Isaac Newton's Natural Philosophy edited by J. Z. Buchwald and I. B. Cohen

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This book is a collection of papers originally presented at a series of meetings at the Dibner Institute for the History of Science and Technology, Cambridge, MA. The volume is in two parts. In the first, we find four essays devoted to the 'motivations and methods' of Newton's research by M. Mamiani, I. B. Cohen, A. E. Shapiro, and M. Feingold. In the second, we find five essays devoted to questions concerning celestial dynamics and rational mechanics by J. B. Brackenridge, C. Wilson, M. Nauenberg, M. Blay, and G. Smith. An appendix contains a paper by Newton's well-known biographer, Richard S. Westfall, prefaced by an appreciation honoring the late author by I. B. Cohen. The specific subjects of the essays are as wide-ranging as they are varied in argumentative style and methodology. I will not review the essays by summarizing them one by one. Some of their technical content might intimidate the reader unfamiliar with this type of historical research. So I will discuss them according to what I believe are the fundamental strengths (and a few weaknesses) of this collection, trying to keep technicalities to a minimum. My choice should by no means be taken as an implicitly judgmental approach to the book. The authors of the essays will, I hope, excuse the limited competence of the reviewer. I have grouped my comments under two broad headings, 'Methods' and 'Results'.

Methods

I sometimes found myself baffled while reading this book, strangely not because of the arduous mathematical notation which is frequently

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employed by the authors, but more mundanely because of their terminological choices. Let me exemplify straightaway. Mamiani opens his essay by claiming that the theme of his investigation is a 'principle' according to which 'a dynamic point of view' should guide 'analyses of the development of scientific ideas' [3]. Thus, in Mamiani's view, a consequence of this principle is that 'science proves, on close examination, to consist, to some degree, of radical "transformations" of existing ideas, concepts, and methods'. A few lines below, we learn that we need to look for the 'growth of the scientific concepts'. Eventually, the author explains that his goal is to 'focus attention on a particular transformation that marked the migration of categories and methods from one discipline to another'. Principles, ideas, concepts, methods, development, migration of categories... I really wonder. What is the theme of this essay? Mamiani wishes to argue that the celebrated rules for philosophizing (*regulae philosophandi*) in book 3 of the later editions of Newton's *Principia* are simply a transformed version of a set of rules developed by Newton in the *Treatise on the Apocalypse*. The latter set of rules has a 'source', according to Mamiani, a treatise on logic and rhetoric by Robert Sanderson, his *Logicae artis compendium* published at Oxford in 1618. What does Mamiani mean by 'source'? No explanation is given. However, in a further, even more confusing re-statement of the essay's goals, Mamiani claims that he will show that the rules for interpreting the *Apocalypse* were in turn (mostly) a transformation of Sanderson's rules. What about the original theme of Mamiani's essay, a 'principle' according to which 'a dynamic point of view' should guide 'analyses of the development of scientific ideas'? I am lost. Maybe the author too got lost in his terminological maze.

At any rate, here is an instance of Mamiani's conclusions. We find in Sanderson's book the following 'law of brevity': 'Nothing should be left out or be superfluous in a discipline' [11]. This was transformed by Newton in the *Treatise* into the following two rules: 'To assign but one meaning to one place of scripture', and 'To keep as close as may be to the same sense of words' [11]. This couple of rules eventually became *Rule I* in the 1687 edition of the *Principia*, namely, '*Causas rerum naturalium non plures admitti debere, quam quae et verae sint & earum phaenomenis explicandis sufficiant*' [11]. No translation is furnished by Mamiani, but by way of helping the reader I will give mine: 'No more causes of natural things should be admitted than those which are true, and which are sufficient to explain the phenomena of those things'. Having first thought up the law of brevity I wonder why Sanderson did not proceed to write up the *Principia*. Mamiani comments: 'Thus, the transformation of concepts is the key to understanding the innovative procedures of the *new science*' [12]. Are rules concepts? Maybe they are in Mamiani's mind. Further, what are the 'innovative procedures' referred to here? Another little linguistic puzzle, it seems to me.

I shall give a second example of how terminological and methodological issues impinge on the questions raised by this collection by looking at two essays, Nauenberg's and Wilson's, since both investigate Newton's researches on lunar motion but from quite opposite methodological standpoints. I will try to explain why Nauenberg's historiographic approach obscures instead of illuminating Newton's physico-mathematical procedures, while the historical sensitivity of Wilson's splendid essay furthers our understanding of them.

Nauenberg wishes to show that by 1686 Newton had developed a perturbation method to deal with Keplerian motions in general, and that such method '*corresponds* to the variation of orbital parameters method first developed in 1753 by Euler and afterwards by Lagrange and Laplace' [189] (emphasis added). The evidence for Nauenberg's claim lies in a fascinating text by Newton, only published in the 20th century [see Whiteside 1967–1981, 508–537]. First and foremost, we may ask, what does Nauenberg mean by 'correspond'? No clue is to be found in his essay. Since the mathematics in Nauenberg's essay is complex, I will not go into the details of his argument here. However, I should like to suggest an example of what 'correspond' might in fact mean in a context with which the reader may be more familiar and which has the added bonus of being mathematically much simpler.

In modern textbooks, you may have come across Galileo's time-squared law of free-falling bodies expressed as a simple proportionality, in the following notation for example:

$$s \propto t^2$$

where

$$\begin{aligned} s &= \text{space} \\ t &= \text{time} \\ \propto &= \text{`proportional to'.} \end{aligned}$$

Sometimes you may also have found an algebraic equation expressing Galileo's time-squared law such as

$$s = kt^2,$$

where k is a constant. Galileo did not use any form of symbolic or algebraic notation, though. Algebra was totally alien to him. He wrote the proportionality of space and the square of time in plain natural language, in the mathematical style of Euclid. He would not have used an algebraic formula (let alone admit a ratio between two non-homogeneous quantities such as space and time). Yet I suspect that in Nauenberg's view the formula above, or the equation, would correspond to Galileo's result rather unproblematically. But this is simply not the case. The thought processes required to arrive at and understand equations are largely different from those underlying Galileo's mathematical natural language. As long as you are interested in Galileo's thought processes, you would do well not to succumb to the lure of superficial correspondences.

By the same token there is not much notation in Newton's writings that is relevant to our subject. In the manuscript on lunar motion, which is in Latin, Newton mostly makes use of natural language in order to express proportionalities; and at times he has recourse to a very simple algebraic notation in which ratios are written down as fractions, exactly as he does in the *Principia*. In addition, his reasoning depends on powerful visual representations based on geometric diagrams—so much so that a modern reader accustomed to our textbooks in mechanics, cast in the language of college calculus, might be struck dumb by the *Principia*, precisely because it is a work of geometry wholly in the style of Euclid's *Elements*. On the other hand, I have counted 107 formulas involving Leibnizian and functional notation in Nauenberg's essay! All of this symbolism would have been totally alien to Newton, precisely as the above formula for the time-squared law would have been alien to Galileo. Briefly, then, what Nauenberg does is this. He re-writes or (as we might say in order to do justice to the author, since there is an element of creativity here) divines Newton's procedures in the Leibnizian language of the calculus or, to be sure, in one of its many modern guises; and then he claims that the same procedures were 're-discovered' later by the continental mathematicians who had adopted and developed the Leibnizian calculus. Thus, he argues that Newton's method for studying lunar motion *corresponds* to the variation of orbital parameters method first developed in 1753 by L. Euler and afterwards by Lagrange and Laplace. He seems to be motivated, I think, by the illusion that all of Newton's procedures are mechanically 're-writable' in a homogeneous mathematical style.

Recent Newton scholarship, however, has argued convincingly that most of Newton's fundamental results were not reached by means of a secret analysis and then subsequently dressed up in a geometrical style, such as that found in the *Principia*. Newton's reasoning processes were originally quite different [see, e.g., De Gandt 1995]. To represent them in a Leibnizian symbolism is arbitrary and unwarranted. Instead of deepening our understanding of the objects of historical research, such representation obliterates its very substance. Further, it has also been forcefully suggested that the development by which the continental mathematicians of the 18th century gradually transformed the *Principia* into the new language of the Leibnizian calculus was neither a 're-writing' of results, nor a rediscovery of methods that Newton had guarded from public scrutiny. On the contrary, that process was a formidable intellectual enterprise which mobilized the most creative mathematical minds of the 18th century [cf. Guicciardini 1999 and Blay 2002].

Let us now turn to Wilson's essay. One key element shapes Wilson's argumentative strategy. He wishes to compare the method by which both Newton and the later continental mathematicians tackled the problem of the Moon's apsidal motion (on which more in a moment). However, Wilson resists the temptation to read backwards into Newton's approach the language of Leibniz.[1] Imagine the orbit of the Moon around the Earth. It is an ellipse, though one that is very nearly circular. But for the sake of visualization now imagine the orbit as markedly elliptical, like that of a returning comet, for

[1] To be sure, he uses a form of Leibnizian calculus to voice, so to speak, some of the assumptions that he believes guided Newton's analysis; but he does not attribute the formulas themselves to Newton, nor, crucially, does he draw conclusions on the basis of the magical art of divining the existence of Leibnizian formulas inside the Newtonian mind. On page 167, for instance, Wilson explicitly shows a genuine Newtonian formula together with the modern notational equivalent with which he works. He is very careful to distinguish the two, though.

example. Now, the apsidal motion is the slow motion by which the ellipse itself rotates around the central body. It is called 'apsidal' because astronomers call 'apses' the points furthest and nearest to the body orbited by another body, in this case the intersections of the orbit and the major axis of the ellipse. Newton failed to solve the problem of the motion of the Moon's apses. In Wilson's words, Newton's 'brave conclusion' is worthless because of a fatally flawed assumption [168], the technical details of which are irrelevant here. Why did the great Newton make such an error? Was it because he did not have at his disposal the powerful notational system of the Leibnizians? The answer is complex. True, he did not have the calculus in the form of Leibniz' symbolism. But, in Wilson's view, what appears to be the ultimate constraint on his reasoning strategies is that Newton visualized the apsidal motion as the motion of a rotating ellipse. That was the real hindrance in his understanding of the phenomenon. And this is the high point, historically most revealing, in Wilson's essay. Newton's thought processes do not proceed from formulas to their physico-geometrical meaning. It is meaning in the form of the visual representation of phenomena that guides his mathematical procedures.

The problem of apsidal motion was solved later on in continental Europe by Clairaut, L. Euler, and d'Alembert. When Clairaut first realized that the visual representation of the rotating ellipse was misleading, he was relieved. For, previously, he had had to come to terms with the only hypothesis that could save the appearance of the motion of the Moon, the abandonment of the very law of universal gravitation (in the form of the inverse square of the distance).

We may now ask: What made the achievement of the continental mathematicians possible? We may begin to shape an answer as follows. The continental mathematicians had long abandoned the geometric style of the *Principia*. They put absolute faith in, and staked their reputations on, the power of Leibnizian algorithms, even when the meaning, in terms of visual representations, of the mathematics they were developing escaped them. Wilson's essay shows a facet of this achievement with plenty of historical insight.

Results

Alan Shapiro's essay is concerned with Newton's work on diffraction and the reasons that delayed the publication of the *Opticks* [1704]. Its principal strength lies in its being based on first hand knowledge of the relevant manuscripts and worksheets. It is often assumed that what kept Newton from publishing the *Opticks* was his rivalry with Hooke; and that when the latter died, Newton felt that the right moment to publish his researches on optics had come. Shapiro, however, tells a different and more intriguing story. The fact is that Newton had developed a model of diffraction based on a hypothesis that later on proved untenable. Diffraction is the phenomenon that causes beams of light to bend when passing close by an object's edges. It is revealed by patterns of light and darkness in the image of the object projected onto a screen. Newton eventually abandoned the early model after he had satisfied himself that experimental data could not possibly fit the model's predicted patterns. Whatever the reasons may be that really determined Newton's delay in publishing the *Opticks*, an issue concerning which Shapiro offers a balanced discussion, Shapiro's essay shows the riches still awaiting Newton scholars in the form of manuscript materials (unfortunately) spread in libraries all over the world.

Michel Blay shows another way in which manuscript resources may illuminate this kind of historiography. He has delved into the records preserved in Paris of sessions of the Royal Academy of Science in order to illustrate the genesis of new concepts, such as that of instantaneous speed in the work of Pierre Varignon. By comparing Varignon's algorithmic treatment of motion problems with Newton's, Blay casts light on the profound transformation that led the continental mathematicians to shape a Leibnizian version of rational mechanics. Research on manuscript material is powerfully revealing, and there are serious limitations to what historians can achieve by simply considering published material. Bruce Brakenridge's essay is devoted to the concept of curvature in Newton's dynamics. Brakenridge gives us an account whose intricacies could never have been disentangled but for the wealth of manuscript material published by Whiteside [106]. Curvature is the amount of 'crookedness' of a curve at any single point. It was this concept that was central, at various stages, to Newton's investigations of the nature of the forces acting on bodies moving along curved paths.

Let us go back to the 'public' *Principia*. I am very sympathetic to Smith's essay. Smith is first of all an engineer, as I was some time ago. So I read with great pleasure his essay on book 2 of the *Principia*, on motion in fluids, a part of the *Principia* for which the scholarly literature is scant if there is any at all to be found. Smith believes that what he calls 'Newton's style' in book 2 is no different from the style of the rest of the *Principia*. The Newtonian style, in Smith's view, is a global approach to natural philosophical inquiry, a 'sequence of idealizations, each of which is used to draw conclusions from phenomena, and which together comprise successful approximations in which residual discrepancies between theory and observation at each stage provide an evidential basis for the next stage' [251]. Regrettably, the technical aspects of book 2 prevent me from discussing the details of Smith's nicely articulated argument, once again; but I found his analysis of what we might call Newton's 'construction of the idealization of fluid resistance' utterly convincing. Fluid resistance is tricky. It depends on so many factors that experimentation with bodies moving in real, viscous fluids may easily become baffling. Newton came up with pendula, for example, as a means to getting a handle on the phenomena of motion in fluids. But ingenious as this was, the data yielded by pendular oscillations remained confusing even for him. All in all, according to Smith, fluid resistance resisted Newton's empirical attempts to decipher its intricacies.

I should also mention Feingold's paper on the relationship between Newton and the Royal Society. More specifically, the question posed by the author [78] is: 'What were the consequences for the fortunes for the Society of Newton's uncompromising conviction concerning the primacy of mathematics in the domain of natural philosophy. . . ?' I confess that I do not incline much to sociological analyses: interesting as the story recounted by Feingold is *per se*, how it illuminates the subject of the book escapes me. In addition, valuable information is to be found in the essays by Cohen on the influence that Huygens' *Traité de la lumière* exerted on Newton's decision not to have his name printed on the frontispiece of the *Opticks*, and by Westfall on the technological developments that made possible the mathematization of nature in early modern Europe.

In conclusion, we owe a profound debt of gratitude to the editors for assembling such a valuable collection of essays. Anybody who is seriously interested in Newton's achievement should read this book and plunge into the wealth of fascinating arguments that I have only begun to outline in this review.

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