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The rectification of quadratures as a central foundational problem for the early Leibnizian calculus

Viktor Blåsjö

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

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Abstract

Transcendental curves posed a foundational challenge for the early calculus, as they demanded an extension of traditional notions of geometrical rigour and method. One of the main early responses to this challenge was to strive for the reduction of quadratures to rectifications. I analyse the arguments given to justify this enterprise and propose a hypothesis as to their underlying rationale. I then go on to argue that these foundational concerns provided the true motivation for much ostensibly applied work in this period, using Leibniz's envelope paper of 1694 as a case study.
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Zusammenfassung

Transzendentale Kurven stellten eine grundlegende Herausforderung für die frühen Infinitesimalkalkül dar, da sie eine Erweiterung der traditionellen Vorstellungen von geometrischen Strenge und Verfahren gefordert haben. Eines der wichtigsten frühen Antworten auf diese Herausforderung war ein Streben nach die Reduktion von Quadraturen zu Rektifikationen. Ich analysiere die Argumente die dieses Unternehmen gerechtfertigt haben und schlage eine Hypothese im Hinblick auf ihre grundlegenden Prinzipien vor. Ich behaupte weiter, dass diese grundlegenden Fragenkomplexe die wahre Motivation für viel scheinbar angewandte Arbeit in dieser Zeit waren, und benutze dafür Leibniz' Enveloppeartikel von 1694 als Fallstudie.
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1. Introduction

In the early 1690s, when the Leibnizian calculus was in its infancy, a foundational problem long since forgotten was universally acknowledged to be of the greatest importance. This was

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

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the problem of reducing quadratures to rectifications. That is to say, in modern terms, when encountering an integral such as $\int \sqrt{1+x^4}\,dx$, which cannot be evaluated in closed algebraic form, the pioneers of the Leibnizian calculus preferred to express it in terms of the arc length of an auxiliary curve instead of leaving it as an area, i.e., in effect, to rewrite the integral in the form $\int \sqrt{1+(y')^2}\,dx$ for some algebraic curve $y(x)$ concocted solely for this purpose. They were fully aware that the opposite reduction (expressing an arc length as an integral) is the easy and natural one computationally from the point of view of the integral calculus. Nevertheless they insisted on reducing quadratures to rectifications for a variety of reasons to be discussed in detail below. But first a few words about the broader context.

The problem of rectification of quadratures was generally encountered in the setting of differential equations. Many differential equations that cannot be solved in closed algebraic form can be solved "by quadratures," i.e., the solution curve can be constructed given the assumption that areas of curvilinear figures can always be determined. In particular, any differential equation with separated variables, $f(x)dx = g(y)dy$, amounts to a recipe for constructing a solution curve "by quadratures," as indicated in Fig. 1. But the assumption that general curvilinear areas can always be determined was considered deeply unsatisfactory and was accepted as a last resort only. A method for rectifying quadratures would mean that this assumption could be replaced by the assumption that arc lengths of curvilinear figures can be determined, which was considered more reasonable. For this reason the rectification of quadratures was considered to be one of the most promising general methods for constructing curves known only by their differential equation.

In this way the rectification of quadratures formed one of the cornerstones of a broader complex of foundational concerns regarding the representation of transcendental curves. The proper means of representing transcendental curves was a—or, arguably, *the*—preeminent foundational problem for the early Leibnizian calculus. Transcendental curves and the quantities constructible with their aid were found to be indispensable in numerous branches of mathematics and physics, such as the brachistochrone in dynamics, the cate-

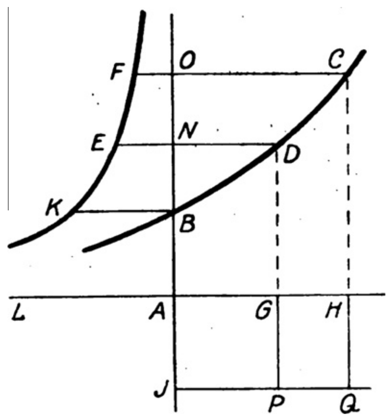

Figure 1. Solution "by quadratures" of the differential equation $adx = a^2dy/y$ in [Johann Bernoulli, 1692c, lecture 10] (figure from [Johann Bernoulli, 1914, 41]). The lines EN and PG are chosen so that the areas $KBNE$ (area under a^2/y) and $AJPG$ (area under a) are equal. Their intersection D is then a point on the sought curve. Bernoulli was well aware that the solution curve is what we would call an exponential function (he calls it "Logarithmica") but he evidently considered the geometrical construction more fundamental than a description in such terms (see Section 3.1 below for further discussion of this point).

nary in statics, the cycloidal path of the optimal pendulum clock in horology, the loxodrome in navigation, caustics in optics, arc lengths of ellipses in astronomy, and logarithms in computational mathematics. However, these curves were profoundly incompatible with the norms of mathematical rigour current at the time, as the very epithet "transcendental" (coined by Leibniz[1]) attests—the point being that these curves "transcend" the domain of algebra. In his epoch-making work *La Géométrie* of 1637, Descartes had taught the world how to represent curves by algebraic equations. With the lines, circles and conic sections of classical geometry being of degree one and two, Descartes's reconceptualisation of geometry to include algebraic equations of any degree was a way of subsuming and extending virtually all previous knowledge of geometry. This continuity with classical geometry lent credence to Descartes's claim that only curves that could be expressed by polynomial equations were susceptible to geometrical rigour. In the later half of the 17th century, however, even Descartes's extended notion of geometry was found to be too restrictive, as one curve after another that transcended its bounds proved vital to this era's revolutionary mathematisation of nature. With nothing like the clear-cut rigour of Greek and Cartesian geometry available to deal with their multitude and complexity, these new transcendental curves exerted a profound strain on the foundations of the subject. The variety of new techniques for characterising these curves—such as differential equations, infinite series, analytic expressions, and numerous mechanical and geometrical constructions—blurred the boundaries between known and unknown, while the fact that many of these curves were most naturally defined in physical terms left the foundations of geometry entangled with mechanics.

In a such period of foundational turmoil, mathematical considerations alone are insufficient to uniquely determine the path of progress. Extramathematical choices must necessarily play a part in directing research, whether they be philosophical, psychological, aesthetic, or otherwise. But these extramathematical considerations have a fleeting life span. Though once the torches that reveal the first contours of a terra incognita, they are swapped for swords as the new area is conquered, and altogether antiquated by the time the battlefront has pushed ahead toward new frontiers. Today the infinitesimal calculus is a pastoral idyll where we send our young to practice, but it looked very different to the first explorers to glimpse this land in the flickering light of philosophical torches, and the manner in which it was conquered was largely determined by the dragons they imagined themselves seeing there. The problem of rectification of quadratures was one of these foundational dragons whose imposing presence profoundly shaped the development of the calculus.

2. Why rectify quadratures?

The importance and value of reducing quadratures to rectifications is attested in both words and deeds by all the major figures involved the early Leibnizian calculus,[2] but the moti-

[1] Leibniz used this term in private manuscripts as early as 1673 (see [Knobloch, 2006]) and subsequently in print from his first calculus-related paper [Leibniz, 1682] onwards.

[2] To wit, Leibniz, Jacob and Johann Bernoulli, Huygens and l'Hôpital. I give a few general quotations to this effect here; below follow many more.

Leibniz [1691c]:

> I would also like to be able to reduce quadratures to the dimensions of curved lines, which I consider to be simpler. Have you perhaps considered this matter, sir?

vation for doing so is not completely unambiguous. The most common argument is that "the dimension of the line is simpler than that of an area," as Leibniz repeatedly stressed.[3] Leibniz even traced the pedigree of this principle back to Archimedes's reduction of the area of a circle

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Je souhaitterois aussi de pouvoir tousjours reduire les quadratures aux dimensions des lignes courbes, ce que je tiens plus simple. Avés vous peutestre pensé à ce point, Monsieur?

Huygens [1691]:

I would also like to be able to reduce the dimensions of unknown spaces to the measurement of some curved line ... but I think in most cases it will be very difficult.

J'aimerois bien aussi de pouvoir reduire les dimensions des espaces inconnus à la mesure de quelque ligne courbe, lors que ces deux quadratures n'ont point de lieu, mais je le crois le plus souvent tres difficile.

Leibniz [1693a]:

I would like completely general and short ways of reducing inverse tangent problems in any case at least to quadratures, and then the quadratures to the extension of curves into lines, since it is more natural to measure areas by lines than the other way around.

Ich mochte wündschen vollkommene allgemeine und kurze wege, die problemata Tangentium conversa allezeit wenigstens auff quadraturas zubringen, und dann die quadraturas auff extensiones curvarum in rectas, denn ja naturlicher ist spatia zu meßen per lineas, als contra.

Johann Bernoulli [1694d]:

I believe you are right to say that it is better to reduce quadratures to rectifications of curves, rather than the other way around.

Recte ut opinor mones quod praestat reducere quadraturas ad Rectificationes curvarum quam contra.

Leibniz evidently worked on this problem at least as early as April 1686, judging by his dating of a note accompanying a manuscript of his entitled "De reductione quadraturarum ad curvarum rectificationem" ("On the reduction of quadratures to the rectification of curves"). See [Bodemann, 1895, 297].

3 Leibniz [1693d]:

I would much prefer, for example, to reduce to quadratures to the rectification of curves, because the dimension of the line is simpler than that of an area.

Mais il y a des methodes que je souhaiterois bien d'avantage, par exemple de pouvoir reduire les quadratures aux rectifications des courbes, car la dimension de la ligne est plus simple que celle d'un espace.

Leibniz [1694a]:

It is better to reduce quadratures to the rectifications of curves than the other way around, as is commonly done. ... For certainly the dimension of a line is simpler than the dimension of a surface.

Praestat reducere Quadraturas ad Rectificationes Curvarum, quam contra, ut vulgo fieri solet. ... Nam simplicior utique est dimensio lineae quam dimensio superficiei.

The same point is expressed in Leibniz [1693b]; see Footnote 42 below.

to its circumference.[4] But elsewhere Leibniz emphasised instead that a rectification "enlightens the mind" more than a quadrature.[5] Then again in other cases rectifications seem to be preferred over quadratures for the sake of greater practical feasibility.[6] Altogether the diversity of arguments at first appears quite confusing.

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4 Leibniz [1691a]:

> I would like to be able to always reduce the dimensions of areas or spaces to the dimensions of lines, since they are simpler. And that is why Archimedes reduced the area of the circle to the circumference, and you [i.e., Huygens], Wallis and Heuraet reduces the area of the hyperbola to the arc of the parabola. It is easy to reduce arcs to areas, but the converse—that is the task, that is the toil. If you should come to facilitate this research some day, Sir, I would be delighted to benefit thereof.
>
> Je souhaitte de pouvoir tousjours reduire les dimensions des aires ou espaces, aux dimensions des lignes, comme plus simples. Et c'est pour cela qu'Archimede a reduit l'aire du cercle à la circomference, et vous[,] Mons. Wallis et Mons. Heuraet avés reduit l'aire de l'Hyperbole à la ligne de la parabole. Il est bien aisé de reduire les lignes aux aires, mais vice versa, hoc opus hic labor est. Si vous y voyés quelque jour, pour faciliter cette recherche, Monsieur, je seray bien aise d'en profiter.

5 Leibniz [1693f]:

> But among the geometrical constructions I prefer not only those which are the simplest but also those which serve to reduce the problem to another, simpler problem and that contribute to enlighten the mind; for example, I would wish to reduce quadratures or the dimensions of areas to the dimensions of curved lines.
>
> Mais entre les constructions Geometriques je prefere non seulement celles qui sont les plus simples mais aussi celles qui servent à reduire le probleme à un autre probleme plus simple et contribuent à éclairer l'esprit; Par exemple je souhaiterois de reduire les quadratures ou les dimensions des aires aux dimensions des lignes courbes.

6 Huygens [1694]:

> It is a strange assumption to take the quadratures of every curve as given, and if the construction of a problem ends with that, apart from the quadrature of the circle and the hyperbola, I would have believed that nothing had been accomplished, since even mechanically one does not know how to carry anything out. It is better to assume that we can measure any curved line, as I see your opinion is also.
>
> C'est une etrange supposition de prendre les quadratures de toute courbe comme estant données, et quand la construction d'un Probleme aboutist à cela, hormis que ce ne soit celle de l'hyperbole ou du cercle, j'aurois cru n'avoir rien fait; parce que mesme mechaniquement on ne scauroit rien effectuer. Il vaut un peu mieux de supposer qu'on peut mesurer toute ligne courbe, comme je vois que s'est aussi vostre sentiment.
>
> Jacob Bernoulli [1695] also stressed the importance of practical feasibility in a very similar context:

Certainly I consider curves which nature herself can produce with a simple and free motion, whatever their type and degree, preferable in constructions than other curves, even algebraic ones, which can be drawn either not at all or with difficulty; since that which joins the greatest exactitude with the greatest ease must be judged the best in the practical accomplishment of the work. Nempe existimo curvas, quas natura ipsa simplici & expedito motu producere potest, quorumcunque sint generum & graduum, in constructionibus praeferendas esse aliis, etiam algebraicis, quas arte vel nullo modo vel difficulter delineamus; cum illud semper in practica effectione operis sit censendum optimum, quod cum summa exactitudine summam quoque facilitatem conjunctam habet. See also Footnotes 9, 13 and 14 for further expressions of the same idea by both Bernoullis.

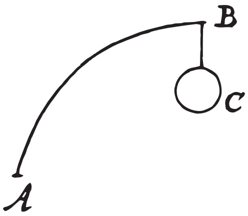

Figure 2. The elastica. The elastic beam AB bends under the weight C. From Jacob Bernoulli [1691].

Extramathematical principles such as these often show their true colour only in moments of conflict, so we should be grateful that the problem of rectification of quadratures was involved in one major confrontation of opposing views. This concerned Jacob Bernoulli's solution [1694a] of the paracentric isochrone problem[7] by rectification of the elastica, i.e., the curve assumed by a bent elastic beam (Fig. 2). In introducing his solution, Jacob Bernoulli appears quite certain that it will be appreciated. And with good reason: the rectification of quadratures was universally valued, as we have just seen, and the use of one mechanically defined curve to construct another also had ample precedent.[8] Thus, by way of justification, Bernoulli only passingly alludes to the practical feasibility of his solution:

> For although it is possible to carry out constructions by means of the squaring of any algebraic area, [the construction by rectification of the elastica] is to be preferred, I judge, since it is generally easier in practice to rectify a curve than to square an area, and especially since nature herself seems to have drawn it.[9]

Perhaps to his surprise, Bernoulli's construction was universally condemned. Huygens finds it "strange" and would prefer a construction by rectification of an algebraic curve,[10] as

[7] A challenge problem posed by Leibniz [1689]. See [Bos, 1993, 31–34] for more details, including differential equations for both the elastica and the paracentric isoscrone.

[8] Famous examples include Leibniz's construction of logarithms by the catenary [Leibniz, 1691b,d; Leibniz, 1692a,c] and Leibniz's and Huygens's use of the tractrix to, e.g., square a hyperbola (see [Bos, 1988]).

[9] Jacob Bernoulli [1694a]. "Nam quanquam idem exequi liceat, mediante quadratura spatii alicujus algebraici, alterum tamen construendi modum praeferendum censeo, tum quod generaliter facilius in praxi rectificentur curvae, quam quadrentur spatia, tum praesertim quod ipsa natura (siquidem convenientem tensionis legem observet) illum praescripsisse videatur."

[10] Huygens [1694]:

> It seems that you hold for true his construction of your paracentric [isochrone], after having examined, as I believe, the demonstration, as I have not yet done. It's quite a strange encounter to have there been able to employ his elastic curve; but your construction will assuredly be much better, if you only need to measure a geometric curve, or at least [a curve] for which you know how to find the points.
>
> Il semble que vous teniez pour veritable sa construction de vostre paracentrique, apres en avoir, comme je crois, examiné la demonstration, ce que je n'ay pas encore fait. C'est une rencontre assez etrange d'y avoir pu emploier sa courbe du ressort; mais vostre construction sera assurement meilleure de beaucoup, si vous n'avez besoin que de mesurer une courbe geometrique, ou de laquelle du moins vous scachiez trouver les points.

would Leibniz[11] and l'Hôpital.[12] The strongest condemnation, however, came from Jacob's younger brother, Johann:

> No one can fail to see how to construct [the paracentric isochrone] by quadrature of curvilinear areas [i.e., from the differential equation with separated variables]; but because the squaring of areas is not easy in practice, one attempts to do it by rectification of some other curve; if this curve can be algebraic, he sins against the laws of geometry who has recourse to a mechanical [curve]; especially if this very mechanical [curve] is no less complicated to describe than the quadrature of areas.[13]

This attack is issued in a paper where Johann Bernoulli instead constructs the paracentric isochrone by the rectification of an algebraic curve. But before this attack went to print Jacob Bernoulli had already arrived at the same rectification himself. However, he did so without altering his extramathematical views. In response to criticisms he instead elaborated on his original justification for his construction:

> There are three main methods for constructing mechanical or transcendental curves. The first is by areas of curvilinear figures, but it is ill-suited for practice. It is better to employ a construction by rectification of an algebraic curve; for curves can be more quickly and

[11] Leibniz [1694b]:

> The difference between us there is that he uses the rectification of a curve, which is itself already transcendental, namely his Elastica, and that thus his construction is transcendental of the second kind. Instead of like me using only the rectification of an ordinary curve of which I give the construction through common geometry.
>
> La difference qu'il y a entre nous là dessus est qu'il se sert de la rectification d'une courbe, qui est elle même deja transcendante, sçavoir de son Elastique, et qu'ainsi sa construction est transcendente du second genre. Au lieu que je me sers seulement de la rectification d'une courbe ordinaire dont je donne la construction par la Geometrie commune.
>
> Leibniz's rectification construction is in Leibniz [1694d], where the same point is repeated in similar words: Bernoullis construction is "transcendentalem secundi generis" since it starts with a transcendental curve (the elastica) and then performs a transcendental operation (rectification), which makes Leibniz's construction by a rectification of an algebraic curve "toto genere simpliciorem" ("of an entirely simpler kind").

[12] l'Hôpital [1694]:

> Regarding the curve which you call the paracentric isochrone, I am very pleased that one has finally found its solution, but as my remoteness from Paris has prevented me from seeing the Acts of Leipzig, I am not yet able to judge. It seems to me from what you tell me that your own [solution] will be much simpler and more general than that of Mr Bernoulli, since you find that there is an infinity [of solutions] where he only finds one, and since you use the rectification of an algebraic curve while he uses that of a transcendental one.
>
> A l'egard de la ligne que vous appellez isochrone paracentrique, je suis bien aise qu'on en ait enfin trouvé la solution, mais comme mon éloignement de Paris m'a empesché de voir les Actes de Leipsic, je n'en puis encore juger. Il me paroît par ce que vous me mandez que la vôtre sera beaucoup plus simple et plus generale que celle de Mr Bernoulli, puisque vous trouvez qu'il y en a une infinité où il n'en trouve qu'une seule, et que vous vous servez de la rectification d'une courbe algebraique lorsqu'il en employe une transcendente.

[13] Johann Bernoulli [1694e]. "Per quadraturam spatii curvilinei construi posse nemo non videt; sed quia in praxi non facile quadrantur spatia, tentandum illud est per rectificationem curvae alicujus; quae si potest esse algebraica, in leges Geometriae censendus est peccare qui recurrit ad mechanicam; praesertim si haec ipsa mechanica non minus operose per quadraturas spatiorura describatur."

> accurately rectified, using a string or small chain wrapped around them, than areas can be squared. I hold as equally good such constructions as are carried out without rectification and quadrature, by means of a single description of some mechanical curve, whose points, though not the whole curve, can be found geometrically in infinite number and arbitrarily close to each other; such is the usual Logarithmica, and perhaps others of the same type. The best method, however, wherever it is applicable, is that which uses a curve that Nature herself, without artifice, produces with a quick motion, almost instanteneously at the will of the geometer; for the preceding methods require curves whose construction, whether by continuous motion or by the finding of many points, is usually either slow or exceedingly difficult to carry out. Thus constructions of problems that assume the quadrature of a hyperbola or the description of the Logarithmica, other things being equal, I consider to be inferior to those which are carried out using the Catenary, as a suspended chain quickly assumes this shape of its own accord before the hand carries out the rest of the construction.[14]

Thus the construction of the paracentric isochrone by the elastica "would without a doubt be the best," he continues, if the assumption regarding the laws of tension made in the derivation of the elastica was truthful. But "it is safer not to trust" this assumption, and instead "have recourse to the second mode of construction and seek an algebraic curve whose rectification achieves the result."[15]

The fact that both Bernoullis found the construction by rectification of an algebraic curve almost immediately following the initial construction using the elastica speaks to the credibility of Jacob Bernoulli's professed preferences when he first introduced the elastica construction. For had he not truly felt that the rectification of the elastica was preferable to the rectification of an algebraic curve, he would surely have sought—and thus found rather easily, as subsequent history shows—the solution by algebraic curves, rather than allowing his brother the opportunity to immediately undermine his work with what he calls a "more excellent" solution.

Thus I believe that we have here a genuine conflict of extramathematical preferences, as opposed to a mere attempt to save face. Whereas some enthusiastic phrase casually

[14] Jacob Bernoulli [1694c]. "Triplex praecipue modus habetur construendi curvas mechanicas, sive transcendentes. Primus, sed ad praxin parum idoneus, fit per curvaturas spatiorum curvilineorum. Melior est, qui instituitur per rectificationes curvarum algebraicarum; accuratius enim & expeditius rectificari possunt curvae, ope fili vel catenulae ipsis circumplicatae, quam quadrari spatia. Eodem loco habeo illas constructiones, quae peraguntur absque ulla rectificatione & quadratura, per solam descriptionem curvae alicujus mechanicae, cujus puncta, licet non omnia, infinita tamen, & quantumvis proxima, geometrice inveniri possunt, qualis esse solet Logarithmica, & si quae sint ejus generis aliae. Optimus vero modus, sicubi haberi possit, ille est, qui peragitur ope alicujus curvae, quam Natura ipsa, absque arte, motu quodam celerrimo & quasi instantaneo ad nutum Geometra producit; cum praaecedentes modi requirant curvas, quarum delineatio, sive per motum continuum, sive per plurium punctorum inventionem, ab Artifice instituatur, communiter vel lenta vel impedita nimis existit. Ita constructiones illas Problematum, quae Hyperbolae quadraturam vel Logarithmicae descriptionem supponunt, caeteris paribus, posthabendas censeo iis, quae ope Catenariae peraguntur, seu curvae, quam suspensa catena sponte sua citius induerit, quam reliquis ipse describendis primam manum admoveris."

[15] Jacob Bernoulli [1694c]. "Tertii modi Constructio, quae fieret mediante Linea Elastica … sine dubio foret optima; si natura, alicubi tensiones viribus tendentibus simpliciter proportionales effecisset … Idcirco nec isti fidere satis tutum; praestatque recurrere ad secundum, construendi modum, & quaerere Curvam Algebraicam, cujus rectificatione scopum assequamur."

dropped by Leibniz in a personal letter to a friend may have to be taken with a grain of salt, the raging sibling rivalry between the Bernoullis suggests that they would have taken these matters with the utmost seriousness and left no room for error when they put their extra-mathematical preferences on record in these articles.

For this reason I shall consider this conflict as the key to evaluating extramathematical motivations for rectifying quadratures. So what does this episode tell us? In part it concerns the legitimacy of using physically given curves in mathematics, an issue which we must set aside for our present purposes. But it also casts some light on the motivations for the problem of rectification of quadratures.

In particular, Jacob Bernoulli's idea that a rectification is preferable to a quadrature since it can be effected by placing "a string or small chain" along the curve and then pulling it taut has been treated by several scholars as more or less interchangeable with the Leibnizian dimensionality argument.[16] However, the quotation from Bernoulli above is, to my knowledge, the first explicit mention of it,[17] despite the numerous discussions of the problem of rectification of quadratures predating this paper. And, as we have seen, Jacob Bernoulli stood alone against the rest of the establishment in this conflict.

In opposition to this concrete argument rooted in practice we saw Johann Bernoulli argue a more abstract case, namely that using a mechanical curve where an algebraic one will do is to "sin against the laws of geometry." To be sure, Johann also refers to practical ease as a motivation, but practice plays a different role in his argument. To him, it seems, practical simplicity is merely a suggestive justification for the "laws" of geometry, not an ultimate arbiter in and of itself. This point of view is certainly consistent with Leibniz's views cited above. Leibniz's appeals to a dimensional hierarchy, though initially suggested by simplicity considerations, seems to go beyond whatever partial justification such considerations can confer upon them and take on an absolute, legislative stature akin to Johann's "laws." This is reminiscent of the hierarchy of degrees in Cartesian geometry, or the distinction between "plane," "solid," and "linear" problems in ancient Greek geometry.[18] According to Pappus, it is "not a small error for geometers" to solve a problem

[16] See [Bos, 1974, 8], [Bos, 1993, 105] and [Weil, 1999, 9]. Hodgkin [2005, 180], even takes the string argument to have been the main motivation for preferring rectifications over quadratures.

[17] It is true that the very term "rectification" suggests this idea. However, in its technical usage it was not primarily intended thus, just as a "quadrature" or "squaring" usually no longer referred to the literal finding of a square of equal area as the figure in question. It should also be noted that the idea of measuring arc lengths by unwrapping strings was well-known in the context of evolutes (see below, esp. Fig. 7).

[18] I.e., problems that can be solved by ruler and compasses, by conic sections, and by more complicated curves respectively. This classification is famously expressed by Pappus (*Collection*, Book IV; see [Pappus, 2010, 144–146]) but did not originate with him. As Jones writes, "Pappus is our only explicit authority on this mathematical pigeon-holing, and he says nothing about how it developed and when. However, it is difficult not to see Apollonius' two books on *Neuses* as inspired by the constraints of method imposed on the geometer ... The only conceivable use for such a work would be as a reference useful for identifying 'plane' problems, and hence avoid the solecism of treating them as if they were 'solid'." [Pappus, 1986, 530] Descartes refers to Pappus's classification but of course goes on to refine it by requiring that one "go further, and distinguish between different degrees of these more complex curves" [Descartes, 1954, 40]. ("Ie m'estonne de ce qu'ils n'ont point outre cela distingué diuers degrés entre ces lignes plus composées" [Descartes, 1637, 315].)

"from a non-kindred kind," i.e., using curves of a higher order than necessary.[19] Descartes legislates similarly:

> We should always choose with care the simplest curve that can be used in the solution of a problem, but it should be noted that the simplest means not merely the one most easily described, not the one that leads to the easiest demonstration or construction of the problem, but rather the one of the simplest class [i.e., degree] that can be used to determine the required quantity.[20]

As in these cases, so in ours: simplicity, practical feasibility, or, for that matter, properties of "mind"—a favourite with Descartes as well as Leibniz—is invoked to justify the hierarchy, but once in place it is the hierarchy itself that is used to evaluate mathematics, not the underlying reasons originally used to justify it. In this way I think the conflict over the paracentric isochrone suggests a useful framework for imposing some order on the multitude of arguments thrown about to motivate the problem of rectification of quadratures. This point of view squares well with Leibniz's reproof of Jacob Bernoulli's construction by rectification of the elastica as "transcendental of the second order"[21]: the construction is judged by its hierarchal classification rather than on the basis of simplicity, the enlightening of minds, or what have you. Indeed, the connection with the geometrical tradition that we have just highlighted is invoked by Leibniz himself is a very similar context, namely when he condemns Fatio de Duillier for using a curvature-based approach where a first-order differential equation suffices. For, says Leibniz, curvatures "depend on differentio-differentials [i.e., second derivatives], which are what we call transcendental of the second order: it is as if one were to solve a plane problem by conic sections or even higher [curves]."[22]

Another interesting parallel suggested by this point of view concerns the underlying need for imposing a hierarchy in the first place. In the case of the ancient Greek hierarchy of curves, Jones notes:

> Restrictions on the permissible use of higher orders of loci ... probably became prevalent only after experience had shown how easily conic sections made possible the solutions not only of problems that had not been solved by compass and straight edge, but also of problems ... that were already soluble, but only with difficulty. (Jones in [Pappus, 1986, 540])

In other words, the introduction of a new mathematical technique must be accompanied by methodological restrictions so as not to trivialise the Gordian knots of old by allowing them to be cut with modern weaponry incongruous with the spirit of the challenge. Again there is a parallel in Leibniz when he stresses that his criterion of reducing quadratures to rectifications is consistent with earlier work such as Archimedes's reduction of the area of a

[19] [Pappus, 2010, 145].
[20] [Descartes, 1954, 152, 155]. "Il faut auoir soin de choisir tousiours la plus simples, par laquelle il soit possible de le resoudre. Et mesme il est a remarquer, que par les plus simples on ne doit pas seulement entendre celles, qui peuuent le plus aysement estre descrites, ny celles qui rendent la construction, ou la demonstration du Problesme proposé plus facile, mais principalement celles, qui sont du plus simple genre, qui puisse seruir a determiner la quantité qui est cherchée." [Descartes, 1637, 370].
[21] See Footnote 11.
[22] Leibniz [1700]: "haec vero a differentio-differentialibus pendeat, quae sunt, ut nos loquimur, transcendentia secundi gradus: quod perinde est, ac si quis problema planum ad sectiones Conicas, immo altiores referat."

circle to its circumference and the reductions of the quadrature of the hyperbola to the arc length of a parabola by Wallis, Heuraet and Huygens.[23]

In general, when a radically new mathematical technique is introduced that poses a potential threat to established norms of mathematical practice, the need arises to impose methodological restrictions in the form of a *hierarchy of methods*. The methodological framework chosen must not be too restrictive, as it must allow the new mathematics room to flourish, but it must also not be too liberal, as it should respect cherished parts of the mathematical canon and not render them obsolete by allowing old problems to be solved in trivial ways. This latter requirement I call *retroconsistency*. A further important desideratum of the methodological hierarchy imposed is that it be broadly justifiable on grounds independent of the new mathematics in question. In other words, methodological opportunism is considered bad form. This desiderata I shall refer to as *pre facto justifiability*.[24]

I propose that the need for such a hierarchy of methods was the fundamental force underlying the principled preference for rectification over quadratures. In this way some cohesion emerges in the variety of arguments presented for reducing quadratures to rectifications. In particular, the numerous arguments alluding to simplicity in various forms speak only to pre facto justifiability, which explains to some extent the indefinite nature of these arguments and their weak force in an actual moment of conflict. Thus, as we have seen above, the various arguments raised by Leibniz are readily interpreted as alternately addressing these desiderata, but at the moment of truth, when the elastica conflict cut to the heart of the matter, he phrased his judgment in terms of the hierarchy of methods itself rather than its subsidiary desiderata. Again, this explains also why Jacob Bernoulli's simplicity arguments were unanimously opposed despite their prima facie similarity to previous arguments by his opponents: he did not recognise the subordinate role of such arguments as addressing pre facto justifiability only.

This point of view can also help us bring out a certain measure of common ground between the otherwise widely different treatments of the foundational challenge posed by transcendental curves in the Leibnizian and Newtonian traditions. Newton took no foundational interest in the problem of reducing quadratures to rectification, and very little interest in the problem of constructing transcendental curves generally. Indeed, when Leibniz [1693b] wrote to Newton asking for "something big,"[25] namely a solution to the problem of rectification of quadratures, Newton [1693] offered with considerable indifference the solution "which you seem to want."[26] The solution he offered was based on his 1666 notes, where this problem occurs inconspicuously as one among many possible permutations of geometrical problems of the form "given this, find that," without any indication that this problem has a special foundational status.

However, at the same time as foundational issues such as that of rectifying quadratures commanded centre stage on the continent, Newton likewise found occasion to set out his

[23] See Footnote 4.

[24] I have chosen this term with the legal phrase *ex post facto* in mind. The concept of a retroactive law introduced after the fact is suggestive in the context of mathematics, as the history of mathematics is replete with after-the-fact rulings legitimising objects originally found abhorrent, such as complex numbers or, in our case, analytic expressions such as e^x or $\log(x)$ as a primary means of describing curves. Such rulings tend to be more pragmatically motivated than the principled argument of a pre facto justification.

[25] "Sed à TE adhuc magni aliquid exspecto." Quoted from Newton [1959–1977, III, 257/258].

[26] "Quam desiderare videris." Quoted from Newton [1959–1977, III, 285/286].

own delineation of "all legitimate geometry" in his extensive but ultimately abandoned drafts for a projected work on *Geometria*.[27] It is possible to extract a rationale for Newton's lack of interest in the foundational problem of transcendental curves from these manuscripts, although the extent to which they are representative of Newton's geometrical work generally is not beyond dispute. This draft treatise is a magistral monograph postdating most of his creative work, as opposed to the *in medias res* philosophising of the continentals. And, as is well-known, Newton's mature predilection for synthetic geometry "in the manner of the Ancients" stands in contrast to much of his earlier, more boldly modern work. On the other hand, the ideas explored in the manuscripts on *Geometria* appear in germinal form in the preface to the first edition of the *Principia* [1687], a fact testifying to their sincerity and centrality at least in Newton's mature thought.

Newton's vision of geometry in these manuscripts stems from the idea that the subject matter of geometry is measurements and inferences about measurements, not constructions. So for example he writes:

> The purpose of geometry is neither to form nor move magnitudes, but merely to measure them. Geometry forms nothing except modes of measuring.[28]

In particular, "geometry does not posit modes of description"[29]; rather it "postulates because it knows not how to teach the mode of effection."[30] The role of constructions in ancient Greek geometry is indeed ambiguous.[31] The surviving corpus allows several interpretations, and the Leibnizian and Newtonian modes of dealing with transcendental curves can be seen as belonging to different trajectories of extrapolation from it. The Leibnizian tradition is associated with the idea that *that which is known is that which is constructed*, thus making construction postulates the bedrock of geometrical knowledge. Newton, on the other hand, takes construction postulates as a licence for ignorance, stipulating what falls outside the purview of geometry proper. From this point of view the problem of the construction of transcendental curves, so fundamental in the Leibnizian tradition, becomes a non-problem, or a non-geometrical one at any rate. Instead, "any plane figures executed by God, nature or any technician you will are measured by geometry in the hypothesis that they are exactly constructed,"[32] a proclamation underwritten later by a postulate allowing the drawing of essentially any curve given by a "precise rule."[33]

Newton's discussion serves a second purpose for us, namely as an illustration of how fundamental the need was at the time for a hierarchy of methods satisfying the desiderata outlined above. Certainly Newton recognised that the drastic multiplication of geometrical techniques in the late 17th century had left the field in want of clear methodological foundations: "In such a diversity of opinion let us see what lead we need at length to follow," he writes.[34] The "lead" proposed by Newton is indeed motivated in terms of the desiderata we

[27] These manuscripts, dating from the early 1690s, are in Newton's *Mathematical Papers* [1967–1981], VII. The quoted phrase is from p. 389. "Geometriam om[n]em legitimam" (p. 388).
[28] p. 291. "Intentio Geometriae est megnitudines nec formare nec movere sed mensurare tantum. Nihil format Geometria praeter mensurandi modos." (p. 290).
[29] p. 289. "Geometria modos descriptionum non ponit." (p. 288).
[30] p. 291. "Postulat Geometra quia modum effectionis docere nescit." (p. 290).
[31] Cf., e.g., [Knorr, 1983].
[32] p. 289. "Figuras quasvis planas a Deo Natura Artifice quovis confectas Geometra ex hypotesi quod sunt exactè fabricatae mensurat." (p. 288).
[33] p. 389. "certa lege" (p. 388).
[34] p. 295. In tanta sententiarum diversitate videamus quid tandem sequendum sit. (p. 294).

have outlined. To begin with, Newton supports his characterisation of geometry as being about measurement rather than construction in the first instance by reference to the early history of the subject: "'Geometry' means the art of 'earth-measure'" and "the reason for its first intuition must be preserved."[35] In our terms, this is a pre facto justification. Furthermore, Newton is quite explicitly concerned with retroconsistency, discussing in some detail how ill-chosen postulates would "put all ancient geometry out of joint."[36] And when he introduces his "precise rule" postulate he takes great pains to construe it as a generalisation of Euclid's postulate of circles, and hastens to add that:

> I do not recount these [postulates], however, in order to displace Euclid's postulates. On those 'plane' geometry best relies. The present ones we will be free to employ each time the topic is one of 'solid' and 'higher than solid' geometry.[37]

In conclusion, I have argued in this section that cohesion and rationale can be brought out in the apparent diversity and disparity of extramathematical arguments regarding the rectification of quadratures by considering them as subsidiary to more fundamental principles, namely the need for a hierarchy of methods being both retroconsistent and justifiable pre facto. I have briefly argued further that the same fundamental principles were appreciated by Newton when he faced a similar problematic, which adds credibility to my initial thesis and can be taken as a modicum of reconciliation between the perplexingly different attitudes towards the foundational status of transcendental curves, and the problem of rectification of quadratures in particular, in the Leibnizian and Newtonian traditions of the calculus.

I use the term "foundational" as a convenient shorthand for the complex of issues discussed in this section, although the precise sense in which they were foundational admittedly remains somewhat elusive. At the very least they were clearly foundational in the general sense of pertaining to underlying principles, as they did not concern specific results or problems but rather addressed the underpinnings of all work on transcendental curves. It is debatable to what extent they were also foundational in the stricter sense of pertaining to the certainty of mathematical knowledge and the delineation of which objects and methods are acceptable in mathematics. I believe our protagonists deliberately left this question open, and that they did so with good reason. On the one hand, to rectify quadratures is to build up the complicated from the simple—arguably the premier safeguard of certainty and exactness in Euclid and Descartes alike, as well as a time-honoured principle of methodological purity. Thus the motivation for elevating the requirement that quadratures be reduced to rectifications to a "law of geometry" akin to the foundational principles of Euclid and Descartes is readily apparent. On the other hand, such a move would have been premature given the lack of general methods for actually performing this reduction in practice and the exceptional state of flux and rapid expansion of the field at this time. Indeed, as we have seen, Leibniz often spoke of the rectification of quadratures as a kind of research program rather than an absolute law, though at the same time recognising its foundational potential. If this research program had been conclusive, it may very well have led to definitive proclamations on the foundational status of the rectifications of quadratures, just as

[35] p. 287, p. 291. "Geometria idem sonat quod Ars mensurandi terram." (p. 286) "Et servanda est ratio instutionis primae" (p. 290).
[36] p. 297. "Geometria omnis antiqua luxabitur." (p. 296).
[37] p. 391. "Haec autem non trado ut Postulata Euclidis submoveam. Illis Geometria plana optime innititur. His uti licebit quoties in Geometria solida et sursolida res est." (pp. 388, 390).

Descartes's foundational program was the conclusion of his geometrical research rather than its starting point, as Bos [2001] has shown. But things did not turn out that way, and the program never advanced beyond its exploratory, pre-legislative stage.

3. How the problem of rectification of quadratures shaped the early development of the calculus

From the general considerations above I shall now turn to the question of how the problem of rectification of quadratures shaped the early development of the calculus. My interest in doing so is not to trace the details of the various attempts at actually solving the problem—a tragic story, no doubt, of a tortuous struggle to realise a dream that ultimately had to be abandoned. Rather my interest lies in arguing that an appreciation of these foundational concerns illuminates choices made by mathematicians in this period that are systematically misconstrued from the points of view of alternative historiographical perspectives.

3.1. Geometrical versus analytical conceptions of the calculus

For the modern reader, the preoccupation with the representation of curves in the early history of the calculus is readily perceived as the perhaps unavoidable but ultimately incidental teething troubles of an adolescent branch of mathematics. On this view, the transition from geometrical constructions to the analytic view of the calculus that won ascendancy in the 18th century was a rather straightforward process in which superfluous relics of tradition gave way in light of reason. Though rarely spelled out in so many words, a conception such as this seems to underlie for example the following passage in a recent history of analysis.

> When [Johann] Bernoulli wrote this in 1692, he could not carry out the final integration [of the differential equation for the catenary] since he did not yet know that the integral of $1/x$ is the logarithm. . . . [Instead] he reduced the construction of the catenary to squaring a hyperbola. This example shows clearly the role of geometry in infinitesimal calculus at the beginning of the 18th century. [Jahnke, 2003, 111]

Thus Bernoulli's preferred manner of representing this transcendental curve is here characterised as an idiosyncrasy stemming from ignorance, and this is furthermore taken to have been the general pattern at the time.

But the claim that Bernoulli "did not yet know that the integral of $1/x$ is the logarithm" is an oversimplification at best. The work referred to is his lectures on the integral calculus.[38] But, as we have seen above (Fig. 1), earlier in the very same work Bernoulli treated the differential equation $a\,dx = \frac{a^2 dy}{y}$ and concluded correctly that "curva est Logarithmica" (p. 421). The step from here to explicitly writing $\log(x)$ for the antiderivative of $1/x$ was in no way profound, as witnessed by its inconspicuous first appearance in print two years later, when Leibniz casually wrote "$\frac{dy}{v} = \frac{dy}{a}$, ergo $\log v = \frac{y}{a}$" without further ado in the course of a parenthetical remark.[39] Furthermore, immediately after giving his own construction of the catenary, Bernoulli went on to discuss Leibniz's construction of the same curve, which is based on logarithms. In the course of this discussion Bernoulli writes "per naturam Loga-

[38] Johann Bernoulli [1692c, lecture 36].
[39] Leibniz [1694d, lecture 36].

rithmicae, $zdy = adz$" (p. 494), thus demonstrating again his complete understanding of the differential equation of the logarithmic curve. But neither Bernoulli nor Leibniz took this to mean that an analytic expression involving $\log(x)$ or e^x should be considered a "solution" to the differential equation for the catenary. On the contrary, Leibniz explicitly states that the catenary is "second to no transcendental curve" in terms of simplicity.[40] Thus Leibniz did not consider his construction of the catenary to have reduced it to a more elementary function but rather as having established a connection between two equally complicated curves (as underlined by his discussion of how to use the catenary to compute logarithms).

In all, these considerations show that reason rather than ignorance lay behind Bernoulli's preferred mode of representation of the catenary, and that the eventual transition to a standardised analytic mode of expression was a far more complex process than one of straightforward enlightenment. Indeed, the aversion to analytical representations of transcendental curves exhibited by the early pioneers of the calculus is highly rational in light of the hierarchy of methods hypothesis that I proposed above, as it is very difficult to imagine an analytically based hierarchy of methods that meets the demands of retroconsistency and pre facto justifiability as convincingly as the geometrically based alternatives.

3.2. The relation between pure and applied mathematics

A second traditional view that obscures the importance of foundational quandaries over transcendental curves is the notion that the calculus was developed to meet the needs of applied mathematics. This view is expressed for example by Truesdell [1987]: "the infinitesimal calculus and rational mechanics [developed] together, the former largely responding to conceptual problems set by the latter" (p. 77). Provocatively put, my thesis in the remaining part of this essay is that this common view should be turned on its head: it was the foundational needs of mathematics that motivated physical investigations rather than the other way around.

Indeed, the early history of the calculus appears paradoxical from Truesdell's point of view, as he himself admits. For instance, in concluding his account of "the first researches on the catenary," Truesdell [1960] writes:

> Nearly everything that concerns principle is taken from sources that lay unpublished for fifty to one hundred and fifty years. Indeed, the original papers consist in little else than 'constructions', i.e., the explanation of a desired curve in terms of properties of possibly more familiar ones. From the standpoint of mechanics, at least, the first researchers concealed everything they ought to have published and published only what they had better discarded. (p. 85)

"From the standpoint of mechanics"—yes. But from the standpoint of foundational investigations regarding transcendental curves—no. The catenary was a showcase for the reduction of quadratures to rectifications, with both Huygens and Johann Bernoulli publishing constructions based on rectification. Their choices in publishing make perfect sense from this point of view. By contrast, general investigations of the underlying physical laws were apparently not considered worthy of publication, even though Truesdell praises them in the highest terms:

> [Jacob] Bernoulli reached deepest of all the students of continuum mechanics of his century. In the theory of perfectly flexible lines in the plane, he derived the general equations

[40] Leibniz [1691b, 277]. "nec ulli Transcendentium secundam."

> and thus, had his work been known, would have closed the subject. (p. 109) Elegant as were the quick solutions of Leibniz and [Johann] Bernoulli for the ordinary catenary, these achievements of [Jacob] Bernoulli are of a different order of worth. (pp. 84–85)

Besides these unpublished "achievements of a different order of worth" on the theory behind the catenary, Jacob Bernoulli's second main claim to fame is, according to Truesdell, his paper on the elastica [Jacob Bernoulli, 1694b]. "It is difficult," writes Truesdell, "to find words to describe the power and beauty of this paper," which deals with what was then "the deepest and most difficult problem yet to be solved in mechanics" (p. 96). This he did publish, and from Truesdell's point of view it is inexplicable why Bernoulli chose to publish one of his excellent discoveries but not the other. From our point of view, however, an explanation suggests itself. For Bernoulli did not publish his elastica paper until at least three years after his initial discovery,[41] and then this publication is accompanied in the same volume of the *Acta* by a paper [Jacob Bernoulli, 1694a] using the rectification of the elastica to give a "most elegant" solution to a longstanding challenge problem of Leibniz', as discussed above. Thus it is tempting to imagine that Bernoulli judged his investigations worthy of publication largely because of its application to the problem of rectification of quadratures, which would be consistent with the publication choices we saw in the catenary case.

Unfortunately the historical sources left to us are insufficient to determine what relative weights Bernoulli attached to his theory of the elastica and his application of it, so this episode must remain suggestive only. Instead I shall now turn to a different case study—one in which a quirk of history affords an opportunity to test my thesis regarding the importance of the problem of rectification of quadratures in guiding the direction of mathematical research.

3.3. *Case study: The motivation for Leibniz's envelope paper of 1694*

In 1693, Leibniz wrote to Newton asking him for a solution to the general problem of reducing quadratures to rectifications: "I would very much like to see how squarings may be reduced to the rectifications of curves, simpler in all cases than the measurings of surfaces or volumes."[42] Newton [1693] wrote back with a solution that produces the rectifying curve by an envelope construction. Leibniz never referred to or made any use of Newton's construction in any of his writings, and subsequently the matter has been largely ignored by historians. However, Leibniz did publish an important paper on envelopes less than one year later, which, I claim, shows clear signs of being the outcome of Leibniz's study of Newton's solution. Indeed, I shall show how a simple train of thought leads naturally from Newton's letter to this paper.[43]

3.3.1. *Newton's method for rectifying quadratures*

Newton's solution as sent to Leibniz goes as follows (Fig. 3). A curve $y(x)$ is given and we seek to express its quadrature $\int_0^B y(x)\,dx$ in terms of arc lengths. For each point on the x-axis from $x = 0$ to $x = B$ we draw a ray whose angle ϕ with the x-axis is defined by $\cos\phi = y(x)$ (or, more generally, $a\cos\phi = y(x)$, where a is a constant large enough so that $y(x)/a$

[41] He announced the problem and claimed to have a solution in Jacob Bernoulli [1691].

[42] Leibniz [1693b], quoted from Newton [1959, III, 258]. "ut quadraturae … (quod valde vellem) reducantur ad curvarum rectificationes, ubique superficierum aut corporum dimensionibus simpliciores."

[43] My argument is based entirely on published sources. I have not consulted Leibniz's Nachlass.

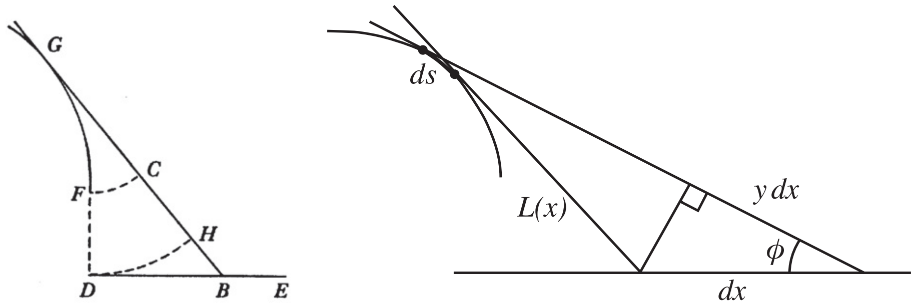

Figure 3. Left: Newton's figure from his 1693 letter to Leibniz. Right: the same configuration with my notation.

does not exceed 1; for the moment I limit my discussion to the case $a = 1$ for clarity). Next we find the curve FG enveloped by all these rays, where F is the point corresponding to $x = 0$ and G to $x = B$. Then $\int_0^B y(x)\,dx = BG - (GF + FD)$, where D is the origin of the coordinate system, so the integral has been expressed in terms of arc lengths, as required.

Newton does not include a derivation in his letter but his solution can be verified as follows. Pick one of the rays in the enveloping family. The condition $\cos\phi = y(x)$ means that if we take a dx-step away from its x-intercept and use this step length as the hypothenuse of a right-angled triangle determined by the ray then the leg of that triangle adjacent to ϕ is ydx, as illustrated in Fig. 3. Writing $L(x)$ for the distance to the envelope and ds for the envelope's arc element, we see from the figure that $L(x) + ds + ydx = L(x + dx)$, or $dL = ds + ydx$, which integrates to $L = s + \int y\,dx + c$. Our s is Newton's $FG = CG$, and our constant of integration c is the quantity $DF = CH$ appearing in Newton's solution, as we see by letting $x \to 0$ (and thus $s \to 0$ and $\int y\,dx \to 0$) in Fig. 3. Thus our formula $\int y\,dx = L - (s + c)$ corresponds to Newton's formula $\int_0^B y(x)\,dx = BG - (GF + FD)$.

Fig. 4 shows Newton's method applied to two specific examples. The method works flawlessly for $y(x) = \sqrt{x}$. However, for $y(x) = x^2$ it fails: the line segment FD assumed in the construction is not finite, as required. And the same problem occurs for many other choices of $y(x)$. But in light of the above derivation this is a minor problem. It means only that the constant of integration c cannot be immediately read off from the figure. However, the rectification still applies in the form $\Delta \int y\,dx = \Delta L - \Delta s$ for any choice of the bounds of integration that does not include the origin.

Figure 4. The enveloped curve FG of Newton's construction in the cases $y(x) = \sqrt{x}$ (left) and $y(x) = x^2$ (right).

3.3.2. Hypothetical reconstruction of Leibniz's reading of Newton's letter

Let us now consider Newton's construction from Leibniz's perspective. First I wish to indicate a derivation of Newton's result which is more heuristic and Leibnizian in spirit than the verification offered above. This will show that Newton's construction is highly amenable to Leibniz's way of thinking and that Leibniz would certainly have been able to master it with ease.

We want to find a line segment of length equal to the quadrature $\int y\,dx$. Thus the arc elements $d\sigma$ of the sought line will effectively equal the area elements ydx of the quadrature. The standard Leibnizian principle of dimensional homogeneity, however, requires that one expresses this in the form $ad\sigma = ydx$ for some constant a, so that both sides of the equation have the dimension of an area. The simplest way to create an area equal to ydx is to make a parallelogram with the same base and height, as indicated in Fig. 5. To rewrite this area in the form $ad\sigma$ we take a to be its length and $d\sigma$ its width, which must be placed at an angle to the x-axis as $d\sigma$ is generally not equal to dx. This angle is denoted ϕ in the figure and it follows at once that $\cos(\phi) = y/a$, just as in Newton's construction. Repeating the same construction for each dx produces many arc elements $d\sigma$ along the x-axis as shown. To assemble these into a single line segment, each arc element is rotated until it aligns with the final $d\sigma$, producing a line segment (shown grey) equivalent to BH of Newton's figure. The rotations need to take place along a family of circles perpendicular to each consecutive pair of arc elements. In other words, the centre of each circle is determined by the point of intersection of the extensions of two consecutive arc elements, as this guarantees that the arc elements are part of radial rays and thus perpendicular to the circle in question. But this means that the centres of these circles form the envelope of the extensions of the arc elements, and thus the enveloped curve FG of Newton's construction naturally emerges from this line of reasoning.

In this way Leibniz could quite seamlessly subsume Newton's general construction within his own framework. However, some problems present themselves when one tries to apply this method in specific instances using Leibnizian reasoning. At this point Leibniz already had his famous method of envelope determination, which may be stated thus in

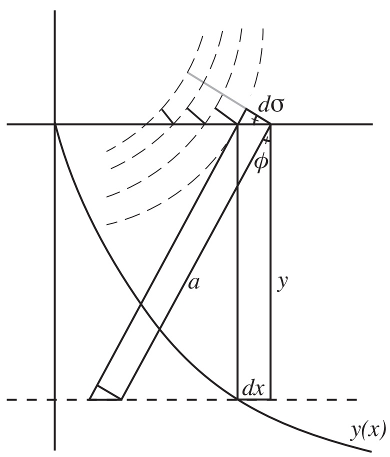

Figure 5. A heuristic argument leading to Newton's construction.

modern terms: to find the envelope of the family of curves $f(x,y,\alpha) = 0$, combine the two equations $f(x,y,\alpha) = 0$ and $\frac{d}{d\alpha}f(x,y,\alpha) = 0$ so as to eliminate α.[44] So the obvious thing to do from Leibniz's point of view would be to attempt to find Newton's envelope curve *FG* using this method. For this purpose it is necessary to translate Newton's condition $\cos\phi = y(x)$ into an algebraic equation for the family of enveloping lines. An easy calculation shows that Newton's condition translates into a slope of $-\frac{\sqrt{1-y^2}}{y}$, so the line in the family having x-intercept α has the equation $Y = -\frac{\sqrt{1-(y(\alpha))^2}}{y(\alpha)}(X-\alpha)$ (I use capital letters for the variables as $y(x)$ already has a meaning).

To go further we must specify the curve whose area is to be rectified. Let us consider the case $y(x) = x^2$. In this case the family of enveloping lines is $Y = -\frac{\sqrt{1-\alpha^4}}{\alpha^2}(X-\alpha)$. Leibniz's method for finding envelopes tells us to eliminate α by combining this equation with

$$\begin{aligned} 0 &= \frac{d}{d\alpha}\left(-Y - \frac{\sqrt{1-\alpha^4}}{\alpha^2}(X-\alpha)\right) \\ &= -\left(\frac{2\alpha}{\sqrt{1-\alpha^4}} - \frac{2\sqrt{1-\alpha^4}}{\alpha^3}\right)(X-\alpha) + \frac{\sqrt{1-\alpha^4}}{\alpha^2} \\ &= -\frac{\alpha^5+\alpha-2X}{\alpha^3\sqrt{1-\alpha^4}}, \end{aligned}$$

i.e., $2X - \alpha^5 - \alpha = 0$, a formidable task which would have been all but unsolvable for Leibniz. And this was the very easy case $y(x) = x^2$, which is by no means atypically complicated. For almost any other choice of $y(x)$ the situation is just as bad if not worse.[45]

Leibniz's envelope paper (1694c) provides some evidence that he indeed attempted to approach Newton's solution in this way. For the first problem proposed in Leibniz's paper is exactly identical to the problem of finding *FG* in Newton's construction: given the slopes of the enveloping lines as a function of their x-intercept, find the envelope (see Fig. 6). Furthermore, Leibniz never actually works out any instance of this problem, which makes sense in light of the fact that the problem is generally intractable in the cases relating to Newton's construction. Thus we already have a significant indication that Newton's letter influenced Leibniz's paper. This hypothesis is strengthened further as we continue to reconstruct hypothetically Leibniz's train of thought upon reading Newton's letter.

Having failed to find the envelope, it is easy to imagine that Leibniz would have gone on to seize on the idea that Newton's rectification is based on evolutes, as the "unwrapping" of *GFD* into *GCH* in Fig. 3 clearly suggests. This would have been easy to see for Leibniz, who was very familiar with Huygens's systematic investigation of the use of evolutes for rectification in his work on the pendulum clock [Huygens, 1673, part III]. However, Huygens's idea, outlined in Fig. 7, only enables us to rectify curves whose involutes are known. While Huygens was able to give a general method for finding the evolute of a given curve, he had no general method for finding the involute for a given evolute. Thus he was able to rectify a

[44] Leibniz's justification for this rule is sketchy, but the basic idea is that a point on the envelope is not only on one of the enveloping curves, so that $f(x,y,\alpha) = 0$ for some α, but also on the "next" one, i.e., it satisfies $f(x,y,\alpha + d\alpha) = 0$ as well, whence $\frac{d}{d\alpha}f(x,y,\alpha) = 0$.

[45] $y(x) = \sqrt{x}$ happens to be an exception. In this case Leibniz's method works and Newton's construction can be confirmed.

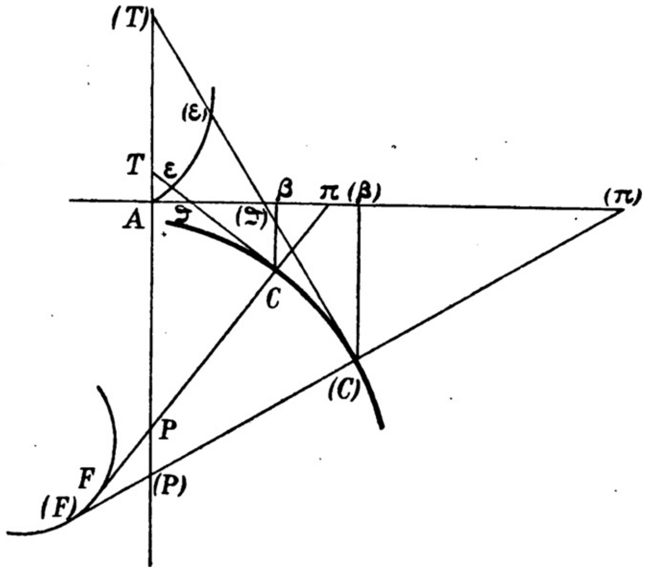

Figure 6. The main figure of Leibniz's paper on envelopes [1694c] (as reproduced in [Leibniz, 1908]). Below is a complete list of the problems considered by Leibniz in the order in which they appear in the paper, followed by a note on how they can be interpreted in the context of Newton's rectification method. Given the tangent line CT as a function of its axis intercepts T and ϑ, find the curve CC. This is equivalent to Newton's construction with $\phi = \angle AT\vartheta$. Given the point ε on the curve $\varepsilon\varepsilon$ as a function of T, find the curve CC. This variant of the first problem is analogous to uncovering the hidden involute curve not mentioned by Newton in his letter and using this curve in place of the earlier angle condition, as Newton did in his 1666 notes. Given the normal PC as a function of its axis intercepts P and π, find CC. As Leibniz remarks, this problem is reducible to the above since the normals also define the evolute FF (equivalent to the CC of the first problem), from which CC is given by evolution. This is Newton's construction with $\phi = \angle A\pi P$ and the evolute–involute pair explicitly indicated. Given the length of the normal PC as a function of the coordinate AP, find the curve CC. This is the problem of finding the involute curve needed for Newton's construction for a given $f(x)$ (= PC). (This is the only problem Leibniz actually works out in the paper.) Given the length of the tangent TC as a function of the coordinate AT, find the curve CC. This is the problem of finding the evolute curve given $l(x)$. This is how Newton in fact finds the evolute in his 1666 notes (using a curvaturesque formula for $l(x)$; see below). (As Leibniz notes, this problem is not solvable by his envelope method; instead he refers to his method of construction by tractional motion [Leibniz, 1693e]. See [Bos, 1988, 44–52] for a discussion of this paper.)

great many curves by starting with various involutes, but the general problem of rectifying any given curve remained unresolved.

Newton has an ingenious trick for circumventing this problem, as we know from his more complete account of his construction in his October 1666 tract on fluxions,[46] namely to consider the involute in terms of what Whiteside calls an "unusual semi-intrinsic system of co-ordinates." It is quite easy to reconstruct this trick from Newton's letter. One only needs to add the hidden evolute to the diagram to obtain Fig. 8 and proceed as above. In the exact same way as we found the expression $L = s + \int y\,dx + c$ above, we find that $l = s - \int y\,dx + C$ in this diagram. Applying an analogous argument for $f(x)$ gives

[46] Newton [1967–1981, I, 400–450, Problem 10].

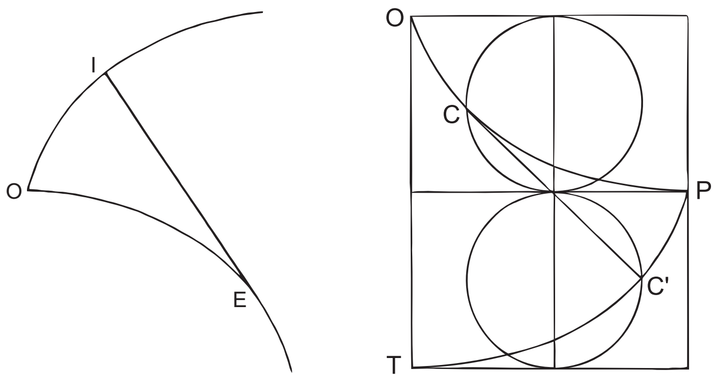

Figure 7. Left: The general method of evolute rectification. OE is the evolute of the involute OI. The arc OE = the line segment EI. (OE is the locus of centres of curvature of OI. OI is the path traced by the end of a string unwound from OE.) Right: The special case of the cycloid. The cycloid generated by C is the evolute of the congruent cycloid generated by C'. The arc CP = the line segment CC'. In particular, arc OP = line segment OT = twice the diameter of the generating circle.

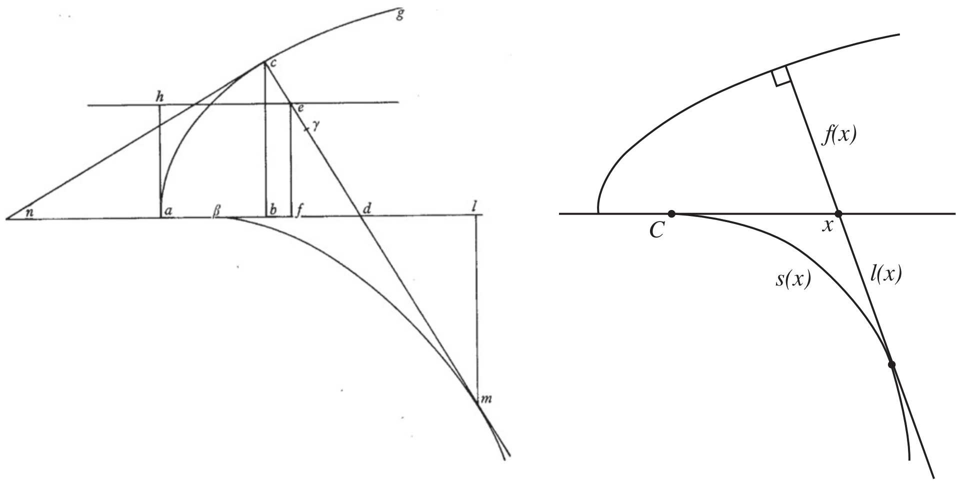

Figure 8. Left: The hidden evolute geometry of Newton's rectification method, not mentioned in the letter to Leibniz but seen here in Newton's 1666 tract on fluxions. Right: The same figure with my notation. The quadrature $\int_0^X y\,dx$ can be expressed in terms of arc lengths as $C + s(X) - l(X)$. (The $L(x)$ from Fig. 3 would be obtained by extending $l(x)$ to the y-axis in this figure, although the angle ϕ has now been redefined since the condition $\cos(\phi) = y(x)$ now takes its x-values from a different axis.)

$f(x) + l(x) + ds = f(x + dx) + l(x + dx)$, i.e., $df = ds - dl$. Integrating and substituting the known expression for l, we obtain $f = \int y\,dx$. This uncovers Newton's fundamental idea: to find the involute needed to rectify $z(x) = \int y\,dx$, form the curve with this z as the radial coordinate $f(x)$ in the "semi-intrinsic" coordinate system $(x,f(x))$ of Fig. 8. Once this idea is in place everything about $\cos(\phi)$ can be forgotten and the rectification can be restated purely in terms of evolute and involute.

This would have given Leibniz fresh hope after his failed attempt to calculate FG as an envelope, because he already knew other methods for finding evolutes. But to do this one must first find the hidden curve determined by the z's. This is another envelope problem: to find the envelope of the families of circles with centres at x and radius z. But contrary to the previous envelope problem this one is solvable by Leibniz's method. In fact, it is the very problem solved in Leibniz's envelope paper (see Fig. 6).

Leibniz's solution is as follows. We seek the curve (x,y) enveloped by a family of circles centred on the x-axis whose radii are determined by the values of an auxiliary function (b,c). The circle centred at the point $(b,0)$ has the equation $(x - b)^2 + y^2 = c^2$. If we take the auxiliary function to be the parabola $b = c^2$, then this equation reduces to $x^2 + y^2 + b^2 = 2bx + b$. Following Leibniz's general envelope method, we differentiate this expression with respect to b to get $b = x + \frac{1}{2}$. Then we use this equation to eliminate b from the equation we have before differentiating, which leaves us with the equation for the envelope, $y^2 = x + \frac{1}{4}$.

Thus Newton's method for rectifying $z(x) = \int y\,dx = x^2$ involves finding the evolute of this other parabola, $y^2 = x + \frac{1}{4}$. Using one of the several available general methods for finding evolutes, Leibniz could now easily compute the equation for the envelope curve FG required for Newton's construction. In this case the evolute is the semicubical parabola $16\left(y - \frac{1}{2}\right)^3 = 27\left(x + \frac{1}{4}\right)^2$, as was well known.[47] Indeed, the rectification is easily confirmed in this case. See Fig. 9.

However, this method of finding the involute is not very powerful. It fails, *prima facie*, as soon as $y(x)$ does not have an explicit algebraic antiderivative, since then it is typically not possible to eliminate the parameter as required by Leibniz's method. But Newton claims in his letter that the evolute can be found "geometrically" (i.e., algebraically) whenever $y(x)$ is "geometrical." In fact, Newton is right, as we know from his 1666 notes. There he derives an expression for $l(x)$ equivalent to a radius of curvature calculation, namely

$$l(x) = -\frac{1 - y^2}{y'}.$$

Since the right-angle triangle with $l(x)$ as its hypothenuse and its other legs parallel to the coordinate axes contains an angle ϕ, it is similar to the infinitesimal triangle with hypothenuse dx and legs $y\,dx$ and $\sqrt{1 - y^2}\,dx$ in Fig. 3 above. From this its horizontal and vertical components are easily computed to be $-\frac{y - y^3}{y'}$ and $\frac{(1-y^2)^{3/2}}{y'}$ respectively, so the evolute has the parametrisation

$$\left(x - \frac{y - y^3}{y'}, \frac{(1 - y^2)^{3/2}}{y'}\right).$$

[47] The particular case of an evolute of a parabola was a standard example which had been solved in print by Huygens and both Bernoullis. Huygens gave a general method for finding evolutes in his book on the pendulum clock [Huygens, 1673, part III], which is also where he introduced the concept of evolute. Calculus-based methods where later developed based on the osculating circle of Leibniz [1686], whose centres of curvature define the evolute (as noted by Leibniz [1691d]). Jacob Bernoulli [1692a] used this idea to devise a calculus-based method for finding evolutes: the osculating circle intersects the curve with multiplicity 3, which corresponds to a vanishing second derivative. Johann Bernoulli [1692b] (also Johann Bernoulli [1692c, lecture 16]) gave a solution in more elementary terms by avoiding the differentiation in favour of expressing the multiplicity of the root in algebraic terms.

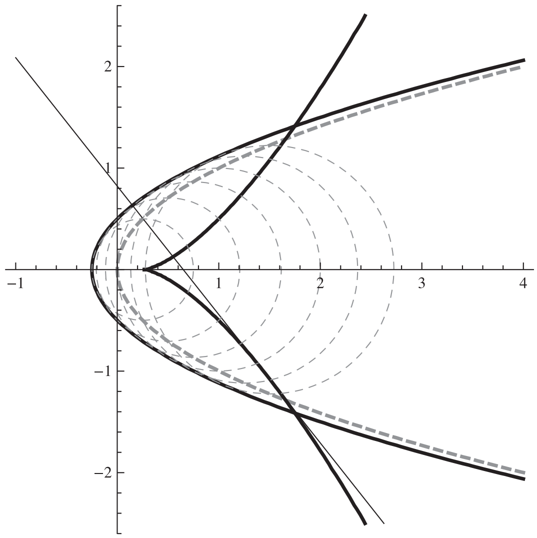

Figure 9. The evolute form of Newton's rectification method applied to the quadrature $\int \frac{dx}{2\sqrt{x}}$. The dashed parabola has a y-value of $f(x) = \int \frac{dx}{2\sqrt{x}} = \sqrt{x}$. For each point on the x-axis, a circle is drawn (shown dashed) with this point as its centre and $f(x)$ as its radius. These circles envelope the involute needed, the solid parabola. The evolute of this curve is the semi-cubical parabola also shown solid. Drawn thin is the tangent line needed for the rectification of $\int_0^{0.65} \frac{dx}{2\sqrt{x}}$.

Thus the evolute is indeed geometrical whenever y is—at least in the sense of having an algebraic parametrisation—so Newton's claim is correct.[48]

It is doubtful whether Leibniz pursued the problem in this direction. However, two considerations suggest this possibility. Firstly, an approach based on curvature readily suggests itself as a means of avoiding integrating $y(x)$ since evolutes are loci of centres of curvature and curvatures generally involve only derivatives and second derivatives, two facts very familiar to Leibniz. Secondly, Leibniz's envelope paper closes with a quite inexplicable mention of a problem that does not involve envelopes and cannot be solved using the methods of the paper (see Fig. 6). This oddity could be explained by the fact that the problem is equivalent to finding the evolute when $l(x)$ is given, which as we just saw is precisely the way in which Newton found this curve.

3.3.3. What did Leibniz think of Newton's solution?

Leibniz quite probably did not consider Newton's construction a fully satisfactory solution to the problem he had in mind. It seems that Leibniz was thinking of the more direct problem: given a quadrature $\int y\,dx$, find a curve $g(x)$ whose arc-length equals it, i.e.,

$$\int y\,dx = \int \sqrt{1 + (g')^2}\,dx.$$

[48] Whiteside is confused on this point. In his excellent notes on the 1666 treatise he recognises that Newton is correct [Newton, 1967–1981, I, 439, esp. n. 137]. When commenting on the letter to Leibniz, however, he mistakenly claims that Newton is wrong "since all but a few functions z having 'geometrical' fluxional derivatives will not themselves be algebraic" [Newton, 1967–1981, VII, xx, n. 46], a point which is irrelevant since Newton's expression for the evolute involves only $y = z'$ and y', not z.

For when Leibniz next brings up the problem of rectifying quadratures in an article in the *Acta* the following month, he claims that $\int \sqrt{a^4 + x^4}\, dx$ can be rectified by a hyperbola,[49] and this is certainly not the result of using Newton's construction, which would give a much more complicated curve.

Thus when Leibniz says that he wants to "reduce squarings to the rectifications of curves" he means that he wants to transform a quadrature problem into a rectification problem. Newton, on the other hand, takes the problem to be about *actually rectifying* the quadrature, that is to say, to find a *straight* line segment with a length equal to the given area. No wonder, then, that Newton's method gives a more complicated solution than Leibniz desires, since it in effect solves two problems at once: it both reduces the quadrature to a rectification problem and then solves the rectification problem at the same time.

Nevertheless, it seems plausible that Leibniz would have considered Newton's construction as important, not only because it provides one very general and powerful way of rectifying quadratures (although perhaps too indirectly for Leibniz's tastes), but also since it in a way solves the problem of rectifying a curve by evolutes when the involute is unknown, which had been a recognised lacuna in the theory of evolutes since its introduction by Huygens.

3.3.4. Conclusion

We have seen that the examples used in Leibniz's envelope paper are precisely the problems he would have faced had he tried to untangle the rectification of integrals in Newton's letter. My hypothesis that this was indeed the actual background for the paper is strengthened by the fact that Leibniz elsewhere showed a tendency to rework ideas that he came across and publish his own variants of them. Notable cases includes his introduction of the osculating circle in response to Huygens's theory of curvature,[50] as well as his planetary theory, which Meli [1997] has shown to have been a reworked version of Newton's theory in the *Principia* rather than an independent discovery. To some extent it appears that Leibniz used this publication strategy to establish, if not outright priority of discovery, at least the fruitfulness of his own methods in generating organically the results of others. His envelope paper of 1694 and the specific choices of examples in it make sense from this point of view.

Further evidence for my thesis is provided by a letter by Leibniz [1693c] to l'Hôpital written a few months before he received Newton's construction. In reply to l'Hôpital's queries, Leibniz here outlines his envelope method and provides a worked example which he says he has chosen for simplicity of illustration. But this example is not used in the 1694 paper, again suggesting that the problems of the 1694 paper were occasioned by Newton's letter.

But for our purposes the most remarkable thing about the story of Leibniz's envelope rule is how strikingly it fits the inversion of the traditional conception of the interplay

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[49] It happens that Leibniz was mistaken, as he later admitted [Leibniz, 1695]. A cubical parabola is needed rather than a hyperbola. But this does not alter my point.

[50] In a letter to Leibniz, Huygens [1691] pointed out that the idea of the osculating circle was implicit in his work all along:

> You can easily believe that in reading it I did not find this consideration novel, since these sorts of contact enter naturally into my Evolutions of Curved Lines.
> Vous pouvez bien croire qu'en le lisant je ne trouvay pas cette consideration nouvelle, parce que ces sortes de contact entrent naturellement dans mes Evolutions des Lignes courbes.

between "pure" and "applied" mathematics that I suggested above. Before receiving Newton's letter, Leibniz had already known for some time that his rule was useful in optics; quite possibly he even discovered it in this context. But with this as its only merit he treated the rule rather disparagingly. The year before Newton's letter, Leibniz [1692b] published his rule in an inconspicuous little article under the pseudonym "O. V. E." The article is just over three pages, and a good part of it is spent discussing matters of vocabulary that has no direct bearing on the envelope rule. Optical problems are mentioned as motivation, and the rule is then alluded to in abstract terms without a single formula or figure appearing in the entire paper. After having received Newton's letter, where the envelope rule is found to be of consequence for foundational questions, Leibniz's tone is markedly different. He now publishes a five-page paper with detailed calculations and figures devoted entirely to his envelope rule, calling it a "new" application of his calculus "of no small importance for the development of geometry."

In short, viewing this episode through the lens of foundational concerns regarding the representation of curves explains the timing, the specific content, and the rhetoric of the envelope paper. The same cannot be said for the traditional lens of taking mathematical advances as driven by the need to solve "applied" problems—which is indeed the approach taken in a recent study arguing that optical problems regarding caustics were a key motivation for Leibniz's paper.[51]

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