Chapter 4
Why Historical Research Needs Mathematicians Now More Than Ever

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Abstract Using the history of the calculus as an example, I identify some trends in recent scholarship and argue that the time is ripe for a "new internalism" in the historiography of mathematics. The field has made steady progress in the past century: mathematicians have provided clear expositions of the technical content of past mathematics, and historians have produced meticulous editions of textual sources. These contributions have been invaluable, but we are reaching a point where the marginal utility of further works of these types is diminishing. It is time to shape a paradigm of historical scholarship that goes beyond the factual-descriptive phase of the past century. Comparative interpretative work is now feasible thanks to the gains of the past century. Cognitive questions about mathematical practice provide a fascinating and underexplored avenue of research that we now have the tools to tackle. Mathematically trained researchers are needed for this enterprise.

4.1 Introduction

Today is a golden moment to reunite historians and mathematicians. Their acrimonious divorce some decades ago is proving increasingly detrimental to both. Mathematicians sit on technical expertise and are as interested as ever in history, but they are spinning their wheels with repetitive expository accounts, since no historiographical framework helps them mobilise their skills for historical research purposes. Historians have shut themselves off from mathematicians to avoid anachronism, but forget that, while this asceticism may once have been a sound cleanse, it is unsustainable as a permanent diet.

The work that the two divorcees have done while apart is a perfect foundation for their reunion. Retreating to their individual comfort zones, scholars perfected the state of local scholarship in those domains. But we cannot keep tinkering in

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fragmented niches forever. With the accumulation of detailed studies, we are now in a position to take up new lines of research based on synthesising and comparative perspectives.

Let me take an area I have worked on—the early history of the calculus—as a case in point to highlight the fruitful circumstances that make a new internalist historiography more opportune than ever.

4.1.1 Opportunity: Re-engage Mathematicians in History of Mathematics

The history of the calculus remains highly relevant to the mathematician's world-view, as seen for instance in recent high-profile books where the history of calculus features prominently, such as Strogatz (2019)—a *New York Times* Bestseller—or Bressoud (2019)—an interweaving of historical and educational aspects of calculus by a former President of the Mathematical Association of America. But, regrettably, in terms of historical content, these books are less groundbreaking, relying in large part on a rather limited set of historical set-pieces that are often repeated in one popular work after another. The title of one very successful book of this type—"The Calculus Gallery" (Dunham 2005)—inadvertently hints at the limitations of this approach: historical mathematics is reduced to a canonised collection of iconic snapshots, briskly toured under fluorescent lights; seen only on their aloof pedestal rather than in the creator's workshop. Left unanswered are questions about how the technical details of particular mathematical masterpieces were organically embedded and functioned in broader research practice.

All of these authors are highly qualified mathematicians, yet their competence is wasted on repetitive re-exposition of known material because mathematically inclined authors lack—and do not find in recent historical scholarship—any sense of direction in which history of mathematics as a research field could evolve through the kind of analysis that a mathematician can provide. "The early history of the calculus of variations is a well-beaten track" (Giaquinta and Freguglia 2016, vii) another recent book apologetically admits, before beating the same track once again. There is a wealth of fascinating and unexplored historical questions that mathematicians could very fruitfully address, but mathematicians do not know how to do new and valuable scholarship by asking novel questions about the technical substance of past mathematical practice. We need a new historiography to provide this lacking impetus, and thus rejuvenate history as a mathematical research field.

4.1.2 Opportunity: Recent Historical Scholarship Abundant in Details but Lacking in Global Vision

Current scholarship in the history of calculus is lopsided toward specialised source studies. The Newton Project and *Akademie-Ausgabe* of the works of Leibniz are epicenters of expertise in the field. By providing comprehensive and meticulous editions of sources, these projects are invaluable. But their success inherently contain the germ of a fresh start in a different direction: the excellent state of specialised source work opens the way for synthesising perspectives.

This is timely, as for the history of the calculus (as for many other historical topics) no comprehensive and accessible survey that synthesises the insights of recent research and points the way to future research has been written for generations. Highly dated books such as Edwards (1979) are still in print and widely used; the antiquated Boyer (1959) is still Amazon's top hit for "history of the calculus" and no up-to-date alternative is available. This is doubly unfortunate. For historical scholarship itself, it shows that increasing specialisation has left the field lacking in big-picture vision. Furthermore, for students and mathematicians, the lack of accessible overview is a gatekeeping barrier that makes it very difficult to keep up-to-date with recent historiography and enter the field of historical research. This blocks mathematically talented people from contributing to the field, and hence the sense that modern historical scholarship is divorced from mathematics becomes a self-fulfilling prophecy.

4.1.3 Opportunity: Join Forces with the "Practice Turn" in Recent Philosophy of Science and Mathematics

Not only mathematicians can profitably be re-invited into historical scholarship, but also philosophers. Again the timing is just right. In the twentieth century, much philosophy of mathematics was fixated on logical rigour. In the case of the history of the calculus, this meant for example many papers on the relation between Robinson's nonstandard analysis and classical infinitesimal calculus—an anachronistically motivated debate that is orthogonal to the concerns of historical mathematicians. But with the more recent "mathematical practice" movement in philosophy of mathematics, philosophers have turned to questions regarding the motivation, methodology, heuristics, and research choices of historical mathematicians, as well as cognitive-historical questions such as for instance how visual and notational devices shape styles of thought. Hence the interests of the historian and the philosopher are more favourably aligned now than in the past century.

4.1.4 Opportunity: Current Societal-Educational Questions Turn on Calculus

A new historiography has the opportunity to be inclusive in another important direction as well. Again the history of the calculus provides a case in point. Calculus has an image problem, now more than ever. It was never a crowd-pleaser to begin with, but the old student refrain “when will I ever use this?” has lately been gaining considerable traction among senior academics as well. An October 2019 *Freakonomics* episode joined a growing chorus that would be happy to see dusty old calculus yield space in the curriculum for more “twenty-first-century skills” such as “data fluency.” As books such as Strogatz (2019), Bressoud (2019), and Orlin (2019) indicate, history is one of the mathematician’s best tools for conveying the relevance and excitement of calculus amid such assaults. What Heilbron (1987, 559), says of science is true for mathematics as well: as “applications threaten to suffocate the traditional core of the subject”—a core “informed by the humanistic ideal”—“partnership with history may be the most promising course by which science may save itself from being crushed by its technological successes.”

We historians must find a way to build on all of these opportunities constructively, rather than isolating ourselves to uphold a puritanical ideal of our subject.

4.2 Example: Huygens’s Proto-Calculus

Figure 4.1 outlines a mathematical argument from the works of Huygens that is quite typical of its time. The first thing that strikes a modern reader is the geometricity of the proof. Indeed, one may say that “Huygens actually thinks geometrically, he sees the relations in the figures, formulas are secondary to him,” as Bos (1980, 132), observed in a different context. But this is merely a descriptive observation. We want to dig deeper and consider the ramifications of this point of view for the mathematical practice of the time. Compared to calculus proper, what were the cognitive possibilities and limitations of this style of proto-calculus mathematics?

Many aspects of Huygens’s argument can be matched with analogous notions within the calculus: geometrical properties of tangents play the role of derivatives; inferring “global” properties of the system from a characterisation of all its instantaneous local states plays the role of integrating a differential equation; using a circle as a reference figure plays the role of using trigonometric functions to express the quantities and relationships involved. To what extent were these proto-calculus analogs functionally equivalent to their calculus counterparts? In some respects they could do everything the calculus can do; in other respects not. What respects are these exactly?

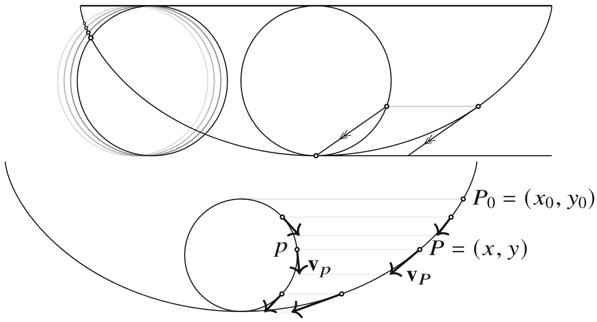

Fig. 4.1 Top: Definition of cycloid as the curve traced by a fixed point on a rolling circle, and the tangent of the cycloid expressed in terms of its generating circle in its middle position. Bottom: Huygens's proof that the period of cycloidal motion is independent of amplitude. The particle P is descending under gravity along a cycloid, starting from rest at P_0. Consider the horizontal projection p of this point onto an associated circle as shown. From physics we know that $|\mathbf{v}_P| = \sqrt{2g(y_0 - y)}$. From the tangent result shown on the left we know how to decompose this into vertical and horizontal components. By definition, $\mathbf{v}_p$ has the same vertical component, and is tangent to the circle. This determines the magnitude $|\mathbf{v}_p|$, which turns out to be constant throughout the descent and proportional to y_0. Hence the time of descent of P has been expressed in terms of the arc length of the circle. From there it immediately follows that the time of descent is the same for any P_0

In other words, what exactly did the calculus add that was new compared to these existing practices? For example:

- Did the calculus remove the need for the geometrical ingenuity exhibited by Huygens, and replace it with routine applications of symbolic-computational rules? Leibniz often stressed the value of his calculus in such terms, but he is a biased witness. Did arguments such as that of Huygens truly rely greatly on geometrical ingenuity and imagination, or is that merely how it appears to someone without a working knowledge of this style of mathematics?
- Does a calculus solution to a particular problem carry over more easily to a similar problem while Huygens-style geometrical proofs are sui generis? Euler (1736, 2), thought so. Can his opinion be validated by a comparison of pre-calculus and calculus historical sources, or did Euler only feel this way because he was more familiar with calculus methods?
- Did the calculus provide the tools to state general theorems about, say, entire classes of functions, whereas Huygens-style methods are limited to specific, concrete cases? An argument against this hypothesis, perhaps, is for example Huygens's completely general proof that the evolute of any algebraic curve is itself algebraic (Huygens 1673, III, Prop. XI).
- Is Huygens's approach damagingly dependent on working with "global" properties of entire figures and systems, whereas the calculus can successfully operate in the dark with local (differential equation) information and only need to interpret the final solution globally at the end, if at all?

- Did the calculus facilitate these kinds of problems primarily by brute-force power ("crunching the formulas"), or as a conceptual heuristic and a way of thinking about how to even formulate the problem in the first place? The latter point of view is perhaps what is captured by the paraphrase by Arnol'd (2012, vii), of a Newtonian maxim as "it is useful to solve differential equations."

4.2.1 Mathematical-Practice Historiography

Questions like those above may be called cognitive, to distinguish them from what is purely textual or factual. Cognitive questions concern how certain ideas functioned in the minds of historical thinkers, and what overall role they played in their mathematical thought: What could these ideas do, and what not? What was the lay of the land of mathematical research as seen through the lens of these ideas? How did outlooks such as that of Huygens and that of Leibniz differ in how they drew the boundaries and the infrastructure connections between the well settled, the active frontiers, and the aspirational *terra incognita* on the research landscape map?

Cognitive questions cut to the heart of what makes history relevant for many audiences. Mathematicians are drawn to these questions because they concern reconstructing past mathematics as it appeared through the eyes of active researchers. Mathematics teachers and students, because these questions point to a path of hands-on examples from which a mature view of the field gradually crystallises. Philosophers, because these questions trace the formation of fundamental concepts. Historians, because these questions target precisely what was idiosyncratic and uniquely situated about past ways of thinking.

But cognitive questions are elusive, since they try to get at thought processes that are beneath the surface. They cannot be answered by a purely textual analysis of source documents (the expertise of historians), nor by a purely formal analysis of the mathematical content (the expertise of mathematicians). Tackling cognitive questions therefore requires new historiographical methods that go beyond established practices of historians and mathematicians, but build on the strengths of both.

4.3 Need to Move Beyond "Photorealism" Historiography

The historiography of mathematics is stuck in a binary that for the past decades have pitted mathematicians and historians against each other in cartoonish terms. In what is by now a tired cliche, historians condemn the mathematicians' practice of utilising modern mathematics to analyse and illuminate historical works. Portraying this as the root of all evil, historians prided themselves on banning mathematical paraphrase and restricting themselves entirely to literal scrutiny of textual sources. This was in some ways a corrective in the right direction at the time, but it should not be mistaken for the endpoint and perfection of historiographical method. The simplistic good-versus-evil self-fashioning of the present consensus has become such a dominant narrative that past generations of mathematically oriented historical scholarship is now routinely dismissed as “at best anachronistic” (Imhausen 2021, 80).

Ultimately this point of view is as sterile and one-dimensional as taking hyperrealistic still lives to be the endpoint and perfection of art. Breaking free from the stifling ideal of photorealism allowed artists to better see the organic soul of their scenes and capture their human significance with greater emotive force. The same will happen in the history of mathematics. Liberating ourselves from the moribund “still life” textualist historiography, we will bring out what is less tangible but more vividly alive.

To be sure, reconstructing past mathematical thought is a tightrope walk that has long been difficult to get right. But Chang (2017) is right to subdivide internalist history of science into an “orthodox” and a “complementary” mode. “Orthodox” internalism is subservient to the current values of the field whose history is investigated, whereas “complementary” internalism is pursued precisely because it complements current orthodoxy in the field and regards it critically. Traditional internalist-mathematical approaches have been too often carelessly dismissed by historians based on arguments that in reality strike only against orthodox internalism.

It is true that mathematicians can be too cavalier in projecting modern notions onto past mathematics, as when Arnol’d speaks of Huygens investigating “the manifold of irregular orbits of the Coxeter group H_3” (Arnol’d 1990, 8). Of course, such approaches are likely to be insensitive to historical thought, and to bulldozer over its nuances with predetermined ideas and unwarranted extrapolations. (Examples of episodes from the history of the calculus that have been misunderstood for such reasons are discussed in Blåsjö (2015), Blåsjö (2017a), and Bell and Blåsjö (2018).) This is why it has become *de rigueur* among historians to insist—as for example the most prominent historical monograph on Huygens’s mathematics immediately does—that “historical accuracy and insight are lost when results are couched in modern terms” (Yoder 2004, 7). This may be called the photorealism axiom of modern historiography.

The insistence on “photorealism”—or exact adherence to the surface form of the written text—has been a blessing and a curse for the historiography of mathematics. This ban on paraphrase has cleansed the field of many a naïve anachronism, as intended. But less widely recognised are its unintended knock-on effects. Photorealism effectively precludes comparative, synthesising studies, and hence forces a fragmentation of historical scholarship into narrowly specialised studies. This is an overreach of the photorealism axiom that goes beyond its originally intended scope and justification. New vistas for progress would open up if comparative and synthesising analyses could be rehabilitated as historical methods without reversing the gains made by the photorealism phase of the past decades.

4.3.1 Consequence of "Photorealism" Historiography: Microscopic Focus on Minor Sources

Photorealism historiography predictably steers the field into an arms race of hyperspecialisation. Comparative, synthesising perspectives necessarily go beyond the textual surface and are hence at odds with photorealism, whereas celebrating previously neglected textual specifics is the bread and butter of this historiography. Thus, predictably, the recent literature is heavily lopsided toward detailed studies of such things as the unpublished views on infinitesimals of a historical figure whose English Wikipedia page consists of two sentences (Domingues 2004), or how Leibniz once made a computational slip (using nine leading zeroes instead of eight in the decimal representation of a fraction) on a piece of scrap paper when he was trying to estimate e numerically from its power series (Probst and Raugh 2019). Such an increasingly microscopic focus has greatly improved local precision and expertise in historical scholarship, but with an exclusive focus in this direction the field is left without purposeful vision on a more global scale.

4.3.2 Alternative: Global Cognitive Contextualisation

Let us take the example of Leibniz's calculation of e and consider what new questions we would ask about this episode from a practice-oriented cognitive perspective.

As Probst and Raugh (2019) observe, Leibniz's manuscript appears to have been the first explicit occurrence of the numerical value of e. But what was the significance of this to the mathematical practice at the time? The natural logarithm and e eventually became fundamental in mathematics, but what did this enable mathematicians to do that they couldn't do before? In the context of the calculus, $\ln(x)$ is "natural" by virtue of being the logarithm function with the simplest derivative, but seventeenth-century calculus often reasoned in terms of proportionality and dimensional homogeneity, which arguably meant that there was no marked preference for $1/x$ over a/x. Is the modern canonisation of $\ln(x)$ and e merely cosmetic, or does it have cognitive import? If so, in what way, and did Leibniz see it that way?

Leibniz used power series for his computation, but sophisticated computational techniques for logarithms had been around since before Leibniz was even born. Already in 1622, Speidell gave a table of genuine natural logarithms for all integers from 1 to 1000, agreeing with the modern $\ln(x)$ to six decimal places, though the table omits the decimal point (Cajori 1991, 153; Speidell's work is now available at *Early English Books Online*). How does the calculus-based power series paradigm compare with earlier computational practices such as those for logarithms? Did the new paradigm excel compared to earlier techniques by efficiency, extension, unification, or simplification? How easily could earlier mathematicians such as

Speidell have repurposed their algorithms to compute e if they had wanted to? Since base-10 and base-e logarithm tables were available in print half a century before Leibniz, and since calculating $e = 10^{\lg(2)/\ln(2)}$ is readily reduced to such tables, the numerical value of e could in theory have been looked up in five minutes in a good library in Leibniz's time. Would contemporaries of Leibniz well-versed in established logarithmic practices have regarded computing e (once defined) as routine? More generally, how were calculus innovations parsed in relation to established proto-calculus practices, and how does the significance of key calculus concepts differ from modern perceptions when read through such a lens?

To answer such questions we must be attentive not only to specifics of individual documents such as Leibniz's e manuscript; we must also understand the overall scope of the know-how of logarithmic functions established in the mathematical practice of the time. Only a synthesising, comparative perspective could ever answer to this purpose.

The Leibniz e episode also raises intriguing questions about the relation between concrete calculations and curve plotting on the one hand and abstract theory on the other. As Probst and Raugh (2019) observe, Leibniz's most immediate purpose with computing the numerical value of e was to plot the graph of the shape of a hanging chain, the catenary $y = (e^x + e^{-x})/2$. Was this a mere "after the fact" pragmatic way to draw a curve already found theoretically, or did visual and numerical checks play a role of verification that removed lingering doubts that the theoretical derivation may not have been correct? More generally, were these kinds of calculations and drawings used as an integral part of research itself, for verification or explorative purposes, rather like a modern mathematician may use a computer?

Again, these are issues that cannot be answered by zooming in on the details of isolated cases but only by a comprehensive analysis of patterns of thought. Fortunately, philosophers have recently been very interested in issues such as diagrammatic reasoning (e.g. Giaquinto 2007; Hanna and Sidoli 2007), so we are better equipped than ever with conceptual tools to help us in such an analysis.

4.3.3 Consequence of "Photorealism" Historiography: Overdependence on Novel Sources

Two of the most high-profile interpretative innovations in the recent literature on the history of the calculus are the claim that previously unpublished manuscripts by Leibniz suggest "a complete transformation of the prevailing view on the position Leibniz held on the foundation of infinitesimal techniques" (Rabouin 2020, 19), and the claim that when modern imaging techniques revealed some previously unreadable words on an ancient Archimedes manuscript, this was "a major discovery" that "made us see, for the first time, how close Archimedes was to modern concepts of infinity" (Netz and Noel 2007, 29).

In both cases, revisionist interpretations are based on previously unpublished documentary sources. Indeed, it could hardly be otherwise, given the exclusively textual focus of photorealism historiography. This may seem right and proper: it simply shows that our field is evidence-driven. Yet if this is the only game in town then the consequences will be predictably detrimental.

If publishing new sources is a precondition for publishing new interpretations, then the field closes itself off from the analytic insights of mathematicians and philosophers, who have unique expertise that they would not have been able to develop if they had devoted the bulk of their time to editing unknown texts. Meanwhile, historians who do excellent work on editions of sources automatically have their voice greatly amplified also on interpretative questions: a conflation of credibility in one domain with authority in another.

Furthermore, an addiction to extracting ever more from the increasingly depleted potential of hitherto unpublished sources, and a lack of alternative ways of making scholarly progress, dooms us to keep mining the archives for novelty with diminishing returns. One may say that a historiography that refuses to go beyond the directly textual is bound to enter a "fracking" stage toward the end of its life cycle, as researchers are pushed to find ways of extracting new discoveries from documents that previous prospectors had treated as unpromising.

4.3.4 Alternative: Rigorous Historiography for Evaluating Other Types of Interpretative Hypotheses

Just as physics thrives on an interplay of theorists and empiricists, so historical scholarship would benefit from a wide range of interpretative thought rather than regarding as exclusively legitimate that of those who personally work on publishing ever more novel source material. For this, we need new standards of assessing hypotheses that go beyond direct one-to-one correspondence with textual evidence. We need to shift the focal point of historical research from the microtextual to a more overarching level of patterns of thought.

4.3.5 Consequence of "Photorealism" Historiography: Overemphasis on Surface Form

The axiom that fidelity to historical actors' modes of expression is the same thing as fidelity to the conceptual essence of their underlying thought entails that differences between geometric and algebraic styles are ipso facto profound. Hence, predictably, modern historians have placed considerable emphasis on the work of the second generation of calculus practitioners, who opted for a more algebraic approach than the geometrical style of people like Huygens and Newton. This was “a monumental conceptual shift” (Shank 2018, 234), “a major step that cannot be overestimated” (Speiser 2008, 108), modern historians assert. But it is hard to escape the impression that these claims are driven more by historiographical commitments than by analyses of mathematical practice. For example, Shank (2018) opens with a long and detailed chapter on the historiography of mathematics but has no technical discussion of actual mathematics in the entire book.

4.3.6 Alternative: Practice-Based Assessments of Importance

We need a new approach that neither erases differences between geometrical and algebraic approaches through anachronistic translation into modern mathematical terms, nor assumes that such differences are necessarily conceptually profound. I suggest that to answer these kinds of questions is to build up a comprehensive picture of what for instance Huygens’s methods discussed above could and could not do. Only through such an overall sense of what it was like to wield these tools as research weapons can we understand the significance of the technical details of an argument such that by Huygens. And only through a detailed, comparative study of many specific examples can we build up such a general picture.

4.3.7 Conclusion on “Photorealism” Versus Cognitive History

All-out war on anachronistic paraphrase has not only eliminated the intended culprits but also inflicted additional casualties: comparative perspectives and mathematically insightful commentaries face a hostile climate under the new regime, and mathematicians and philosophers are alienated from the field. Left are historians playing it safe in the wake of this ideological purge, limiting themselves to the most directly textual domains of scholarship: narrowly specialised studies and archival work on unpublished sources. The field is losing vigour, like a person who avoids food poisoning only at the cost of suffering severe malnutrition.

Cognitive history shows how digging into mathematical practice *in media res* and asking new questions can be both mathematically and historiographically exciting. It reconstructs the living, bustling research scene of the time—the hopes and dreams and conundrums and technical obstacles that these historical mathematicians wrestled with in their daily practice. Consequently, instead of having to dig ever more deeply into the archives for fresh kicks, cognitive history shows that scholarship on canonical sources is far from “done” merely by mathematicians having given technically accurate, local descriptions of their content, and historians having traced their documentary network. The field is ready to graduate beyond this basic descriptive phase of scholarship and to dare to pursue new comparative and interpretative perspectives.

4.4 Example: Newton's Unusual Quadrature Manipulations to Describe Inverse-Cube Orbits

Figure 4.2 shows how Newton described the orbits that result from an inverse-cube force law in his *Principia* (1687). As throughout the *Principia*, Newton's style is classical and geometrical. However, because this is one of the more complicated technical problems of the *Principia*, Newton had relied on calculus "behind the scenes" to arrive at these solutions, using the steps outlined in Fig. 4.3. For this reason, this is an interesting case study for clarifying the boundary between classical and calculus methods.

4.4.1 Historiographical Lessons of the Newton Example

The existing literature on this episode is very typical. As far as excellent technical commentaries and explanations of the steps of Newton's derivation are concerned, there is an abundance—or, one might almost say, oversaturation—of literature, including for instance Erlichson (1994), Brackenridge (2003), and Guicciardini (2016). The last of these declares itself "deeply indebted" (210) to the earlier ones, and indeed largely consists of re-exposition rather than novelty as far as technical analysis of mathematical content is concerned. With multiple articles showing so much overlap and rapidly converging to a consensus, one is bound to get the impression that this mathematical style of historical scholarship is effectively "done" and has little more to contribute. No wonder, then, that recent scholarship,

"If with centre C and principal vertex V any conic VR . . . is described, and from any point R of it the tangent RT is drawn so as to meet the axis CV . . . at . . . T; and . . . there is drawn the straight line CP, which is equal to . . . CT and makes an angle VCP proportional to the sector VCR; then, if a centripetal force inversely proportional to the cube of the distance of places from the centre tends towards that centre C, and the body leaves the place V with the proper velocity along a line perpendicular to the straight line CV, the body will move forward in the trajectory VPQ which point P continually traces out."

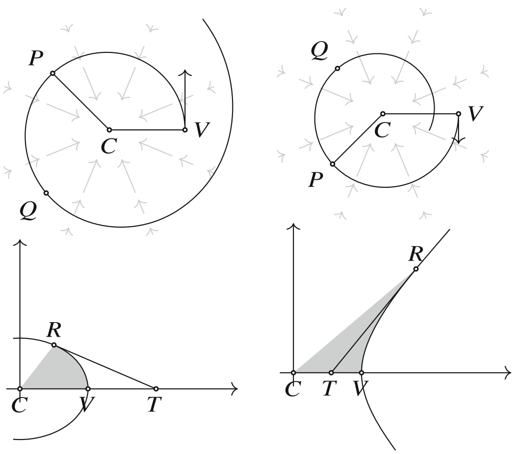

Fig. 4.2 Newton's description of trajectories in an inverse-cube force field

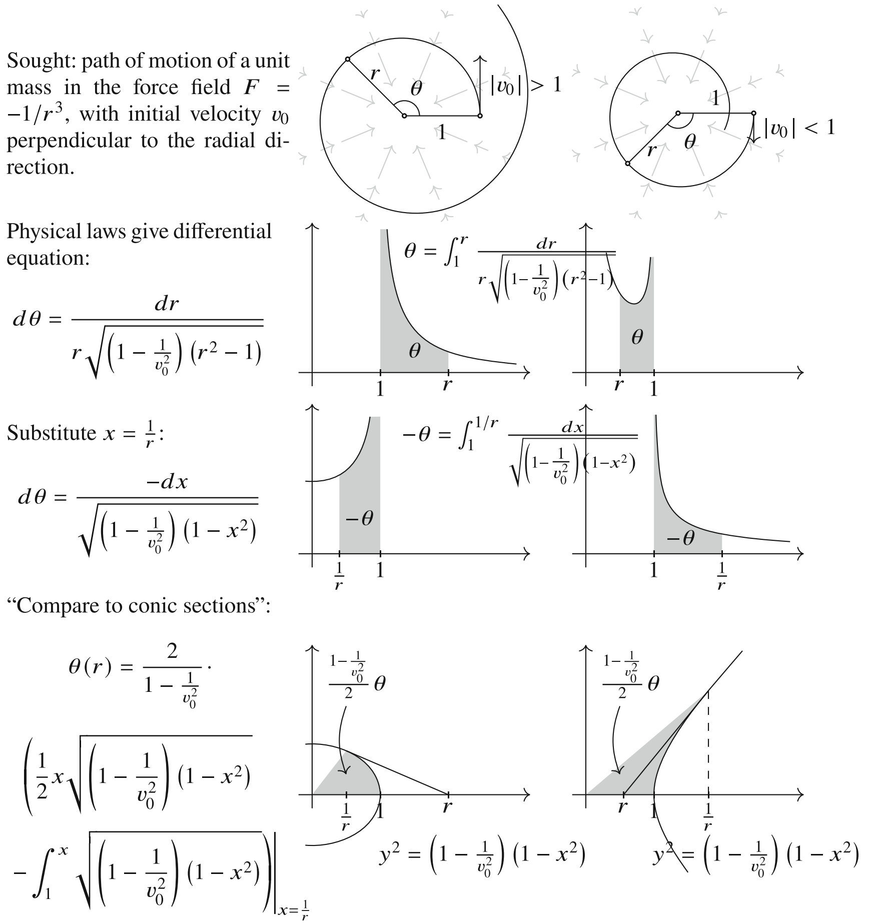

Fig. 4.3 Paraphrase of the steps Newton used to derive his description of trajectories in an inverse-cube force field

such as Guicciardini (2016), is instead turning its attention more to textual aspects, such as the context of this episode in Newton's manuscripts and correspondence.

But mathematically oriented historical scholarship has not stagnated because it has exhausted its potential or become obsolete. It is spinning its wheels at the moment, but by reorienting it in a new direction we will be able to harness its power in new ways.

The mathematical commentaries on Newton's orbit derivation are "done" only because they set themselves too limited a task. Very typical is the conclusion by Erlichson (1994) that "the ultimate key to the mystery ...is Newton's practice of expressing abstract quadratures by concrete visualizations" (154). Note the word *ultimate*, as if there was nothing further to be explained! From a cognitive point of view, the postulation of a particular type of geometrical predilection in Newton's style is not an "ultimate" brute fact but merely the beginning of what is to be explained. *Why* was this Newton's practice?

Cognitive questions pick up precisely where purely formal analyses left off. Erlichson stopped when he reduced the matter to a particular disposition in Newton's style, because that's where objective mathematics ends and subjective preferences begin. We need to break down this common barrier that isolates technical mathematical analyses from the less tangible but equally crucial cognitive considerations that drive mathematical research. Technical details of mathematical arguments on the one hand, and broader stylistic and philosophical attitudes on the other, co-evolved and were intimately intertwined.

Today there is agreement on what "the answer" is to integrals such as those involved in the Newton example above (in that case the solutions can be expressed in terms of trigonometric and exponential functions). But, as that example shows, the situation was much more fluid at the time. It is indeed a non-trivial question (that the early practitioners of the calculus wrestled with extensively) to decide even what kind of thing "the" answer should be. What do we want the solution to a differential equation to do? Should it be numerically tractable, visualisable, or qualitatively illuminating? By what such standards is, for example, Newton's reduction to convoluted conic measurements superior to other possibilities that were readily available to him, such as the quadratures of higher-degree curves obtained as intermediate steps in his own derivation, or power series methods? Indeed it is striking that in his final step Newton is able to express everything in terms of conics only by making the shapes of the area segments more complicated and even by completely dropping the geometrical representation of the proportionality constant from the visualisation altogether. So Newton opted for this particular type of geometrical interpretation even though it came at a notable cost.

Newton's choice was hardly mere conservatism, because comparable preferences for qualitative, geometrical characterisations of integrals are commonplace in the early calculus. This includes for instance the prominent seventeenth-century practice of "rectifying quadratures" (Blåsjö 2012), that is to say, expressing insoluble integrals as arc lengths, such as elliptic integrals in terms of the arc length of the lemniscate. Equally odd to modern eyes is the recurrent theme in this period of finding when a generally transcendental problem could be expressed without reference to transcendental quantities (such as trigonometric functions or π). For instance, Huygens, Leibniz, and Johann and Jakob Bernoulli all tried to find classes of segments of the cycloid whose area is "squarable" in this sense. The Bernoullis investigated this in depth and to this end were led to developing what is today known as Chebyshev polynomials (Henry and Wanner 2017). To name another example, Huygens's solution to the catenary problem fell short precisely because it failed to fully reduce the necessary quadratures (Bos 1980, 142), showing that this was a complicated matter that stumped even the best minds. Similarly, Leibniz solved the brachistochrone problem but failed to recognise from his own solution formula that it was the well-known cycloid (Blåsjö 2017b, 185).

Altogether, the varied ways in which seventeenth-century mathematicians chose to transform and "solve" integrals were based on deliberate choices and priorities that were mathematically and philosophically rich but are poorly understood today. The only way to illuminate such questions is through a comparative perspective. If we look at episodes like the Newton orbit example in isolation then we have little choice but to leave it at the weak non-explanation that Newton preferred to express the solution geometrically rather than by a formula. But by taking a comprehensive view and reconstructing the overall state of calculus research at the time, we will be in a much better position to situate his precise choices in a coherent context.

For example, Guicciardini (2016) has shown what calculus manuscripts Newton relied on in his solution, so by studying the guiding motivation implicit in the structure of that treatise, and the uses Newton made of those ideas in other works, we will be able to say much more about what guided Newton's choice of representation in the orbit example than we could have by looking at that episode alone.

In the same way, comparing Newton's approach with those of his contemporaries will also illuminate what he saw as the particular strengths of his chosen integration methods and means of representing curves. Guicciardini (2016) is right that "interesting questions remain open" such as "in what sense do Newton's methods differ from those deployed by Leibniz, Varignon, Johann Bernoulli, and Euler?" (234) It is no coincidence that these questions remain open, since the photorealism axiom penalises comparative research. A historiographical rethink is needed to make progress in these directions. A cognitive turn will redress precisely this problem and thereby revitalise the field and show how the expertise of mathematically trained researchers can be mobilised in new ways to reach new kinds of insights about history.

4.5 Conclusion

Let me summarise the invitation to mathematicians that I have proposed. In a historical text we find a mathematician using a particular technique. For example, Huygens finding the motion of a particle sliding down a cycloid by relating it to the geometry of an associated circle. Or Newton finding the orbit in an inverse cube force field by relating it to arcs and areas of conics.

We want to know what the broader cognitive significance of this technique is. Typically, isolated cases are insufficient to say anything conclusive about this. So we form several interpretative hypotheses consistent with the case at hand.

For example, we may hypothesise that Huygens's use of circle geometry to solve a dynamical problem is effectively equivalent to using the calculus of trigonometric functions. Or in the Newton case we may hypothesise that Newton preferred his convoluted expression in terms of conics, rather than the obvious alternatives such as power series, because it better illuminates the qualitative properties of the orbit.

Such hypotheses entail testable predictions. If the hypothesis correctly puts the finger on a key aspects of the mathematical thought of that author, then in comparable cases that author ought to act in accordance with that hypothesis. So to test our hypotheses we then turn to other works.

For instance, among the various problems that Huygens solved geometrically that we today would solve using the calculus of trigonometric functions, which utilise a reference circle and its properties such as theorems about tangents to effectively go from the same premisses to the same conclusion as the modern calculus proof? Are there cases where Huygens's approach has demonstrable drawbacks compared to an approach based on the calculus of trigonometric functions? Are there cases where Huygens failed to solve a problem that the next generation could solve by the calculus or trigonometric functions?

In the Newton case, his reduction to conic areas and arcs is based on an unpublished catalogue of such reductions—effectively a "table of integrals." What other uses did Newton make of this catalogue? Are those uses consistent with our hypothesis? Is the theoretical structure of the catalogue consistent with our hypothesis?

These kinds of questions can only be answered by a mathematical analysis that goes beyond what is explicit in the texts, and by a comparative perspective that looks at the mathematical practice of the time comprehensively. Answering such questions requires understanding that only mathematicians are likely to posses.

It is easier than ever for mathematicians to enter the field and do this kind of work. Recently published sources and specialised studies have made the field more accessible and easier to navigate, and it has made comparative and interpretative work drastically more feasible. The old hostility to mathematicians among professional historians of mathematics that had its brief heyday is nowadays sooner the subject of historical study itself (Schneider 2016) than a force in the present that anyone needs to fear. Mathematicians turning to history are likely to find a warmer reception today than in those "cold war" decades.

Thus mathematicians can help historians, but there will be benefits in the opposite direction as well. A cognitively reorientation of historical scholarship will make it more mathematically exciting. Our questions about the thought and practice of Huygens and Newton, for example, are precisely the kind of history that is directly relevant to teaching and thoughtful understanding, not as decorative anecdotes but as insights deeply intertwined with content and substance.

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