Enjoying Genius

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Review of: Joella G. Yoder, *Unrolling time; Christiaan Huygens and the mathematization of nature* (Cambridge etc.: Cambridge University Press, 1988; ISBN 0-521-34140-x), xi + 238 pp.

Joella Yoder presents her book as a case study in the mathematization of physics with one central person and one central event: "The person is Christiaan Huygens; the event is his creation of the theory of evolutes" (p. 1). Actually, she uses the event to link two rather diverse episodes in Huygens' work: on the one hand his studies on gravity and the dynamics of accelerated straight or curvilinear motion, and on the other hand his investigations of the curvature and arc length of curves. After the introduction, three chapters (2-4) deal with the first episode. They concern results Huygens achieved in 1659, which was truly his *annus mirabilis*. The highlights in this episode are his analysis of centrifugal force, his determination of the quantitative relation between centrifugal force and gravity, his use of that relation in the experimental determination of the constant of gravity, his study of pendular motion, his discovery of the relation (modern $T = 2\pi\sqrt{l/g}$) between the length of a pendulum and its period (for small oscillations), and his discovery and proof of the isochronism of the cycloid. Chapters 5-7 then pursue Huygens' investigations of a mathematical theme which he had encountered in his studies on curvilinear motion: the evolution ("unrolling") of curves. Yoder discusses Huygens' concepts, techniques and findings in the three closely related domains of evolutes, curvature and arc length, and relates them to the achievements of Apollonius, Van Heuraet, Leibniz and Newton. The eighth chapter gathers together some loose threads in the story, such as the sea trials of Huygens' clocks, the universal measure, the compound pendulum and caustics. Chapter 9 is the conclusion; then follow the notes, the bibliography and the index.

What I liked most in the book was the business of the parabola and the circle. It occurs twice at crucial points in Huygens' arguments, first in his derivation of the quantitative relation between centrifugal and gravitational force, then in his discovery of the isochronic path for pendular motion. It is what I consider to be a characteristically Huygensian insight. It concerns (pp. 19-22) a configuration as in Figure 1. BFG is a circle with diameter a and centre M; BL is the tangent at B. The radius MF prolonged intersects the tangent at E; C and A are the projections of F on the tangent and the diameter through B respectively. The curve FI (see inset) is the so-called involute of the circle at F, that is, the path

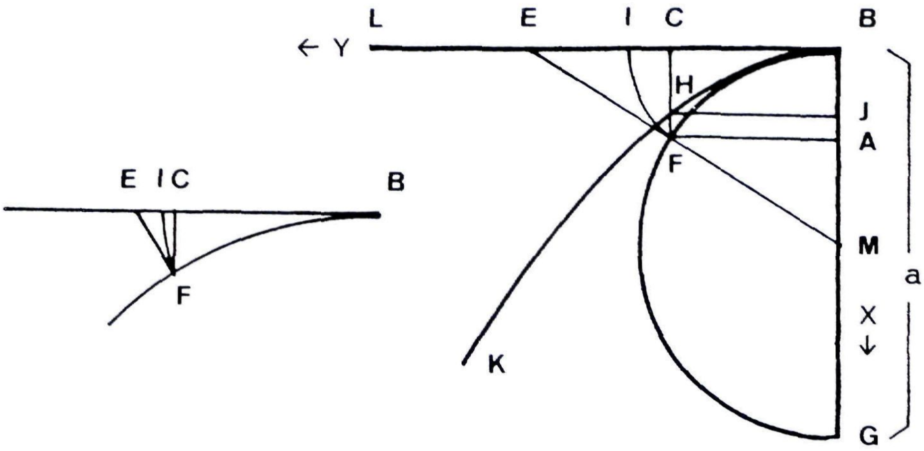

Figure 1 – The parabola and the circle

described by the end of a flexible, non-extensible cord initially wrapped along the arc *FB* and unwound (unrolled) until stretched as *IB* along the tangent; consequently *IB* = arc *FB*. *KHB* is a parabola with its vertex at *B*, its axis along *BM* and its *latus rectum* equal to the circle's diameter ***a***. This means that the equation of the parabola (with respect to the *X*- and *Y*-axes as indicated) is

$$y_P^2 = ax \ .$$

(The equation of the circle is $y_C^2 = ax - x^2$.) The parabola intersects *CF* in *H*; *J* is the projection of *H* on the diameter *BG*.

Huygens' insight concerned approximations that are permissible in this configuration when *F* is close to *B*. In what follows I analyse that insight by elaborating it in more detail than Huygens did himself. In particular I make explicit the approximate identities which he implied by letting the parabola and the circle coincide near *B*; in his own figure, for instance, no distinction is made between the points *H* and *F*.

Huygens realized that as long as the point *F* is sufficiently close to *B* the points *E*, *I* and *C* may be considered to coincide. In other words, the length *EC* may be considered negligible with respect to *BC*. This is a consequence of the fact that the tangent has the same direction in B as the curve. As a result one may argue as if $EB = IB =$ arc $BF = CB$, and also as if $EF =$ arc $FI = FC = AB$. Huygens also realized that another set of approximations applied: for the parabola in the figure one may consider F and H to coincide if F is close to B. In other words, the distance HF may be considered negligible with respect to CF. This is a consequence of the fact that the parabola with *latus rectum* a has at its vertex the same curvature as the circle with diameter a. As a result one may consider J and A to coincide as well and one may argue as if $BJ = BA = CH = CF =$ arc $IF = EF$.[1]

These approximative identities gave Huygens the key to understand the qualitative nature of centrifugal force and its quantitative relation to gravitational force. His arguments may be rendered as follows. Let F represent a body moving uniformly along the circle, and let the corresponding points E, I, C move along the tangent; likewise the points J and A move along the axis and the point H along the parabola; during these motions each point maintains its position with respect to F as in the figure. The approximative identities then imply that during a sufficiently short time-interval after the body has passed B, the motion of C may be considered as uniform (because F's motion is uniform, so I's motion is uniform, and I and C may be considered to coincide). Now when C moves uniformly along the tangent, then, because H is on a parabola, the corresponding motion of J along the axis is uniformly accelerated (or, as Huygens termed it: in equal time-intervals starting from the beginning of the process, the distances traversed by J are as the successive odd numbers $1 : 3 : 5 : 7 : \ldots$). Hence the motion of A is also uniformly accelerated (because A and J may be considered to coincide).[2] Now BA may be considered equal to arc FI, and arc FI is the deviation of the body from its circular path which would occur

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1 The validity of these approximations is easily (though anachronistically) checked by expressing the various lengths as powerseries in terms of $\theta = \measuredangle FMB$; one then finds that BC, BI, BE, arc BF, and FA can all be written as $\frac{1}{2}a\theta + O(\theta^3)$; and EF, arc IF, CF, CH, BJ and BA as $\frac{1}{4}a\theta^2 + O(\theta^4)$.

2 Note that Huygens here uses the approximate identities to conclude that not only the moving points themselves, like C and I or A and J, may be identified but also the type of motion (uniform or uniformly accelerated) they exhibit. The inference is indeed warranted because the approximations involved (as appears from the calculations in note 1) are of the second order. Hence not only the velocities of the approximately equal points may be considered equal but also their accelerations, and thereby their type of motion. Huygens did not explicitly note the strong character of the approximations. Yet it would be natural for him to realize that, for instance, both the ratios $CE : CF$ and $CF : CB$ can be made arbitrarily small by moving F towards B, so that CE is of second-order smallness with respect to CB. Identifying C and E is therefore a second-order approximation and such a realization may well have led Huygens to intuit that the identification can be extended to the type of motion exhibited by the points C and E – and similarly in the case the other identifications. Even if remaining largely implicit, these insights clearly illustrate the extraordinary power of Huygens' geometrical intuition.

if, at B, rather than continuing its circular motion, it were set free and proceeded, by its inertia, along the tangent with the same speed. That deviation, therefore, is the effect which the centrifugal force would bring about if set free. Since the deviation may be considered equal to BJ, it increases (at least during the first intervals of time) in a uniformly accelerated way, that is, in the same way as the distance traversed during fall from rest according to Galilei's law of fall. Thus both centrifugal force and gravity tend to produce (if set free) uniformly accelerated motions. Therefore, Huygens argues, centrifugal force is qualitatively of the same nature as gravity and consequently the two can be quantitatively compared. For this comparison, again, the approximative identities provide the key. They enable Huygens to determine, given the diameter a of the circle, the period T (and equivalently the velocity v) of the circular motion which produces a centrifugal force equal to gravity. Consider the case in which the motion of A is initially the same as actual fall motion. Then, because arc IF = BA, the centrifugal tendency would, if actuated, initially produce the same deviations (arc FI) in the same time intervals as gravity (BA); so in that case the centrifugal force is equal to gravity. On the other hand, because initially A coincides with J, the parabola represents the relation between the distances (as BC) traversed by uniform rectilinear motion with velocity v, and distances (as BJ) traversed in the same time by fall motion. According to Galilei's law of fall that relation is a quadratic one, represented therefore by a parabola. That parabola coincides locally, near B, with the parabola of Figure 1; the two parabolas are therefore the same. Now in the time T in which the body completes a full revolution in the circle, a body moving uniformly with the same velocity traverses a distance equal to the perimeter πa of the circle. Consequently, by the nature of the parabola expressed in its equation, the distance S traversed in fall during time T satisfies

$$dS = (\pi a)^2 .$$

Hence

$$S = \pi^2 a.$$

The result, then, is this: If in circular motion with diameter a, the period T is equal to the time needed for fall from rest along a distance $\pi^2 a$, then the centrifugal force acting upon the body is the same as the gravitational force it undergoes when suspended.[3]

Now isn't that *beautiful*? And *masterly*?

Let me compare Huygens' reasoning with the present-day approach to problems about centrifugal force and fall motion. It seems an easy exercise in elementary classical mechanics to determine for which combination of radius and period the centrifugal force equals gravity. Equating centrifugal force, mv^2/r, to gravitational force, mg, yields $gr = v^2$; inserting $v = 2\pi r/T$ and reordering yields $T = 2\pi\sqrt{r/g}$. This corresponds to Huygens' result because the distance S traversed by a body in free fall during time T is given by $S = \frac{1}{2}gT^2$; inserting the expression for T we find $S = 2\pi^2 r = \pi^2 d$. Hence the required period T can indeed be characterized as the fall time through $S = \pi^2 a$. – A simple exercise, seemingly without much point, and easily solved. But the comparison is misleading, it only shows the dubious advantage of blind machinery over honest and conscientious intellectual toil. The machinery used in solving the exercise is analytical mechanics, including definitions of force as mass times acceleration, acceleration as second derivative of the time-place function (a vector function in the case of circular motion), the constant g of gravity, and a consistent choice of units and dimensions. None of these features was available to Huygens and each involves considerable conceptual profundity which remains hidden because the formulas are so familiar and readily available. Moreover, for Huygens this was no mere exercise; the result was the basis of his studies on fall, circular and curvilinear motion, and the connection he found between circular and fall motion gave him an essentially new and accurate method for experimentally determining the constant of gravity.

So how did Huygens come through, unaided by the tools of later analytical mechanics? The answer is: by the circle and the parabola. It was precisely the approximative identities valid in that configuration which served him where we use the definition of force and the calculus of second-order derivatives. It enabled him as it were to look into the depths of the instantaneous, to quantify non-actuated tendencies to motion and to extrapolate over finite time periods the circular and fall motions that coincide in the initial instant.

I find this exceedingly beautiful. And I can vividly imagine the stirring sense of power Huygens must have felt realizing that he had found the key, that he had bridged a gap, the feeling which made him quote Horace on the manuscript of 21 October 1659: "Freely I stepped into the void, the first" (p. 42, see also

[3] The result also yields the velocity v along the circle required for equality of gravitation and centrifugal force; $v = \pi a/T$, and hence v is equal to the velocity acquired in free fall along distance $\frac{1}{2}a$. - Yoder points out that it was only after working out the quantitative comparison with gravity that Huygens derived, from elementary geometry of the circle, the relation of centrifugal force to velocity and radius expressed in the modern formula $F = mv^2/r$ (p. 22, esp. note 15).

Figure 3.1 on p. 20). And then, imagine the thrill he felt when somewhat later (mid December 1659) exactly the same technique of alternating between the circle and its companion parabola also proved to be the crucial step in discovering (pp. 48-64) that the isochronic path of a pendulum was the cycloid.

My pleasure in re-experiencing these Huygensian insights was due to what I consider the most admirable aspect of Yoder's book: the clarity, faithfulness and care in rendering Huygens' mathematical and mechanical argument. The arguments as she found them, in the manuscripts, in the later publications, and in the versions of historians covering the same ground before her, were multiple, scattered, confusing, and partly unreliable. Yoder convincingly untangles this medley and presents her findings in an admirably clear and short manner. Thereby she enables the reader to really enjoy Huygens' argument and to appreciate its scientific quality and beauty despite the considerable distance, in time and especially in style, between his and our ways of reasoning.

There is more to be admired in this beautiful book. For instance the way Yoder uses the various values for the gravitational constant in Huygens' manuscripts to date the texts from the crucial period 21 October – end December 1659, and to reconstruct, almost day by day, the results Huygens found, by calculation or experiment, and the progress of his understanding and his techniques. Here the standard edition of Huygens' work, the *Oeuvres Complètes*, has often proved misleading; several relevant manuscripts are not incorporated and the texts of other manuscripts are split up and presented in an order defined by the editors' somewhat idiosyncratic policies; so to recapture the coherence of the sources was a laborious task requiring great expertise and familiarity with the material. One fine example of this detective work is Yoder's re-dating of a manuscript drawing of the conical pendulum and her argument, *contra* the editors of the *Oeuvres*, that in the first half of November 1659 Huygens actually constructed a conical pendulum clock and used it to achieve a better value of the gravitational constant (pp. 27-31, see in particular notes 28 and 31).

I also find the style of the book admirable: crisp in explanations, no tendency to superfluous wording, pleasantly personal and many a memorable sentence, such as:

> Moreover, Huygens' strong sense of individual worth would never have let him be a slave to another's ideas, and a system of calculation [Yoder refers in particular to Leibniz' preference for such systems], unlike the loose matrix of techniques that constituted seventeenth-century infinitesimal analysis, does render the user subservient. The clever gives way to the drone. (p. 175)

The book would be boring if it were uniformly admirable. *Unrolling time* is not a boring book; it produced in this reader feelings of pleasure and admiration but also of irritation and disagreement.

I was irritated when reading Chapter 6 on curvature. Yoder notes that there is a standard view of the history of curvature, going back to Zeuthen, which links it with evolutes and which cites both Apollonius and Huygens as originators. She quotes Boyer's formulation: "The concepts of radius of curvature and evolute had been adumbrated in the purely theoretical work on *Conics* of Apollonius, but only with Huygens' interest in horology did the concepts find a permanent place in mathematics" (quoted p. 98). She writes that the view implies "that Apollonius contributed to the history of evolutes and that somehow, in a manner never specified, the history of curvature leads from Apollonius through Huygens to Newton and Leibniz" (p. 98). This implication upsets her. So, to set the story right – and because analyzing the possible influences clarifies Huygens' contributions – she discusses the history of curvature and Huygens' role in it, arriving, at the end of the chapter, at the conclusion that Apollonius, Huygens, Newton and Leibniz "did not influence one another in any significant sense, and their isolation and the differences among their intentions are severe enough to undermine any arrangement of their works into a chronological progression that is meant to function as an outline for a history of curvature" (p. 114). The chapter is indeed a useful and clarifying account of the contributions of the four mathematicians in relation to curvature and evolutes, and of the very different questions that motivated their work. But in between Yoder shows her indignation with earlier historians so often[4] that I became irritated. I think she fights phantoms. Actually the "standard outline" did not claim influences between the chronologically arranged instances of mathematical studies related to curvature; in fact, there is little interest for either influence or motivation in that kind of historiography. Significantly, the grammatical subject in Boyer's sentence is mathematical concepts; they are the active agents in the underlying, mainly tacit, view of the history of mathematics. One may summarize that view, slightly overstated for the sake of argument, as follows: The mathematical concepts, techniques and theorems, not content with a serene existence in a Platonic world, endeavour to be understood by mortal mathematicians; in the course of time they "emerge," abruptly or by degrees. In doing so they grace the mathematicians in whose work they are adumbrated or explicitly realized with the privilege and honour of being associated with their history; the highest honour being that of having one's name attached to them. (And the historians dispense the honours.)

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[4] "Unfortunately, unlike modern commentators ..." (p. 99); "Why does the traditional scenario fail?" (p. 102); "However, if the standard history of curvature is correct ..." (p. 102); "The traditional history implicitly assumes that ..." (p. 102); "Once again the traditional history of curvature errs ..." (p. 108).

Yes, that view is superseded, in fact it takes some effort to realize that it once was convincing and even inspiring. Yoder's book is a good example of the style that replaced it: explicit attention to motivation, influences and context, assessment of earlier mathematics and science primarily in their own terms. But why get upset? Though superseded, the approach was consistent and it produced great works, still very valuable for obtaining insight and an overview, and not harmful as long as one is aware of the special interest of their authors. I feel that the literature of that style deserved a more mature methodological criticism than mere indignation and exposure of its blind spots.

I disagree with Yoder with regard to her assessment in Chapter 7 of Huygens' theory and technique of curve rectification. To rectify a curve means to determine the arc length of that curve between two given points on it. Huygens' achievements concerning rectification are part of his theory of evolutes. Yoder repeatedly describes these achievements as more complete and general than the contemporary methods of rectification, for instance that of van Heuraet. She writes: "Huygens emphasized the universal applicability of his method by providing rectifications, redone according to his own technique, for *all* the important geometric curves of the seventeenth century" (p. 129); and, explicitly comparing Huygens' with Van Heuraet's method, she says of the former: "Yet how much more valuable is a universal method that can yield an answer directly" (p. 129); and again: "The achievement was Huygens' alone. Not only was the arc length directly and universally attainable, it was dependent upon a technique that stemmed from his greatest mechanical invention" (p. 130).

Such claims make curious reading. "Universal applicability" suggests a theory providing the arc length of any curve. With modern hindsight that means that the theory must be powerful enough: 1) for any curve to determine the equivalent of the integral that expresses the arc length, and 2) to determine the value of that integral. Item 1), taking "equivalent" in a broad sense, and assuming the possibility of determining tangents to any curve, is what Van Heuraet did (as adequately described pp. 125-126), and what Huygens might have done, had he been interested; but Yoder convincingly argues that Huygens was not primarily interested in it. Item 2) cannot be credited to any seventeenth-century mathematician, since the problem is still unsolved at the present time. To be more specific: a universal theory of rectification should, for instance, provide the rectification of the ellipse; that rectification involves an elliptic integral, and elliptic integrals are notoriously undeterminable unless by approximation or by fiat.

So what *is* Huygens' theory of rectification about which these claims of universality are made? Yoder explains that clearly enough: Huygens' theory provides the means to determine for a given curve C its so called "evolute" $E(C)$, which is the curve by whose "unrolling" C can be described (in Figure 1, inset, the circle arc BF is the evolute of the curve IF). Huygens realized that if C and $E(C)$ are given one can easily rectify $E(C)$. (In the Figure: If BF and IF are given, BF can be rectified, so arc $BF = IB$.) Thus for every curve C Huygens' theory supplies another curve $E(C)$ which is rectifiable.

But for the theory of evolutes to count as a universal theory of rectification one needs the converse of what Huygens supplied; for any curve E to be rectified, a curve C should be determined such that E is the evolute of C. This, however, Huygens did not and indeed could not do because determining such a curve C is equivalent to determining the value of the arc length integral for the curve E, that is, equivalent to item 2) above. Yoder's statement that he "provided rectifications redone to his own technique, for all the important curves of the seventeenth century" is, if not downright wrong, strongly misleading. Huygens did not rectify these curves, he rectified their evolutes. But these evolutes were special curves and Huygens could hardly have thought that by determining ever more evolutes he would encounter all the common curves.

The final page of Chapter 7 adds confusion to disagreement on this issue. Yoder there seems to acknowledge the essential restriction of Huygens' method of rectification:

> It was the companion curve that was rectified, not the given curve. Although innumerable arc lengths could be measured in this way, given a specified curve, it could not be rectified by this method unless its involute [that is, the curve whose evolute it is] was recognizable (p. 147).

However, she merely calls this a "flaw" and a "weakness," and she seems to suggest that it could be eliminated by the antiderivative approach to integration (which is the same as claiming that by the antiderivative approach one may determine any integral). And again she claims that Huygens gave "the first general technique for actually measuring the length of curves rather than merely transforming them" (p. 147). I disagree; what was general in the theory was the idea of unrolling and the determination of evolutes; actually measuring the length of curves required much more. I think Yoder has uncritically adopted Huygens' own high opinion of his theory. The danger with her presentation is that in future literature on rectification of curves we may, on her authority, have Huygens as the creator of the first complete theory of rectification. He was not. He created a full theory of evolutes, not of rectification. – So far irritation and disagreement, but certainly pleasure and admiration prevail.

This has become a very mathematical review. Should I apologize? Or should I at least have issued a warning at the beginning, as Yoder carefully does (p. 7): "Mathematics ahead!"? But this is a very mathematical book. Although "the mathematization of nature" also involves nature and the process of bringing mathematics to bear on it, these two aspects turn out to be of much less importance than mathematics itself in understanding Huygens' achievements. Nature enters Huygens' investigations in two ways, through his experiments and through his world view. Yoder argues that, although Huygens was a skilful observer and experimenter, "his greatest successes were those achieved with a pen, by a man possessed of a great logical facility, a born mathematician" (p. 170). Huygens' world view was mechanistic: he conceived kinematic and dynamical phenomena in terms of matter and motion. But once that view had provided him – along more or less conclusive ways – with a suitable starting point, he formulated that starting point as an axiom, and proceeded mathematically while the mechanistic model receded into the background or disappeared altogether. The question of how far Huygens followed others – notably Descartes – in adopting a mechanistic world view turns out to be of little relevance for understanding his achievements (pp. 35-36). Nor does Yoder find that his particular way of bringing mathematics to bear on nature can be explained by any conscious research programmes or metaphysical commitments (pp. 169-170).

So is there only mathematics in the book? No, there is also, pervasively, Huygens himself. In the concluding chapter Yoder focuses on Huygens the person and relates his choices and strategies in mathematizing nature to aspects of his personality. Searching for recurrent patterns in his professional behaviour she steps outside the cadre of the two episodes of accelerated motion and evolutes. She finds such patterns for instance in his studies of the catenary curve, prompted first by Mersenne and later by Jakob Bernoulli, in his discussions with Leibniz on the calculus, and in the contrast between his approach to the centre of oscillation and Hooke's. An image arises, sketched in a few broad lines but no less convincing, of a man strongly dependent on outside stimuli. Huygens preferred to take up prestigious problems posed by others, and even though his solutions generally had much more scope than the original problem required, he remained primarily a problem solver (p. 174) rather than a builder of grand theoretical syntheses.

This sketch of Huygens the person is helpful and adequate for this study of his achievements concerning accelerated motion and evolutes. Yet *Unrolling time* makes the reader want to know Huygens better, as a scientist and even more as a person. The ease with which Yoder in the last chapter adduces evidence from other periods of Huygens' scientific career shows her mastery of the Huygensian material. Indeed, we may soon expect the publication of Yoder's complete description of the Huygens manuscripts, the result of several years of intensive archival research and promising to be indispensable for those who want to study Huygens without being restricted to the labyrinthine *Oeuvres Complètes* for material. It is a safe guess that among living scholars Yoder is the one who knows Huygens best. I sincerely hope that she will write a full-scale scientific