Four years later Leibniz (1694i) has only grown more confident if anything:

> I have no doubt that we will find one day a method of reducing everything to the simplest quadratures. I even believe I see the method, of which I also have examples, but I am not in a state to work on it.[306]

Leibniz (1693l) too sees the problem as a rather straightforward one "to compute the necessary canons, for which I have no time."[307]

In retrospect it is abundantly clear that Leibniz was overly optimistic in these hopes. The "canons" he envisioned did not materialise, of course, and ten years later he had little progress to show for his programme except for a highly imperfect paper on partial fractions. Partial fractions is indeed a crucial method for the reduction of quadratures to standard ones, and Leibniz makes it clear that the general, foundational problem of the reduction of quadratures to canonical ones is his motivation in studying this method. Thus Leibniz (1702) writes in his partial fraction paper for example that:

> This leads us to a question of the greatest importance: whether all rational quadratures can be reduced to the quadrature of the hyperbola and the circle ... I have discovered however that to him who thinks this the abundance of nature will be more tightly contracted than is appropriate.[308]

This is accurate enough, but Leibniz's justification for it is not. According to Leibniz, "$\int dx : (x^4+a^4)$ can be reduced to neither the circle nor the hyperbola by this analysis of ours [i.e., partial fractions], but establishes a new kind of its own,"[309] whereas in reality $\int 1/(x^4+a^4)dx$ can be expressed in terms of logarithms and arctangents. This mistake notwithstanding, it is illuminating to see how Leibniz wishes to give geometrical meaning to the result:

> And I have wished ... that as $\int dx : (x+a)$, or the quadrature of the hyperbola, is known to give logarithms or the division of a ratio, and $\int dx : (xx+aa)$ the division of an angle, so the sequence could be continued further, and it ought to be established to which problem $\int dx : (x^4+a^4)$, $\int dx : (x^8+a^8)$, etc., correspond.[310]

So still at this late date, and in the context of this eminently analytical line of research, Leibniz remains adamant that the results be anchored in geometry. Indeed, his desire to concoct geometrical problems to match foundational questions seems to have a direct equivalent in Greek geometry, as discussed in section 3.6.3. As ever, it is not Leibniz's way to start with concrete problems and address foundational questions as needed for those problems; rather to him foundational questions always come first, and specific problems are merely ways of instantiating them and making them concrete.

7.3 The rectification of quadratures

7.3.1 Why rectify quadratures?

The analytic-computational simplification and classification of quadratures discussed above is easy to relate to from a modern point of view, but at the time a geometrical and

decidedly more idiosyncratic method for reducing quadratures was more prominent, namely that of reducing quadratures to rectifications. That is to say, 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 doing it the other way around, because they thought it made more geometrical sense. As Leibniz (1693b) puts it:

> 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.[311]

Rectifying a quadrature replaces a very weighty assumption (that certain areas can be found) by a more modest one (that certain lengths can be found), but, to be sure, the latter is still an assumption that is far from trivial. Thus the rectification of quadratures is more of a simplification than a completely satisfactory solution. As Leibniz (1694i) explains:

> There are several degrees of solutions [to transcendental problems]; the most perfect, without doubt, is that which reduces transcendentals to the area of a circle or hyperbola. In the absence of that I want to be able to describe the transcendental curve by points in imitation of the logarithmica which is described by mean proportionals. And when this is also lacking, I content myself with obtaining my goal by the rectifications of curves. But there are cases so difficult, where all that I can do so far is to give an infinite series.[312]

But despite this drawback 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; there is perhaps greater universal consent on this issue than on any other scheme for resolving the problem of transcendental curves. For example, Leibniz (1691g) writes to Huygens:

> 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?[313]

To which Huygens (1691d) replies:

> 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.[314]

Johann Bernoulli (1694a) likewise agrees:

> I believe you are right to say that it is better to reduce quadratures to rectifications of curves, rather than the other way around.[315]

But despite this widespread agreement the motivation for reducing quadratures to rectifications 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. Thus Leibniz (1693d) writes:

> I would much prefer, for example, to reduce the quadratures to the rectification of curves, because the dimension of the line is simpler than that of a space.[316]

And again 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.[317]

Leibniz (1691c) even traced the pedigree of this principle back to Archimedes's reduction of the area of a circle to its circumference:

> 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 have reduced 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.[318]

But elsewhere Leibniz (1693i) emphasised instead that a rectification "enlightens the mind" more than a quadrature:

> 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 which contribute to enlighten the mind; for example, I would wish to reduce quadratures or the dimensions of areas to the dimensions of curved lines.[319]

Then again in other cases rectifications seem to be preferred over quadratures for the sake of greater practical feasibility. Thus Huygens (1694b) writes:

> 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 a bit better to assume that we can measure any curved line, as I see your opinion is also.[320]

Altogether the diversity of arguments used to justify the rectification of quadratures at first sight appears quite confusing.

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 (1694b) of the paracentric isochrone problem by rectification of the elastica, i.e., the curve assumed by a bent elastic beam.[321] We shall discuss the elastica and the paracentric isochrone problem in greater detail in chapter 8. Suffice it to say for now that the problem of finding the curve reduces to integrating $1/\sqrt{1-x^4}$, a complicated and nonstandard integral that required innovative methods such as the use of the elastica for its solution.

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 seen, and the use of one mechanically defined curve to construct another also had ample precedent, such as Leibniz's construction of logarithms by the catenary (section 6.3.2) and Leibniz's and Huygens's use of the tractrix to, e.g., square a hyperbola (chapter 5). Indeed Jacob Bernoulli (1693b) had noted in another context that a certain quantity "depends on the quadrature of a hyperbola; therefore it is found by means of a logarithm or string."[322] This endorsement of the "string" (i.e., catenary) construction of hyperbolic quadratures suggests that his own mechanical construction is sincere, and not a misguided attempt at promoting his own elastica. Thus, by way of justification of his paracentric isochrone construction, Bernoulli only passingly alludes to the practical feasibility of his solution:

> For although it is possible to carry out the same by means of the squaring of any algebraic area, another method of construction 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 [i.e., the elastica].[323]

Perhaps to his surprise, Bernoulli's construction was universally condemned. Huygens (1694b), writing to Leibniz, finds it "strange" and would prefer a construction by rectification of an algebraic curve:

> 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.[324]

Leibniz (1694e) agrees:

> He makes use of the rectification of a curve which is itself already transcendental, namely his elastica, and thus his construction is transcendental of the second order. In place of which I only make use of the rectification of an ordinary curve for which I give the construction by common geometry.[325]

l'Hôpital (1694b) also agrees:

> 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.[326]

The strongest condemnation, however, came from Jacob's younger brother, Johann Bernoulli (1694b):

> No one can fail to see that [the paracentric isochrone] can be constructed by quadrature of a curvilinear area [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 mechanical [curve] itself is no less complicated to describe by the quadrature of areas.[327]

This attack is issued in a paper where Johann Bernoulli instead constructs the paracentric isochrone by the rectification of an algebraic curve (the "lemniscate"—see figure 7.1). 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 Jacob Bernoulli (1694c) 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 a better [method] to employ a construction by rectification of an algebraic curve; for curves can be more quickly and 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 instantaneously 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

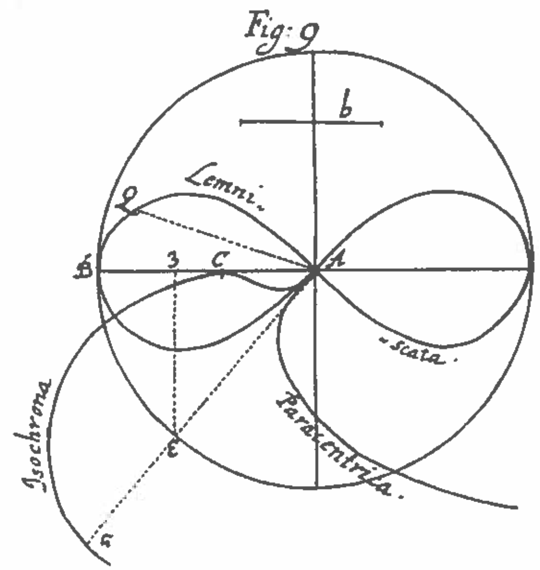

Figure 7.1: The paracentric isochrone constructed by rectification of the lemniscate $(x^2+y^2)^2 = a^2(x^2-y^2)$. From Jacob Bernoulli (1695).

> a suspended chain assumes this shape of its own accord more quickly than you will have moved the first hand for the rest to be described.[328]

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."[329]

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 the latter 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 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 extramathematical 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.[330] However, the quotation from Jacob Bernoulli (1694c) above is, to my knowledge, the first explicit mention of it,[331] 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, seem 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. 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"[332]: the construction is judged by its hierarchal classification rather than on the basis of simplicity, the enlightening of minds, or what have you. This also agrees with our argument in section 3.7.1 about the role of a hierarchy of methods more generally.

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 what I called 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. In this way I believe 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.

Admittedly, the precise foundational status of the rectification of quadratures remained somewhat elusive. They were certainly 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 programme rather than an absolute law, though at the same time recognising its foundational potential. If this research programme had been conclusive, it may very well have led to definitive proclamations on the foundational status of the rectifications of quadratures, just as Descartes's foundational program was the conclusion of his geometrical research rather than its starting point.[333] But things did not turn out that way, and the program never advanced beyond its exploratory, pre-legislative stage.

7.3.2 The motivation for Leibniz's envelope paper of 1694

The importance of the problem of rectification of quadratures in guiding the direction of mathematical research can be seen in a historical episode where a quirk of history affords an opportunity to study Leibniz's attitude towards a certain mathematical result just before and just after he realised that it had important implications for the problem of transcendental curves. The result in question is Leibniz's method of finding envelope curves, i.e., curves determined by their being tangent to a given family of curves; for example, in figure 7.3, $C(C)$ is the envelope of the family of lines TC, $(T)(C)$, etc. Leibniz's method may be stated thus in 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.[334]

Envelopes are central in optics (as they define caustic curves), and quite possibly

This is an analog for the case where s is taken as the independent variable of the usual curvature formula that we saw Johann Bernoulli prove in section 4.4.4.2. Like Johann's, Jacob's proof is based on straightforward but tedious manipulations with similar differential triangles, which we shall not go into here.[357] In fact, as Johann Bernoulli noted,[358] this special form of the curvature expression is not needed; the differential equation for the elastica can just as well be derived from the standard formula for curvature of section 4.4.4.2 with only a few extra steps of algebra. Incidentally, Jacob Bernoulli rather exuberantly calls his curvature results a "golden theorem" ("aureum Theorema"), apparently unaware that such expressions had been known for years. Leibniz (1694e) and Johann Bernoulli (1696a) were unimpressed.

In any case, using his curvature expression and equating this with the other expression for the extension, Jacob obtains the differential equation

$$ax = -\frac{d^2y}{dx\,ds}$$

Integrating both sides with respect to x gives

$$\frac{ax^2}{2} = -\frac{dy}{ds}$$

We see that there are no complications regarding constants of integration since $dy/ds = 0$ corresponds to $x = 0$, in agreement with the definition of the rectangular elastica. If we substitute $ds = \sqrt{dx^2 + dy^2}$ and solve for dy the differential equation becomes

$$dy = \frac{x^2\,dx}{\sqrt{\frac{4}{a^2} - x^4}}$$

Since the variables are separated in this differential equation, it gives a construction by quadratures (in the manner of section 4.4.3). Bernoulli does indeed spell out this construction, but of course it is not completely satisfactory since the areas under a complicated algebraic curve must be assumed given. In his defence Bernoulli maintains that the construction cannot be simplified by the usual method of reduction to measurements of conic sections: "I suspect on compelling grounds the construction of our curve to depend on neither the quadrature nor the rectification of any section of a cone."[359] As discussed in section 7.3.1, Bernoulli tried to turn this adversity into triumph by arguing for the acceptance of the elastica as a geometrical given in terms of which other curves may be constructed, such as the paracentric isochrone, though he failed to convince others that this was an appropriate form of construction.

It seems likely that Bernoulli valued his paper on the elastica (Jacob Bernoulli (1694a)) primarily for showcasing foundational mathematical matters, namely his solution of the paracentric isochrone problem and his "golden theorem" for the radius of curvature. Indeed, as we noted above, he did not publish his elastica paper until at least three years after his initial discovery, and then this publication was accompanied in the same volume of the *Acta* by a paper (Jacob Bernoulli (1694b)) using the rectification

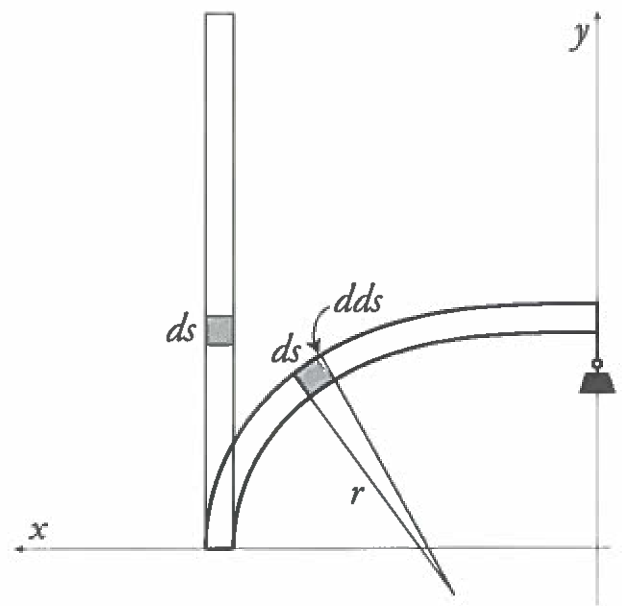

Figure 8.1: Jacob Bernoulli's derivation of the differential equation for the elastica.

of the elastica to give a "most elegant" solution to the paracentric isochrone problem. Thus it seems reasonable to speculate that Bernoulli judged his investigations worthy of publication largely because of this application—that is to say, for its foundational import, in the same manner as how Leibniz valued his envelope rule so much more when it had foundational implications than when it was merely useful for optical applications (section 7.3.2). Admittedly, one may on the other hand point to some evidence that the problem of the elastica had intrinsic interest. In fact, Jacob Bernoulli (1687) asks for advice regarding elastic beams in his very first letter to Leibniz, before he has even mastered the calculus at all, let alone familiarised himself with the problem of transcendental curves. Loaded beams had also been discussed by Galileo (figure 8.2), though with reference to breaking points rather than shape.

8.3 The paracentric isochrone

The paracentric isochrone problem asks for a curve along which a frictionless particle under the influence of gravity recedes from a given point at uniform speed (figure 8.3). This problem was posed by Leibniz (1689a), at the end of an article in which he solved the much simpler problem of the vertical isochrone (see section 4.4.5), which has a simple algebraic solution. Quite clearly Leibniz was interested in the paracentric isochrone problem because it involves a difficult quadrature which cannot be reduced to standard ones—an elliptic integral, as it would nowadays be called. Among the many forms of the differential equation for the paracentric isochrone, perhaps the most interesting for

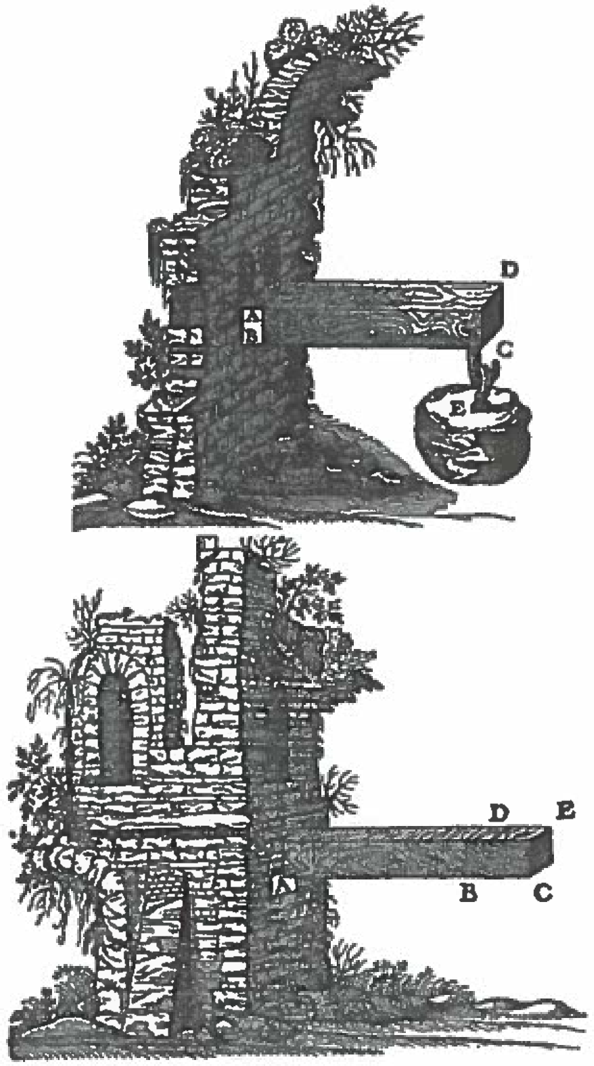

Figure 8.2: Beams in Galileo (1638), day 2.

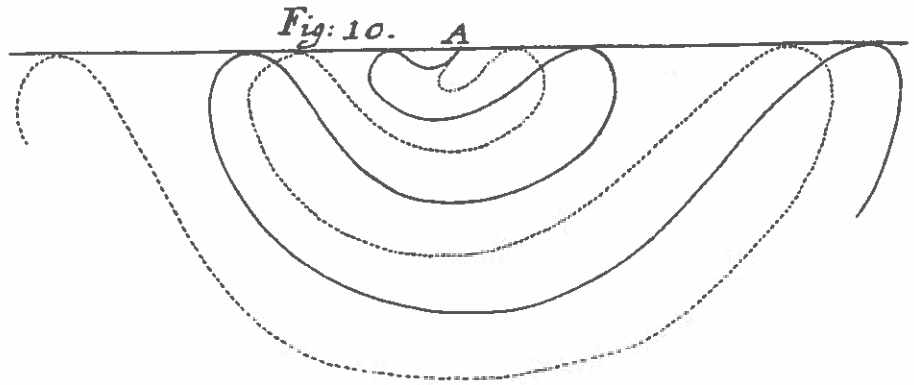

Figure 8.3: Two paracentric isochrones (receding from A at different constant rates). From Jacob Bernoulli (1695).

us, therefore, is the one that reveals its dependence on elliptic integrals in the purest way. This is best done using polar coordinates (r, θ). We shall now derive this differential equation following the method of Johann Bernoulli (1692b), although he used only rectilinear coordinates (x, y) and a kind of semi-polar coordinates (r, y). For my presentation I shall translate his derivation into polar coordinates. Let $(0,0)$ be the point we are receding from, and let the object have arrived at this point by falling a short vertical distance a. Recall from the second approach to the vertical isochrone in section 4.4.5.1 that energy conservation then implies

$$\frac{\text{speed at origin}}{\text{speed at } (x,y)} = \frac{\sqrt{\text{vertical distance fallen at origin}}}{\sqrt{\text{vertical distance fallen at } (x,y)}} = \frac{\sqrt{a}}{\sqrt{y+a}}$$

In polar coordinates the arc element ds is $\sqrt{dr^2 + r^2 d\theta^2}$, so the speed at any given point is $\sqrt{dr^2 + r^2 d\theta^2}/dt$. However, the radial speed (i.e., the speed directed away from $(0,0)$) is dr/dt. This is what is assumed constant in the hypothesis of the problem. But at the origin any speed is purely radial. Therefore the speed at the origin is the radial speed elsewhere, i.e., dr/dt. Putting this into the above equation, we get

$$\frac{dr/dt}{\sqrt{dr^2 + r^2 d\theta^2}/dt} = \frac{\sqrt{a}}{\sqrt{y+a}}$$

In polar coordinates, $y = r\cos\theta$. Inserting this and separating the variables gives

$$\frac{d\theta}{\sqrt{\cos\theta}} = \frac{dr}{\sqrt{ar}}$$

Thus the problem comes down to integrating $1/\sqrt{\cos\theta}$, which upon substituting $\tan\theta/2 = t$ becomes $2/\sqrt{1-t^4}$. This is arguably the most basic integral that goes beyond the standard repertoire of transcendental quantities in common use at the time, i.e., the quadratures or rectifications of circles or quadratures of hyperbolas—or, as we would say, trigonometric and logarithmic functions.

Thus the paracentric isochrone problem was a very natural way of pushing the boundaries of what was known about the representation of transcendental curves. One can be sure that Leibniz posed the problem for this reason, and not because it is of any particular interest from the point if view of physics. Indeed, Leibniz (1690b) hinted at this motivation for the problem:

> Problems of this sort, since they are not within the power of algebra or the commonly recognised calculus, will serve to rouse those who value too highly those things which they have learned, as if nothing of greater importance remains to be sought in these matters, not without detriment to the republic of letters by diminishing the diligence for that which is necessary to advance knowledge.[360]

The problem of constructing the paracentric isochrone did indeed give rise to foundational advances, as we saw in section 7.3.1.

8.4 The brachistochrone

The brachistochrone problem asks for the curve along which a frictionless particle under the influence of gravity descends as quickly as possible from one given point to another. The solution curve is a simple cycloid,[361] so the brachistochrone problem as such was of little consequence as far as the problem of transcendental curves is concerned. But certain secondary aspects of the brachistochrone problem turned out to be of greater relevance in this regard, as we shall see.

The brachistochrone problem is exceptional among the physical problems we have encountered in that it was evidently pursued almost exclusively for its intrinsic interest and beauty alone. From the point of view of method the greatest novelty of the problem is that it seeks an optimal curves as opposed to an optimal value or point, as in traditional optimisation problems. This point was stressed by Leibniz (1697e). Johann Bernoulli (1697e) also understood well that Leibniz was driven by systemic considerations and tried to use promises of such rewards to lure Leibniz to consider the problem:

> For it is worthy of your application, because it will perhaps present an opportunity for new speculations regarding curves.[362]

But the main reason for pursuing the problem remained the fact that it is "an exceptionally beautiful problem,"[363] as Leibniz (1696c) put it. Leibniz (1696b) even writes with regret of its seductive allure:

> The problem is surely the most beautiful, and it draws me reluctantly and resistingly to it by its beauty, like the apple did Eve. For it is a grave and harmful temptation to me, impaired in strength and burdened by a mass of other things; so that I do not readily dare more things which require more intense labour of meditation.[364]

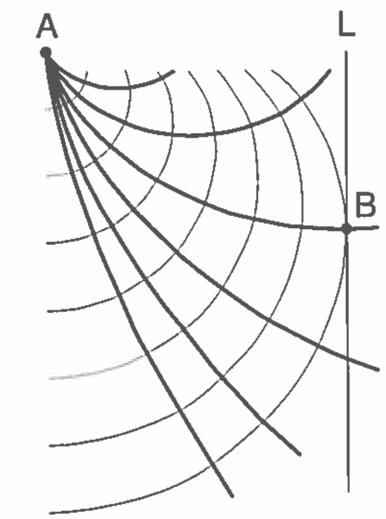

Figure 8.4: Brachistochrone (thick) and synchrone curves. B is the point on L that can be reached most quickly from A.

On this occasion Leibniz manages only to derive a differential equation for the solution, without realising that it is a cycloid.[365] When Johann Bernoulli (1696b) explains to him that the solution is a cycloid, Leibniz (1696d) is happy to have been enlightened:

> It was delightful for me to see the agreement of our solutions to the problem proposed by you [i.e., the brachistochrone problem]; with the same curve, although we gave different constructions. I was content to discover how the curve can be constructed by the squaring of the circle ... You progressed further and beautifully found it to be the same as the cycloid.[366]

The fact that even Leibniz could not recognise the simple and well-known cycloid in his own quadrature expression is a telling testament to the inadequacy and opacity of representations of transcendental curves involving quadratures.

A variant of the brachistochrone problem proposed by Jacob Bernoulli (1697b) is that of finding the curve of quickest descent from a given point A to given vertical line L. This problem is related to the concept of synchrones, i.e., the loci of points that take the same time to reach from A (see figure 8.4). If the synchrones are assumed known, the variant brachistochrone problem is easily solved: simply find the synchrone to which L is tangent, and then the point of tangency B will obviously be the point on L that is quickest to reach from A. This is problematic, however, for to find a point on a synchrone one must evaluate an integral expressing distance travelled along a brachistochrone curve, which in general cannot be done in closed algebraic form. Thus the synchrones are known only by a pointwise construction that requires the rectification of a different curve for each of its points. Leibniz (1696e) rather hastily took this to be an acceptable way of representing a transcendental curve, so he considered the problem