.

> Therefore if anything can be demonstrated, for a sum of lines or the area of a space formed by steps, in such a way that [6.2.1:] it holds regardless of to what extent the space formed by steps is brought forth, or [6.2.2:] it holds all the more when the intervals of the approximating space formed by steps are of sufficiently small size, then it will also be true for the curvilinear [space], or the error, if any can be committed, will be smaller than any assignable error.[22]

Proposition 6.2.1 seems to say the following. Let Ω be the area under a curve satisfying the conditions E2, I2, I3. Consider an arbitrary subdivision of the integration interval, and let $\Omega_\sqcap$ be the corresponding set of underestimating rectangles. Then if I can prove that $\Omega_\sqcap$ has a certain area, I can use Proposition 6.2.1 to infer that Ω also has this area. Indeed Leibniz has proved this, but it is clearly a very limited theorem since the approximating rectangles $\Omega_\sqcap$ will not be exactly equal to Ω in any but the most basic cases.

Hence the need for Proposition 6.2.2, which seems to say the following. With Ω as above, let Ω_n denote a set of underestimating rectangles for this area corresponding to some subdivision of the integration interval, in such a way that (Ω_n) is a sequence of such sets for which the subdivisions become finer and finer as n increases. Then, if the area of Ω_n is $A(n)$, and $A(n) \to A$ as $n \to \infty$, Proposition 6.2.2 allows one to infer that Ω has area A.

A standard example of calculations of this type is the following. Let $\Omega = \int_0^1 x^2 dx$ and let Ω_n be determined by subdividing the interval into n equal parts. Then

$$\begin{aligned} A(n) &= \frac{1}{n}\left(0 + \left(\frac{1}{n}\right)^2 + \left(\frac{2}{n}\right)^2 + \left(\frac{3}{n}\right)^2 + \cdots + \left(\frac{n-1}{n}\right)^2\right) \\ &= \frac{1}{n^3}\left(1^2 + 2^2 + 3^2 + \cdots + (n-1)^2\right) \\ &= \frac{n(n-1)(2n-1)}{6n^3} \end{aligned}$$

Hence $\Omega = \int_0^1 x^2 dx = \lim_{n\to\infty} A(n) = 1/3$.

Leibniz's Proposition 6.2.2 might seem like a contribution to the foundations of such calculations insofar as it justifies the inference from the areas of Ω_n to the area of Ω. Of course this still leaves the main problem when carrying out such integrations, namely finding $A(n)$ in a useful form—ad hoc methods like the one used in the above example require much ingenuity for each case. But even putting this matter aside, Leibniz's Proposition 6.2.2 can still not be used as a justification for such integrations. This is because Leibniz's proof is concerned only with geometrical estimation of Ω by Ω_n, and not at all with any concurrent algebraic convergence of $A(n)$ to A. Note well that in the above example we found the limit of $A(n)$ by prescribing in advance what subdivisions we were dealing with. We had to choose a particularly simple subdivision to make $A(n)$ algebraically tractable. This means that Leibniz's theorem cannot help us in this case, because Leibniz's proof assumes complete freedom in the choice of the subdivision. It is true that Leibniz's proof shows that $\lim_{n\to\infty} A(n) = \Omega$ in a certain sense, but this in itself is a pointless near-tautology unless one can in fact determine $\lim_{n\to\infty} A(n)$. And there is no reason to think that the choice of subdivisions required by Leibniz's proof makes this feasible. Of course one could try to argue that a particular choice of subdivisions does indeed satisfy all the requirements in Leibniz's proof, but this will in effect amount to proving each result from scratch with a specifically tailored argument rather than relying on a general Proposition 6′—in accord with what I claimed above in Section 1.

22 As quoted above at note 4.

Perhaps more realistic is the use of Proposition 6.2.2 is to prove the equality of Ω with some other area, rather than to establish a numerical value or algebraic expression for its area. The simplest types of such theorems are based on Cavalieri's Principle that two areas are equal if all of their cross-sections are equal, which is perhaps what Leibniz has in mind when he speaks of "sums of lines." A simple example would be the equality of the area of a rectangle with that of a parallelogram with the same base and perpendicular height: if the cross-sections are taken parallel to the common base they will be equal line segments for both figures at every stage. Indeed an argument along Leibniz's lines can be used to make such arguments rigorous by justifying the inference that what holds for approximating rectangles must hold also for the figures themselves. Cavalieri's Principle is very limited in scope and falls well short of a general theory of integration, but even this most basic kind of integration is difficult to cast as an instance of Leibniz's Proposition 6.2.2 since this proposition is limited to underestimating rectangles of one area and does not specify how to relate two areas being approximated in this manner. Any moderately advanced theorem is likely to require the choice of approximating rectangles to be handled with case-specific ingenuity. Leibniz's own Proposition 7 is a case in point. Indeed, the individually tailored Proposition 6.1 is needed precisely because if one tried to prove Proposition 7 from Proposition 6.2.2 one cannot do anything useful with the underestimating rectangles to which 6.2.2 is restricted. This is why different approximating rectangles are needed, which can be linked with the target area as in (P1).

In sum, what I have called Proposition 6.2 can be said to indicate a general strategy for how to work with approximating rectangles in a rigorous way. It is in effect the classical method of exhaustion restricted to the case of area between a curve and an axis being estimated by vertical rectangles. Like the method of exhaustion, however, each instance of its use is going to require elaborate adaptations specifically tailored to the case at hand, just as Leibniz's Proposition 6.1 is specifically adapted to his Proposition 7.[23]

7. Conclusion

The $6'$ interpretation has proved attractive to many, but it suffers from major problems. First of all it asks us to believe that Leibniz developed, with exemplary rigour, a revolutionary account of the foundations of integration in his twenties, yet never published it, or referred to it, or built on it in his later work. On top of this, the $6'$ interpretation leaves major explanatory gaps when it comes to issues internal to the mathematical account, such as why Leibniz's proof of Proposition 6 involves two curves ($d(x)$ and $f(x)$), one constructed in terms of the other, if the theorem is about the integration of only one of them ($d(x)$), and why Leibniz uses secants and the points M to construct his approximating rectangles, when the same area could just as well be approximated more simply by rectangles determined by the points D. I have offered instead a reading showing a straightforward, if less exciting, way of avoiding these difficulties by simply accepting that Leibniz's Proposition 6 is quite mundane and that most of its details have nothing to do with rigour or a general theory of integration at all, but rather pertain only to one particular result (namely (P7)).

References

Arthur, Richard, 2008. Leery bedfellows: Newton and Leibniz on the status of infinitesimals. In: Goldenbaum, Ursula, Jesseph, Douglas (Eds.), Infinitesimal Differences: Controversies between Leibniz and his Contemporaries. de Gruyter, pp. 7–30.

[23] To be sure, Leibniz's Proposition 7 in itself is quite general, and can be usefully applied in a number of cases (which thus do not need to be proved individually since they are special cases of Proposition 7). Indeed, Leibniz was very happy with this proposition for this reason, as he says in its scholium. But the fact remains that it is one result, not a theorem on integration in general.

Child, J.M., 1920. The Early Mathematical Manuscripts of Leibniz. Open Court Publishing, Chicago and London.

Hofmann, J.E., 1949. Die Entwicklungsgeschichte der Leibnizschen Mathematik während des Aufenthaltes in Paris (1672–1676). Leibniz Verlag, München.

Knobloch, Eberhard, 2002. Leibniz's rigorous foundation of infinitesimal geometry by means of Riemannian sums. Synthese 133, 59–73.

Leibniz, Gottfried Wilhelm, 1855. Mathematische Schriften. Band II. Edited by C.I. Gerhardt. Verlag von H.W. Schmidt, Berlin.

Leibniz, Gottfried Wilhelm, 1993. De quadratura arithmetica circuli ellipseos et hyperbolae cujus corollarium est trigonometria sine tabulis, kritisch herausgegeben und kommentiert von Eberhard Knobloch, Göttingen, 1993. Abhandlungen der Akademie der Wissenschaften in Göttingen, Mathematisch-physikalische Klasse 3, 43.

Leibniz, Gottfried Wilhelm, 2004. Quadrature arithmétique du cercle, de l'ellipse et de l'hyperbole et la trigonométrie sans tables qui en est le corollaire. Edition of the Latin text by Knobloch. Notes and French translation by Marc Parmentier. Vrin, Mathesis.

Leibniz, Gottfried Wilhelm, 2012. Sämtliche Schriften und Briefe. Reihe VII: Mathematische Schriften. Band 6: Arithmetische Kreisquadratur. Academie Verlag, Hannover.

Leibniz, Gottfried Wilhelm, 2016. De quadratura arithmetica circuli ellipseos et hyperbolae cujus corollarium est trigonometria sine tabulis. Edition of the Latin text by Knobloch. German translation by Otto Hamborg. Springer-Verlag, Berlin, Heidelberg.

Levey, Samuel, 2008. Archimedes, infinitesimals, and the law of continuity: on Leibniz's fictionalism. In: Goldenbaum, Ursula, Jesseph, Douglas (Eds.), Infinitesimal Differences: Controversies between Leibniz and his Contemporaries. de Gruyter, pp. 107–133.

Rabouin, David, 2015. Leibniz's rigorous foundations of the method of indivisibles. In: Jullien, Vincent (Ed.), Seventeenth-Century Indivisibles Revisited. Birkhäuser, pp. 347–364.

Scholtz, Lucie, 1933. Die exakte Grundlegung der Infinitesimalrechnung bei Leibniz. Ph.D. dissertation, Philipps-Universität Marburg, defended 1933. Offprint by Verlagsanstalt Hans Kretschmer, Görlitz-Biesnitz, 1934.