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Introduction

Conceptualism and contextualism in the recent historiography of Newton's *Principia*

Abstract

Recently the *Principia* has been the object of renewed interest among mathematicians and physicists. This technical interpretative work has remained somewhat detached from the busy and fruitful Newtonian industry run by historians of science. In this paper will advocate an approach to the study of the mathematical methods of Newton's *Principia* in which both conceptual and contextual aspects are taken into consideration.

Sommario

In questi ultimi anni i *Principia* sono stati oggetto di un rinnovato interesse da parte di matematici e fisici. Questo lavoro di interpretazione tecnica è rimasto alquanto isolato rispetto all'attiva e produttiva industria newtoniana dominata dagli storici della scienza. In questo articolo difenderò un approccio allo studio dei metodi matematici dei *Principia* di Newton nel quale vengono presi in considerazione tanto gli aspetti concettuali quanto quelli legati al contesto.

MSC: 01A45

Keywords: Newton; Historiography of mathematics

1. An historiographic problem

1.1. *Recent debates on Newton's* Principia

Since its publication in 1687 Newton's *Principia* has given rise to much debate. In particular, during the first decades of the 18th century the mathematical methods employed by Newton were criticized or defended by the small number of mathematicians who could read the *magnum opus* with sufficient competence [Guicciardini, 1999]. Under its classic façade the *Principia* hides a panoply of mathematical methods: series, infinitesimals, quadratures, geometric limit procedures, classical theories of conic sections and higher curves, projective geometry, interpolation techniques, and much more. How should the science of motion be mathematized? During Newton's lifetime this question was still unanswered. It was only in the 1730s, mainly thanks to the work of Euler, that the mathematical

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community became convinced, at first on the Continent, that the calculus, most notably differential equations and the calculus of variations, was the appropriate language for "dynamics" [Blay, 1992]. Nowadays, a student accustomed with "Newtonian mechanics" will find the language used in the post-Eulerian era somewhat familiar. In contrast, the language of the *Principia*, burdened by geometrical diagrams, the theory of proportions, almost devoid of symbolical expressions, leaves our student, even a tenacious one, perplexed.

Recently the *Principia* has been the object of renewed interest among mathematicians and physicists.[1] A reappraisal of the fertility of geometrical methods in dynamics, the plethora of celebrations for the tercentenary of the publication of Newton's masterpiece, and the conviction that technical history of science can be used for didactic purposes [Densmore, 1995] are some of the factors that have induced some working scientists to open and comment on the *Principia*. The results have been often profound and have added new insight to the researches of such founding fathers of modern Newtonian scholarship as I. Bernard Cohen, Rupert Hall, John Herivel, Alexandre Koyré, Richard Westfall, and Tom Whiteside. Scientists of the calibre of Vladimir I. Arnol'd, Subrahnanyan Chandrasekhar, and Richard Feynman have scrutinized some of Newton's demonstrations in depth [Arnol'd, 1990; Chandrasekhar, 1995; Feynman, 1996]. Some mathematicians and physicists, such as Michel Blay, Bruce Brackenridge, James Cushing, François De Gandt, Dana Densmore, Herman Erlichson, Michael Nauenberg, Tristan Needham, Bruce Pourciau, George E. Smith, Robert Weinstock, and Curtis A. Wilson have turned to technical history, revealing details of several propositions, lemmas, and corollaries of the *Principia*.[2] This is indeed a good moment for Newtonian scholarship, since working scientists have been able to considerably improve our understanding of Newton's mathematical methods. Thanks to this serious interpretative work, Newton's *Principia* is understood today much better than 20 years ago. Historians of science should be grateful for these contributions and avoid ignoring or rejecting them on the basis of academic disciplinary preconceptions.

An exciting feature of the research field explored in the above mentioned studies is that several interesting and fruitful questions have emerged. Did Newton reach his solutions by cogent arguments? Are there in the *Principia* existence and uniqueness demonstrations of such solutions? Did Newton possess a method which he applied in order to reach these solutions? What is the relation between this method and the differential and integral calculus? These questions have been debated by the mathematicians and the physicists who recently commented on the *Principia*.[3] The results of this debate are difficult to summarize. In brief, one can safely say that this debate has deepened our understanding of Newton's mathematization of motion.

On the other hand, it cannot be denied that this technical interpretative work has remained somewhat detached from the busy and fruitful Newtonian industry run by historians of science. Very rarely connections are drawn between the researches of historians of Newton's mathematics and those of historians of, say, Newton's religion or alchemy. Historians of Newton's mathematics work somewhat in isolation from historians of other areas of Newton's thought.

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[1] Two recent fine collections of essays are Dalitz and Nauenberg [2000] and Buchwald and Cohen [2001].

[2] The reader can turn to the References for some bibliographic information on the works of the above-mentioned scholars. It should be noted that this list is not meant to be exhaustive and omissions do not imply a negative judgement of the author. Indeed, the bibliography on Newton's *Principia* is vast.

[3] For instance, Weinstock [2000] gives an account of the debate concerning Newton's treatment of the so-called inverse problem of central forces, a debate which was initiated by Weinstock himself in Weinstock [1982].

1.2. Conceptualist and contextualist history

> It is safe to say that no proof given at least up to 1800 in any area of mathematics, except possibly in the theory of numbers, would be regarded as satisfactory by the standards of 1900. The standards of 1900 are not acceptable today. [Kline, 1974, 69]

> In most sciences one generation tears down what another has built, and what one has established another undoes. In mathematics alone each generation builds a new storey to the old structure. [Hankel, 1871/1889, 25, quoted in: Dauben, 1984, 81]

These quotations express two conflicting conceptions on the relationships between mathematical proofs and context. One might call the former a *contextualist* and the latter a *conceptualist* conception.

According to Kline the acceptability of proofs changes from one context (temporal and perhaps geographical) to another. A proof which would be acceptable, say, in 1650 France, would be rejected in 1850 Germany. Hankel, instead, expresses a widely shared value amongst mathematicians: mathematical proofs are for eternity, cultural changes do not interfere with what is achieved by mathematicians.

Historians of philosophy have often considered the contrast and balance between two approaches; one focussed on textual analysis, the other on the context. Albert William Levi in his *Philosophy as Social Expression* [Levi, 1974] defends what he calls a “contextualist” approach. Levi’s views have been adopted by James Force in his study of William Whiston, Newton’s successor as the Lucasian Chair [Force, 1985]. Force observes: “According to one school of theorist, the philosophically important aspect of a text is the text itself, which, it is maintained, is logically independent of, and intellectually autonomous from, any historical context. All that is relevant to the understanding of any philosophical text is timelessly in the text itself” [Force, 1985, 1]. Levi often refers to this school as semantic “atomism.” Contrasting to this assumption of a permanence of meaning that is outside of time, locked ahistorically in “atoms” of text, is an opposing theoretical school according to which terms and arguments in the history of philosophy must be interpreted within the special framework of concepts and distinctions specific to the thinker’s cultural context [Force, 1985, 1]. The philosophical question raised by the above two quotations is perhaps the fundamental one for a philosophy of mathematics. A satisfactory answer should incorporate both positions into a coherent image of mathematical development, an image that should avoid both extreme relativism and apriorism.

Historians of mathematics are not required to build up a philosophy of mathematics. However, philosophical views on the nature of mathematics orient historical research along different lines. A view that favors context-independence can lead—even though not necessarily—to cumulative, linear history. On the other hand, interest for context-relativity more easily leads to a history where ramifications, diachronic changes, and perhaps even revolutionary changes are possible [Gillies, 1992].

Philip Kitcher has done a great deal in order to defend the fruitfulness of contextualism in the historical study of mathematics. According to Kitcher, a conceptualist approach fails to explain the role of shared metamathematical views in shaping standards of proof, the scope of mathematics, the order of mathematical disciplines, the relative value of particular types of inquiry [Kitcher, 1983, 188–192]. We will briefly present below possible influences of metamathematical beliefs on Newton’s mathematical practice. Kitcher’s theses might be reinforced by what Brian Rotman says about the nature of proofs: “a leading principle is always present—acknowledged or not—and attempts to read proofs in the absence of their underlying narratives are unlikely to result in the experience of felt necessity, persuasion, and conviction that proofs are intended to produce, and without which they fail to *be* proofs” [Rotman, 2000, 8]. It can reasonably be contended that a shared view on the nature and role of proofs stands behind mathematical practice and that changes in such metamathematical views are intertwined with large-scale changes in other components specific to the mathematician's culture [Kitcher, 1983, 191]. This is what makes the contextualist approach fruitful to many historians: it reveals the relationships with culture, society, and technology, and affords a better understanding of the mathematician's motivations. Indeed, one of the strongest reasons in favor of a contextualist approach in the history of mathematics is that it widens the research scope opening relationships with general history of science, history of religion, history of art, history of technology, and so forth.

On the other hand, and not without reason, it has been often observed that, notwithstanding the relationships between mathematics and the context in which mathematics develops, mathematics is ultimately driven by inner, conceptual, motivations. As Brendan Larvor puts it:

> Mathematical development may be distorted by ideological interference, stymied by academic rivalries or halted by the fall of empires. Nevertheless, [...] the direction of mathematical development and the response of mathematics to external stimuli are both best explained by factors proper to mathematics itself. [2001, 215]

The great problem with such statements is to define the boundary which separates what belongs to "mathematics itself" and what is outside it. The definition of such a boundary is far from being trivial.

Yehuda Elkana has developed an interesting two-layered view of scientific knowledge, which can help in articulating the question of the boundary referred above [1981]. Elkana's views were subsequently applied to mathematics by Leo Corry. It is worth quoting Corry at length:

> Any scientific theory raises two sorts of questions: (1) substantive questions of the discipline, and (2) questions about the discipline *qua* discipline, or meta-questions. One can accordingly distinguish two layers related to any scientific field: the "body" of knowledge (answers the first kind of questions) and "images" of knowledge (answers the second kind of questions). The body of knowledge includes theories, "facts", methods and open problems. The images of knowledge play the role of "selectors" for the body of knowledge, by answering meta-questions such as: which of the open problems of the discipline most urgently demands attention? How should one decide between competing theories? What is to be considered a relevant experiment? What procedures, individuals or institutions have authority to adjudicate disagreements within the discipline? etc. It is clear that answers to this kind of question are dictated not only by the substantive content of the body of knowledge alone, but also by additional, external factors. [...] Science as a system of knowledge is composed of two layers, body and images of science, which organically interact and do not have separate existence. The separation mentioned here is an analytical one, which the historian of science may identify in hindsight. [1993, 106]

Following Kitcher's insistence on the role of metamathematical assumptions and Corry's recognition of an organic interaction between the body and the images of mathematics, I would like to advocate an approach to the study of the mathematical methods of Newton's *Principia* in which both conceptual and contextual aspects are taken into consideration. This is especially important in the case of Newton, a thinker who did not draw a clear-cut distinction between natural philosophy, theology, mathematics, and alchemy. This is not to deny that the different sectors of Newton's intellectual activity possessed a relative autonomy. But it would be narrow minded to interpret this autonomy as absolute independence. I believe, and I hope to tentatively argue here, that an approach which favors the study of meaningful resonances between these sectors (mathematics included) is going to be a fruitful one.[4]

I can see two research areas which promise interesting results for a contextualist reading of the mathematical methods employed by Newton in the *Principia*. First, one might inquire whether the myth of the ancients' wisdom and the polemic against the "men of recent times" endorsed by Newton in his studies on chronology, religion, and alchemy was instrumental in shaping (a) the mathematical language

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[4] The complex relationship between the various areas of Newton's legacy is discussed in Iliffe [1998].

in which the *Principia* is written and (b) the ways adopted by Newton in communicating advanced and hidden layers of the mathematical structure of the *Principia* only to faithful acolytes. Second, one might focus on Newton's ideas on the relationships between mathematics and nature. I will maintain below that these philosophical ideas led Newton to affirm that geometry was subsumed under mechanics. In this context I will deal with the connection which Newton established between the generation of magnitude exerted by God and Nature and the voluntary mechanical constructions of the geometer. I have written elsewhere on the first theme [Guicciardini, 1999, 2003]. I will briefly recapitulate my theses in Section 2.1.

2. Newton's philosophy of mathematics

2.1. Classicism and publication practice

Both in his researches on religion and in natural philosophy Newton was accustomed to draw a neat divide between what could be published without restrictions and what had to be communicated in a more controlled way. For instance, while he printed his "Theory about Light and Colors" in the *Philosophical Transactions*, he preferred to deposit the "Hypothesis Explaining the Properties of Light" as a manuscript in the archives of the Royal Society [Newton, 1978, 47–59, 177–235]. He prepared some of his works on chronology and biblical exegesis for the press but communicated his more heretical ideas only to a carefully selected group [Mandelbrote, 1993]. He corresponded on alchemy with a few adepts, but did not give open publicity to his alchemical experiments [Golinski, 1988, 147–167]. There are many different motivations behind these choices. Newton's conviction that hypothetical results should be presented in a provisional form was a driving force behind the publication of his optical, and most probably his alchemical researches. In the case of theology there were much more pressing political constraints which favored prudence.

Also in the case of mathematics Newton followed a publication policy which presents certain analogies with the dualism between public and private encountered in other areas such as theology, alchemy, and optics. One often reads that Newton after the discovery of the calculus did not publish it. This reluctance to publish has often puzzled historians of mathematics. Historians cannot avoid a feeling of disconcert when they realise that most of Newton's mathematical discoveries in the late 1660s and early 1670s were printed decades later, basically after the inception of the priority dispute with Leibniz in 1699. These discoveries, especially those concerning infinite series and the calculus, were so innovative that early 18th-century European mathematics would have been different had Newton been prompter in sending some of his early manuscripts on the method of series and fluxions for publication. Just to take an example, which easily comes to mind, the priority dispute with Leibniz would have been so avoided [Gjersten, 1986, 511–514].

Several explanations of Newton's secretive attitude have been given. Some historians refer to the cost of book printing after the Great Fire [Newton, 1967–1981, III, 5–6]. Some describe Newton as an odd, sometimes even neurotic character, who isolated himself in an ivory tower.[5] Some describe the aftermath of the dispute on optics as a cause for Newton's reluctance to publish his calculus [Westfall, 1980, 252].

[5] A valuable description of Newton's aversity to publication can be found in Christianson [1984, 137–139, 141–143, 182–183].

Some think that in the 1670s Newton's interest shifted from mathematics to other subjects (primarily alchemy, theology, and history): he would have simply lost the motivation to rework his mathematical manuscripts for the press [Mamiani, 1998]. There is a grain of truth in each one of these explanations.

Newton had, however, alternative means to printing to let the outside world know that he was a great mathematician and to acquaint the cognoscenti with the content of his mathematical manuscripts. In the period preceding the printing of the *Principia* most of Newton's mathematical discoveries were rendered available to the mathematical community through rather oblique ways. Newton engineered a complex publication strategy. He allowed some of his mathematical discoveries to be divulged through letters and manuscript circulation. Manuscripts were shown to a selected group of experts in the field (such as John Collins, John Craig, Edmond Halley, David Gregory, and Fatio de Duillier) who visited Newton in Cambridge, they were deposited at the Royal Society in London or as Lucasian Lectures in the University Library at Cambridge, and they were even copied (sometimes in mutilated form) [Guicciardini, 2003].

Newton adopted a publication strategy for his mathematical discoveries that can be best defined as "scribal publication." As Harold Love has shown, in Restoration England the practice of scribal publication was flourishing. Love has described the practice of publishing texts in handwritten copies within a culture which had developed sophisticated means of generating, transmitting, and even selling such copies. Love has masterfully studied the ways in which manuscripts of political, literary, and musical content circulated in Restoration England. The invention of printing did not of course obliterate the practice of manuscript circulation. However, after the invention of printing, scribal publication was pursued with specific purpose. As Love remarks:

> There is a significant difference between the kinds of community formed by the exchange of manuscripts and those formed around identification with a text. The most important is that the printed text, being available as an article of commerce, had no easy way of excluding readers. Interesting in the choice of scribal publication [...] was the idea that the power to be gained from the text was dependent upon possession of it being denied to others. [...] Print publication implied the opposite view of a community being formed by the public sharing of knowledge. [1993, 183–184]

There is evidence that Newton tried to keep control over the dissemination of his mathematical manuscripts within a selected group of mathematicians [Guicciardini, 2003]. The reasons which induced Newton to follow a scribal publication of his fluxional method are complex. Here we can briefly note that he found it convenient to avoid print publication of a method that appeared to him not well-founded from a logical point of view and distant from the rigor attained by the ancient geometrical synthesis.

As a matter of fact, from the early 1670s Newton began distancing himself from his early researches in the "new analysis of the moderns." In particular he began talking in very critical terms of Cartesian algebra, a major source of inspiration for his youthful researches. Colorful invectives against the symbolism of modern mathematics and full of appreciation of the Greek mathematical tradition are often to be encountered in Newton's mathematical manuscripts in the period preceding the *Principia*. For instance, in the late 1670s, commenting on Descartes's solution of Pappus' four-lines locus problem, he stated with vehemence:

> To be sure, their [the Ancients'] method is more elegant by far than the Cartesian one. For he [Descartes] achieved the result by an algebraic calculus which, when transposed into words (following the practice of the Ancients in their writings), would prove to be so tedious and entangled as to provoke nausea, nor might it be understood. But they accomplished it by certain simple propositions, judging that nothing written in a different style was worthy to be read, and in consequence concealing the analysis by which they found their constructions. [Newton, 1967–1981, 4, 277]

In the case of the *Principia* Newton affirmed to have made use of the modern analysis as a heuristic tool and to have retranslated a pristine analytical text into geometrical form in order to conform his work to the style of the ancient geometers. Speaking of himself in the third person he wrote:

> By the help of this new Analysis Mr Newton found out most of the Propositions in his Principia Philosophiae. But because the Ancients for making things certain admitted nothing into Geometry before it was demonstrated synthetically, he demonstrated the Propositions synthetically that the systeme of the heavens might be founded upon good Geometry. And this makes it now difficult for unskillful men to see the Analysis by which those Propositions were found out. [1967–1981, VIII, 598, 599]

This and similar reconstructions that Newton framed a posteriori and in the context of the polemic with Leibniz must be read with caution: it would be simplistic to accept them as faithful historical accounts. They reveal, however, something of Newton's methodological views. It was obviously important for Newton to distance himself from the analysis of the moderns in order to stress the contiguity of his mathematical methods with the "good geometry" of the ancients. Scribal publication was a means to establish his conviction of the inferiority of the mathematical methods of the moderns and the fact that he viewed himself as a heir of an ancient mathematical tradition.

A study of the circulation of the mathematical manuscripts within Newton's circle shows that he revealed the fluxional analysis which stays behind some of the *Principia*'s demonstrations only to a selected group of acolytes.[6] In doing so he conformed himself to a publication practice and a policy of school formation which he considered consonant to an ancient exemplar. According to Newton—who was following a myth originated by the publication in 1588 of Pappus's *Collectiones*—the ancient geometers rendered public in a geometric synthetic language what was found beforehand thanks to a hidden analytical heuristic. The heuristic method was revealed only to the disciples. As a theologian Newton thought himself to be the last of a remnant of interpreters who could decode the symbolic language of the Book of Scripture [Snobelen, 2001]. As a natural philosopher he thought himself to be a rediscoverer of an ancient wisdom revealed by God to the ancient Hebrews and transmitted to priest–mathematicians, such as Pythagoras [McGuire and Rattansi, 1966; Casini, 1984]. Also, as a mathematician, Newton thought himself to be the heir of an ancient tradition in pure geometry. He deliberately distanced himself from the moderns—especially from Descartes—who were instead "uniting the arithmetic of variables [Arithmetica speciosa] with geometry" [Newton, 1967–1981, IV, 421]. It was only to his disciples, who came to visit him in his rooms in Trinity, that he revealed the hidden fluxional analysis. This was the motivation behind a publication practice that has often puzzled historians of mathematics. And this is the reason that certain parts of the *Principia*—especially those concerning the Moon's motion—are so difficult to read. The fact is that they are incomplete: in order to understand them one needs to know the fluxional analysis, which is kept hidden in the printed text [Guicciardini, 1999].

2.2. A mechanically based geometry

But Newton's preference for geometry had deep philosophical roots which went beyond his desire to conform himself to a mathematical language and publication practice consonant with the venerated methods of the ancients.

[6] A particularly interesting case is D. Gregory, whose memoranda reveal how the information flowed from Newton to his disciples. Some of the memoranda have been published in Hiscock [1937].

Recent studies devoted to the history of English mathematics have related the mathematical work of Hobbes, Barrow, and Newton to the empiricist tradition of the English scientific revolution. Pycior has underlined how the preference for geometry manifested by many British mathematicians was a result of a quest for an empirically based mathematics [1997]. Sageng in his study of Maclaurin has defined the British fluxional school as dominated by mathematical empiricism [1989]. Sepkoski in his recent Ph.D. thesis has taken a different view. He has studied a nominalist and constructivist tradition in mathematics that spans from Gassendi to Berkeley via Barrow and Hobbes. According to Sepkoski, the philosophy of mathematics endorsed by Newton falls in part in this tradition and can be termed "physicalist" insofar as it implies a "belief that mathematical representations should be closely aligned with the properties of physical bodies and their motions" [Sepkoski, 2002, 251].

Indeed Newton began the *De quadratura curvarum*, a mature treatise on integration that he composed in the 1690s and published in 1704 as an appendix to the *Opticks*, with the following words:

> Mathematical quantities I here consider not as consisting of least possible parts, but as described by a continuous motion. [...] These geneses take place in the reality of physical nature and are daily witnessed in the motion of bodies. And in much this manner the ancients, by "drawing" mobile straight lines into the length of stationary ones, taught the genesis of rectangles. [Newton, 1967–1981, VIII, 122, 123]

Notice that in these oft-quoted opening lines Newton introduces, beside the reference to the Ancients, another theme that was to become a leitmotiv during the priority controversy with Leibniz: the ontological content of the method of fluxions. In this context Newton maintained that fluent and fluxions are really exhibited *in rerum natura*, while Leibnizian infinitesimals do not exist. This is not to say, as Sepkoski convincingly argues, that for Newton the geometrical representations themselves are the ontologically real entities they describe, but rather that their manner of description is closely related to the real world that we perceive [2002, 250 ff]. Mathematical geometrical magnitudes are constructed by human faculties, but they are constructed in a way that is not arbitrarily detached from empirical experience. Newton often insisted on the fact that the magnitudes of the fluxional methods are accessible to perceptual experience. The fluxional method employs, according to Newton, only finite magnitudes that can be perceived. In the *De quadratura* he wrote: "For fluxions are finite quantities and real, and consequently ought to have their own symbols; and each time it can conveniently so be done, it is preferable to express them by finite lines visible to the eye rather than by infinitely small ones" [1967–1981, VIII, 113–115]. The message delivered in the *De quadratura* is clear: fluxional geometry is compatible with ancient geometry *and* is ontologically grounded.

As I have hinted above, these themes emerged in the heated context of the priority dispute with Leibniz. In the 1710s, when opposing Leibniz, Newton contrasted the safe referential content of his method with the lack of meaning of the differential calculus. In Leibniz's calculus, according to Newton, "indivisibles" occur, but the use of such quantities not only is a departure from ancient tradition, but also leads to the use of symbols devoid of referential content. In the anonymous "Account to the Commercium epistolicum" Newton wrote:

> We have no ideas of infinitely little quantities & therefore Mr Newton introduced fluxions into his method that it might proceed by finite quantities as much as possible. It is more natural & geometrical because founded on primae quantitatum nascentium rationes wch have a being in Geometry, whilst indivisibles upon which the Differential method is founded have no being either in Geometry or in Nature. [...] Nature generates quantities by continual flux or increase, & the ancient Geometers admitted such a generation of areas & solids [...]. But the summing up of indivisibles to compose an area or solid was never yet admitted into Geometry. [1967–1981, VIII, 597, 598]

Nature and geometry must then be conceived as deeply intertwined. It is this interrelation between a mechanically based geometry and natural motion that allows Newton to defend the superiority of his method compared to Leibniz's. His method—he claimed—contrary to the Leibnizian one shows continuity with ancient tradition as well as ontological content.

The importance of adopting a mathematical method endowed with referential content was particularly relevant for Newton's science of motion. In the 17th century the idea that the language for natural philosophy had to be geometrical was deeply rooted. Since Galileo's times the Book of Nature had been thought to be written in "circles and triangles, and other geometrical figures"; it was not conceived as written in algebraic symbols. In writing about motion, velocity, and trajectories in terms of geometry, Newton was inscribing himself in a school of natural philosophy that reckoned Galilei and Huygens as its principal exponents. If we turn our attention to astronomers, an important (perhaps the most important) category amongst the readers of the *Principia*, we find again that the language of geometry dominated their works. The works of Thomas Streete and Vincent Wing, which Newton might have had as sources, have the geometrical representation of trajectories as their object of study.

In the 17th century the language of geometry proved to be extremely useful in the study of kinematics. In fact, geometry allowed the modeling of the basic kinematic magnitudes. Displacements and velocities could be represented by geometrical continuous quantities, and thus kinematics could be studied in terms of the theory of proportions. But Newton's research program was wider: he had to mathematize force, not only kinematical magnitudes. One of his aims in Section 2 of Book 1 of the *Principia* was to find a geometrical representation of central force. He employed the proportionality of force to displacement from inertial motion acquired in an infinitesimal interval of time as well as the relationship between the normal component of force and the trajectory's curvature [Brackenridge and Nauenberg, 2002]. Further, thanks to Propositions 1 and 2, Book 1, time was represented by the area swept by the radius vector.[7] The possibility of establishing a proportionality between force and displacement, or curvature, allowed Newton to geometrize force. The language of geometry is thus what permits the modeling of the world of forces and accelerations, once forces and accelerations are expressed in terms of displacements and curvatures.

Newton's option for geometrical methods fits well with his critical attitude towards symbolism. As the years passed by, he developed a more and more acute hostility towards modern analytics. It would be certainly excessive to say that Newton abandoned completely the "new analysis" that he had developed in his anni mirabiles. Mathematical achievements in algebra, published in 1707 as *Arithmetica universalis*, come from the 1670s. In later years Newton continued to be interested in the algebraic classification of cubic curves, in integration techniques, and in power series. However, we can safely say that after the 1670s Newton contrasted geometrical methods with algebraical ones, with the purpose of showing the superiority of the former to the latter, a superiority which he often emphasized.

Newton often characterized the symbolical methods of algebra and calculus as merely heuristic tools devoid of scientific character. During the priority dispute with Leibniz he affirmed that the differential and integral calculi were just useful in the art of discovery, but of no use in the science of demonstrations. He maintained that his geometric method of fluxions, instead, was founded in the ancient practice of exhaustion techniques and was endowed with safe referential content. During the priority dispute he wrote with disdain: "Mr Leibniz's [method] is only for finding it out" [1967–1981, VIII, 598].

[7] See the papers by Erlichson and Nauenberg in this issue.

Newton's invectives against the use of symbols in the modern analytics resemble those of Thomas Hobbes and Isaac Barrow [Jesseph, 1999, 189 ff; Pycior, 1997, 135 ff; Mahoney, 1990]. His position, however, was more complex. There is undoubtedly a tension in Newton's philosophy of mathematics. Newton was too good an algebraist to completely rule out the analysis of the moderns. As I said above, his position seems to have that of downgrading algebra and calculus to heuristic tools.

The circumstances surrounding the publication of the *Arithmetica universalis* are interesting. It appeared anonymously in 1707. Newton made it clear that he was compelled to publish in order to obtain the support of his Cambridge colleagues in the election to the 1705 Parliament and did not allow his name to appear on the title page [Westfall, 1980, 626, 648–649]. In the opening "To the Reader" it was stated that the author had "condescended to handle" the subject. The *Arithmetica universalis* also ended with oft-quoted statements in favour of pure geometry and against the "Moderns" who had lost the "Elegancy" of Geometry:

> Geometry was invented that we might expeditiously avoid, by drawing Lines, the Tediousness of Computation. Therefore these two sciences [...] ought not be confounded. The Ancients did so industriously distinguish them from one another, that they never introduced Arithmetical Terms into Geometry. And the Moderns, by confounding both, have lost the Simplicity in which all the Elegancy of Geometry consists. [1964–1967, II, 228]

In his mature life Newton published symbolical mathematics, but he insisted in conveying to his reader the idea that such works did not exhaust the scope of his mathematical activity and that geometrical works were superior.

In the *Arithmetica Universalis* Newton made it also clear that the equation does not define the curve. The *Arithmetica Universalis* might be considered an exercise in Cartesian algebra, but—as we noted above—it ended with an Appendix where most of Descartes's methodology concerning curves was rejected:

> it is not the Equation, but the Description that makes the Curve to be a Geometrical one. The Circle is a Geometrical Line, not because it may be expressed by an Equation, but because its Description is a Postulate. It is not the Simplicity of the Equation, but the Easiness of the Description, which is to determine the Choice of our Lines for the Construction of Problems. [1964–1967, II, 226]

Newton went on to observe that the equation of a parabola is simpler than the equation of the circle. However, it is the circle which is simpler and to be preferred in the solution of problems. Descartes's classification of curves as a function of the degree of the equation is not relevant for the geometrician who will choose curves as a function of the simplicity of their mechanical description:

> But Algebraick Expressions add nothing to the Simplicity of the Construction. The bare Descriptions of the Lines only are here to be considered. [1964–1967, II, 227]

Newton observes that from this point of view the conchoid is a quite simple curve. Independent of considerations about its equation, its mechanical description is one of the most simple and elegant: only the circle is simpler. Newton was thus ending his treatise on Cartesian algebra by stating the secondary importance of equations and by insisting—as he does in the very first lines of the *Preface* to the *Principia*—on the fact that geometrical objects should be conceived of as generated mechanically, that geometry is subsumed under mechanics.

The first half of the *Preface* to Newton's *Principia* is devoted to defining "rational mechanics" as opposed to "practical mechanics," to discussing its relationship with geometry and its use in the investigation of nature. Newton affirms that geometry is founded upon mechanical practice and that it is part of universal mechanics. He also denies that exactness appertains exclusively to geometry: quite the contrary, geometry receives its exactness from mechanical practice. In stating these theses in the very first lines of the *Principia* Newton was distinguishing his mathematical method from the one defended by Descartes in the *Géométrie* (1637). Descartes had defined "geometrical" as "what is perfect and exact" and "mechanical" as what is not so. He had, therefore, banished "mechanical" curves from geometry and admitted only those curves which are defined by an algebraic equation. Since his early studies on the fluxional method, Newton had aimed at developing a method for studying properties of both geometrical and mechanical curves. He did so by conceiving curves as generated by motion and by admitting infinite series as legitimate representations of curves. Newton was convinced that studying geometrical magnitudes in terms of their mechanical construction opened access to a much more general approach than Descartes's.[8]

Newton expressed time and again his conviction that geometrical objects should be conceived as generated by motion: geometry, from his point of view, was subsumed under mechanics. Mechanics, in fact, provides geometry with its subject matter, and it does so with a rich variety of mechanical constructions. As Newton wrote in the *Preface* to the *Principia*:

> For geometry postulates [postulat] that a beginner has learnt to describe lines and circles exactly [accurate] before he approaches the threshold of geometry; and then it teaches how problems are solved by these operations. To describe straight lines and to describe circles are problems, but not problems in geometry. Geometry postulates the solution of these problems from mechanics; and teaches the use of the problems thus solved. And geometry can boast that with so few principles obtained from other field, it can do so much. Therefore geometry is founded on mechanical practice [praxi mechanica] and is nothing other than that part of universal mechanics which reduces the art of measuring to exact proportions and demonstrations. [1999, 382]

The use of the verb *postulare* should be noted. The role and meaning of postulates in ancient Greek geometry, and their distinction from *axioms*, is a vexed interpretative question. Newton seems to conceive postulates as "the existence-claims of geometry; they are what geometry is ultimately about" [Garrison, 1987, 611]. Traditionally, postulates provided a means to generate some elementary constructions by the use of mechanical devices, such as rulers and compasses. For instance, in Euclid's *Elements* one reads: "Let the following be postulated: 1. To draw a straight line from any point to any point. 2. To produce a finite straight line continuously in a straight line. 3. To describe a circle with any centre and distance" [Euclid, 1926, I, 154]. As Molland observes, these postulates "lay down conditions under which straight lines and circles may be constructed" [Molland, 1991, 185]. But Newton's geometrical ontology was much larger, for he had to admit not only figures constructible by ruler and compass. Descartes, as we know, had already enlarged the scope of geometry by admitting all algebraical curves (curves defined by an indeterminate polynomial equation) as legitimate. Newton wished to admit the mechanical curves (such as the spiral or the cycloid) too. Consequently Newton's notion of postulate is much broader than both in classic and Cartesian geometry: "[for Newton] it is motion which describes the postulates and serves as the foundation of geometry" [Garrison, 1987, 611]. Any curve generated by continuous motion is, in Newton's terminology, a "fluent quantity" and, as such, a legitimate object of geometry. In the early 1710 Newton wrote a manuscript in which he pondered the relationships between geometry and mechanics. Mechanics, he affirmed, precedes geometry and is based on a number of postulates. One of these was: "To move a given body by a given force in a given direction" [1967–1981, VIII,

[8] On Descartes's refusal of mechanical curves see Bos [2001, 335 ff].

177]. The mechanical generation of magnitudes performed in mechanics is the assumption on which geometry is based, and this mechanical motion presupposes a mover, a force; it must "originate from the activity of some agent" [Garrison, 1987, 611]. In fact, Newton stated in a manuscript treatise on geometry written the 1690s, that the mechanical generation of magnitudes can be performed by "God, nature or any technician." Echoing the *Preface* to the *Principia*, Newton wrote:

> Geometry neither teaches how to describe a plane nor postulates its description, though this is its whole foundation. To be sure, the planes of fields are not formed by the practitioner [ab artifice] but merely measured. Geometry does not teach how to describe a straight line and a circle but postulates them; in other words, it postulates that the practitioner has learnt these operations before he attains the threshold of geometry. [...] Both the genesis of the subject-matter of geometry, therefore, and the fabrication of its postulates pertain to mechanics. Any plane figure executed by God, nature or any technician [a Deo Natura Artifice quovis confectas] you will are measured by geometry in the hypothesis that they are exactly constructed. [1967–1981, VII, 338–343]

Maurizio Mamiani in [1998] has noticed a connection between Newton's mechanically based fluxional geometry and certain aspects of the *De gravitatione et aequipondio fluidorum*—a metaphysical manuscript whose dating is still object of disagreement—where Newton writes that:

> the analogy between the Divine faculties and our own is greater than has formerly perceived by Philosophers [since] in moving Bodies we create nothing nor can we create anything, but we only simulate the power of creation [...] if anyone prefers this our power to be called the finite and lowest level of the power which makes God the Creator, this no more detracts from the divine power than it detracts from God's intellect that intellect in a finite degree belongs to us also. [Newton, 1962, 141–142]

Mechanically described figures, curves in particular, are thus generated by a faculty that mimics Nature and God.[9]

Sepkoski's thesis, according to which Newton was a moderate mathematical constructivist, not only fits well with Newton's conviction that geometry is based upon mechanical practice, that geometrical objects have to be seen as mechanically produced, but is in harmony also with what Cohen has taught us about Newton's use of mathematical models [Cohen, 1980]. According to Cohen, in the *Principia* Newton used mathematical constructs as successive approximations to reality. Each model was seen by Newton as physically false, since it was based on simplifying assumptions that violated the basic laws of motions. For instance, in Proposition 1, Book 1, he considers a single body acted upon by a central force directed towards an immovable point, a situation which contradicts the third law of motion, but which approximates the motion of a light satellite attracted by a massive body. However, in the *Principia* Newton made use of a series of models that adhere more and more closely to actual reality as "each successive idealization extends the one preceding it by dropping an assumption that simplifies the mathematics" [Smith, 2001, 250].[10]

As a concluding remark to this section one can say that it would be excessive to attribute to Newton a definite philosophy of mathematics: he was not committed to philosophy to the extent of Descartes, Hobbes, or Leibniz. However, he held philosophical views that shaped his mathematical research. In a way, one could say that he was both a realist and a constructivist, since the mechanically based geometrical constructs are meant to approximate the real "geneses [which] take place in the reality of physical nature and are daily witnessed in the motion of bodies."

[9] On Newton's constructivism in geometry see Garrison [1987], Dear [1995, 212]. On the relationship between Newton's conceptions of God and his method of fluxions see Ramati [2001].

[10] See also Smith [2002].

2.3. Logic and background material

> Mathematicians could not express their proofs in a formal logical system even if they wished to because mathematical arguments are not merely formal. [...] The inferences appeal to features of the non-logical content, which is why one has to understand so much background material in order to grasp a mathematical argument. [Larvor, 2001, 221]

I believe that it can be safely admitted that the logic of a mathematical proof is invariant: it is not context-dependent. This is why the technical debate on the cogency of certain propositions of Newton's *Principia* is legitimate, informative, and fascinating. The mathematicians and physicists who have recently opened the *magnum opus* to test the cogency of its demonstrations have raised our understanding of the niceties of Newton's mathematical natural philosophy.

However, mathematics, like any other human enterprise, does develop in a context, in continuous relation—as Kitcher and Corry maintain—with other scientific disciplines and with culture in general. As Larvor maintains in the lines quoted in the opening of this section, in order to grasp a mathematical argument one has to be informed about background material that is beyond the reach of mere logical inspection of the demonstrative steps. I hope I have argued convincingly that contextual history is useful for achieving a historical understanding of a technical text such as the *Principia*. In Section 2 we have seen how Newton's cultural and philosophical convictions were intertwined with his characteristic mathematical practice. This practice—well known to historians of mathematics—includes reluctance to publish the calculus, preference for geometry and aversion toward symbolism, mathematical classicism, and the subordination of geometry to mechanics rather than to algebra. All these aspects profoundly shape the mathematical style of the *Principia*. Thus, I believe that an understanding of Newton's mathematical practice adopted in the *Principia* cannot be achieved without a serious collaboration between historians of Newton's mathematics and historians of other sectors of Newton's thought.

But can we go deeper in our attempt to bridge the gap between technical and cultural Newtonian researches? I began my paper by noting that historians of technical aspects of Newton's mathematization of motion work somewhat in isolation, and that connections with historians of cultural aspects of science are seldom established. After the brief presentation of Newton's philosophical ideas proposed above, the issue remains of whether it is possible to make use of knowledge about Newton's philosophical ideas in order to articulate open questions debated in the technical literature. In the remaining part of this paper I will attempt to move toward a contextualist reading of some aspects of Newton's mathematical astronomy.

3. Looking for intersections between conceptualism and contextualism

3.1. Michael Nauenberg and Curtis Wilson on Newton's mathematization of lunar theory

I will consider the recent studies by Michael Nauenberg and Curtis Wilson of Newton's approach to the study of the Moon's motion [Nauenberg, 2000, 2001; Wilson, 2001]. These works are particularly interesting since Nauenberg and Wilson are among the finest historians of Newton's mathematical astronomy. The topic they are discussing is one of the most technical parts of Newton's celestial mechanics. We are thus lucky to have access to these impervious subjects thanks to the guidance of these two technically adept historians. They both read Newton's demonstrations in fine detail, they both have a good command of modern perturbation theory, but, this notwithstanding, they reach somewhat contrasting conclusions concerning the nature of Newton's achievements on the three-body problem. The contrasting conclusions reached by Nauenberg and Wilson, even though they concern a specific topic of Newton's *Principia*, are in a way typical of 20th-century commentators of Newton's mathematical astronomy. Their disagreement, in fact, concerns a question that recurs in the literature: Are Newton's geometrical methods adopted in the *Principia* equivalent to the calculus? While I believe that this issue can be settled only by historians of technical aspects of Newton's mathematics, I will argue that knowledge of the philosophical context adds a new dimension to our understanding of this question.

In the *Principia* Newton dealt with lunar theory in Book 3, Propositions 22 and 25–35. In these propositions Newton dealt with three lunar inequalities; one of these is the "variation," an effect discovered by Tycho Brahe consisting in the fact that the Moon "maximally lags behind its mean position in the octants before the syzygies, and maximally exceeds its mean position in the octants after the syzygies" [Wilson, 2001, 141]. Newton dealt also with the motion of the nodes and the variation of the inclination of the Moon's orbit. In the *Principia* Newton does not deal with the motion of the Moon's apogee, apart from a very limited mathematical approach contained in Propositions 43–45, Book 1. There are, however, a number of manuscripts in the Portsmouth collection, which was presented to the University Library of Cambridge at the end of the 19th century, where a perturbation method is applied to the Moon's apsidal motion [Newton, 1967–1981, VI, 508–535]. In their papers Nauenberg and Wilson deal with Newton's treatment of the variation (most notably, Propositions 26 and 28, Book 3) and with the Portsmouth papers.

One of the questions debated by Wilson and Nauenberg is the equivalence between Newton's methods and those employed by mid-18th-century mathematicians, such as Euler, d'Alembert, and Clairaut. Wilson recognizes that

> Newton in his assault on the lunar problem during the 1680s relied crucially on techniques he had developed in his period of intense algebraic exploration, from the mid-1660s to the early 1670s. These techniques include use of the binomial theorem for approximations, determination of the curvature of algebraic curves at given points, and integration of sinusoidal functions. [2001, 139].

However, Wilson maintains that Newton's methods are far from being equivalent to those of Euler, Clairaut, d'Alembert, and the other great Continental 18th- and 19th-century mathematicians. He writes: "Newton's methods contrast sharply with those of the Eulers [i.e., Leonhard and Johann] and [George William] Hill" [2001, 139], and "success came for Newton's successors only with a new approach, different from any he had envisaged: algorithmic and global" [2001, 140].

Let us briefly consider Propositions 26 and 28. In Proposition 26 Newton aims at determining the variation in velocity of the Moon, due to the solar perturbing force, as it moves along a trajectory that is assumed circular with the Earth at the center T (see Fig. 1) and CD the line of quadratures. From Proposition 25 Newton has a quantitative estimate of the ratio of the mean values of the perturbing accelerative force of the Sun on the Moon to the Earth's much greater accelerative force on the Moon. Such an estimate is achieved by several approximations (for instance, the Sun is assumed to be infinitely distant), the use of binomial expansions (which are truncated), and several trigonometrical relations. Such algebraical calculations are not rendered explicitly in the *Principia* and must be reconstructed by the reader. Newton represents the acceleration of the Moon as seen from an observer at rest on the Earth by the line LT and decomposes LT into two components, a radial component ET and a transverse component LE. In order to find the variation of the Moon's speed "as it moves from C to any point P between C and A, it is necessary to integrate [in Newton's terminology to "square"] the acceleration LE

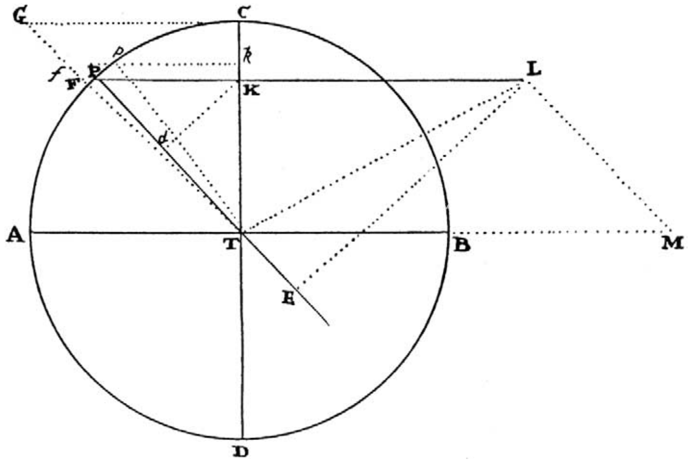

Fig. 1. Newton's diagram for Proposition 26, Book 3, from Newton [1999, 841].

over time" [Wilson, 2001, 144].[11] Newton constructs *GC* at right angles with, and equal to, the circle radius *TC* and shows that such a quadrature is equivalent to summing the infinitesimal stripes, *FKkf* (that is, *LEdt* is proportional to *FKkf*, where we use the Leibnizian *dt* for the infinitesimal increment of time—notice that *dt* is proportional to *da*, where *a* is the angle *CTP* of mean motion of the Moon). Newton does not publish the integration in print, but from the numerical results he achieves it is clear that he must have performed the integral of $\sin(2a)da$. In fact, Newton writes that the increment in rate of areal description is proportional to the "versed sine of twice the distance of the Moon from the nearest quadrature" [Newton, 1999, 843]; i.e., it is proportional to $(1 - \cos 2a)$ (where a is angle *PTC*). So Newton must have performed the integration, but does not reveal the details to the reader. Now Newton has an estimate of the ratio of the velocity at quadratures to the velocity at syzygies. In Proposition 28 Newton tries to determine the variational orbit. As Wilson puts it, "the assumption is that in the absence of the perturbing accelerations *LE* and *ET* the Moon would move uniformly in a circle about the Earth's center" [Wilson, 2001, 146]. So Newton assumes a circular orbit and shows how it must be modified if the perturbing force of the Sun is switched on. As Nauenberg has shown in Nauenberg [1994], here Newton deploys a measure of force in terms of curvature; namely, Newton applies Huygens's law locally. According to this law a body that moves along a circle with constant angular speed has a centripetal acceleration whose strength is proportional to the square of linear speed and inversely proportional to the radius. In the case of a general trajectory Newton knew from his earliest studies on the motion of bodies that the strength of the normal component of total force at a general point P of the trajectory is proportional to the square of the instantaneous speed at P divided by the radius of curvature at P. This physical insight is applied in Proposition 28. Since Newton has (a) from Proposition 25 an estimate of the ratio of the radial component of the perturbing accelerative force of the Sun on the Moon to the accelerative force of the Earth on the Moon[12] and (b) from Proposition 26 an estimate of the ratio of the velocity at quadratures to the velocity at syzygies, he can apply Huygens's law locally in order

[11] In fact the radial component *ET* will not cause any transverse acceleration.

[12] In fact the total radial accelerative force at quadratures and syzygies consists in the Earth's attraction and an added or subtracted radial perturbing accelerative force due to the Sun.

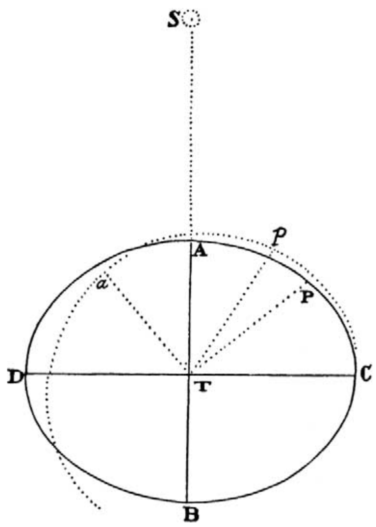

Fig. 2. Newton's diagram for Proposition 28, Book 3, from Newton [1999, 845].

to determine the ratio of curvature at quadratures to that at syzygies. Newton's "general strategy is to determine, in two different ways, the ratio of the curvature of the orbit at syzygies to its curvature at quadrature. The two expressions of the ratio are set equal, and from the resulting equation [Newton] obtains algebraically a numerical value for the ratio of the orbital radius at quadrature to the orbital radius at syzygy" [Wilson, 2001, 146]. To obtain a second expression for this ratio Newton assumes that in the presence of the perturbing force the unperturbed circular orbits is deformed into an elliptical orbit such that the greater axis *CD* lies between the quadratures; i.e., the Sun is at *S* (see Fig. 2), the Earth remaining situated in the center T. Notice that the ellipse rotates with the Sun (see the dotted line).

Here we will not follow Wilson in his masterful identification of flaws and drawbacks in Newton's demonstrations in any detail. What is interesting for us are the conclusions he reaches. In analyzing Proposition 28, Book 3, he notices that "Newton does not construct the orbit [the flattened elliptical orbit of the Moon] starting from the forces, but rather *assumes* a circular orbit, and then shows how it must be modified" [2001, 144]. In fact, Newton writes that "since the figure of the lunar orbit is unknown, let us, in its place assume an ellipse DBCA, in whose center the Earth is placed" [Newton, 1999, 845]. Instead of deducing the orbit from some basic equations, Newton began with some simplifying assumptions concerning the shape of the Moon's orbit (circular in Propositions 25 and 26, elliptical in Proposition 28) and tried to establish the perturbation caused by the Solar force. According to Nauenberg, Newton's solution of the lunar variations in Proposition 28 "corresponds to the periodic solution obtained later by L. Euler and in full detail by G.W. Hill" [2001, 198]. However, as Wilson states, Newton's approach led to several problems. Most notably, the level of approximation was not under control. There was no internal check on the level of accuracy of the various approximations introduced. The method followed by the Eulers and Hill, according to Wilson, was instead profoundly different, since they "started from differential equations that stated exactly the conditions of the problem," and "reference to the differential equations [...] controls the successive approximations" [2001, 153]. Wilson agrees with Nauenberg in recognizing that Newton in Propositions 26 and 28 employed algebraic techniques, most notably binomial expansions, trigonometrical relations, integrations of sinusoidal functions, and calculations of

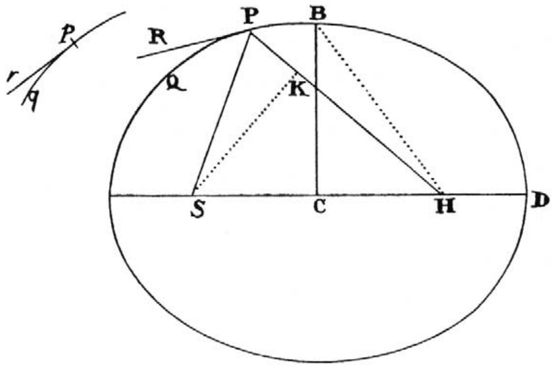

Fig. 3. Newton's diagram for Proposition 17, Book 1, from Newton [1999, 470].

curvatures of algebraic curves, but he did so in a completely different way than Euler or Hill. (a) Newton's use of such algorithmic techniques was not systematic ("Newton on many occasions formulated and solved problems [in celestial mechanics] algebraically, but he did not do so always or as a matter of policy" [2001, 171]). Newton's use of algebraic techniques was not *methodical*. That is, Newton used algebraic techniques in certain passages of his demonstrations, which were fundamentally geometric in character. Newton's demonstrations were always assisted by geometrical and physical insights which were often unjustified (in particular, the degree of approximation was not under control). Instead "both for Euler and Clairaut, dynamical insight was required for the formation of the initial differential equations, but not thereafter" (p. 179). Further, (b) Newton did not deduce the Moon's orbit from the equations of motion but, rather, "assumed a fictive shape (a concentric circle) and reasoned to the modification in it that the perturbing forces would require" (p. 171).

Nauenberg shows that a much more general approach to perturbation theory is developed in the Portsmouth papers, where Newton faces the problem of the determination of the motion of the Moon's apogee. Nauenberg's study of these intricate papers is to be welcomed as a decisive advance in our understanding of Newrton's treatment of the three-body problem. The Portsmouth method depends upon a technique expressed in two corollaries (Corollaries 3 and 4) to Proposition 17, Book 1, added to the second edition of the *Principia*.

Proposition 17 is related to the solution of the so-called inverse problem of central forces. Here Newton considers a body of given mass accelerated by a centripetal inverse square force directed towards S. The body is fired at P in the direction PR with a given initial speed (see Fig. 3). Newton *assumes* that the trajectory is a conic section and that one of the foci is located at the force center S. He determines the unique conic that satisfies the given initial conditions. According to Proposition 16 (again *assuming* that the trajectory is a conic) the initial conditions determine the *latus rectum*, L, of the conic trajectory.[13] Because of the reflective property of conics, the lines joining P with the foci make equal angles with the tangent at P, therefore the direction of the line where lies the second focus H is also determined. Newton

[13] In modern terms one can render Proposition 16 as follows. The semi-*latus rectum*, $L/2$, of the sought conic trajectory is $L/2 = b^2/a = (v^2 SY^2)/(mk) = h^2/(mk)$, where v is the initial speed, SY falls perpendicularly on the tangent at P, a and b are the major and minor semiaxes, h is angular momentum, m, the mass, and k is a constant such that $F = -k/SP^2$. Thus the given initial conditions determine the *latus rectum* of the conic trajectory.

shows that the following geometrical property holds for any conic section:

$$\frac{SP+PH}{PH}=\frac{2(SP+KP)}{L},$$

where SK falls orthogonally on PH. Since SP, L, and KP are unequivocally determined by the initial conditions, PH also is given. And the second focus is thus found. We have now enough information to build the required conic: we have the foci S and H and the major axis $SP + PH$.[14] Proposition 17 allows one to scale the conic trajectory for an inverse square force in function of the initial conditions. For $L < 2(SP+KP)$ the conic is an ellipse, for $L = 2(SP+KP)$ it is a parabola, and for $L > 2(SP+KP)$ it is an hyperbola.

The technique employed in Proposition 17 can be applied, as Newton states in Corollaries 3 and 4, to the study of a perturbed Keplerian orbit. One of Nauenberg's insights is that these two corollaries are crucial for an understanding of the Portsmouth method. They read as follows:

> Corollary 3. Hence also, if a body moves in any conic whatever and is forced out of its orbit by any impulse, the orbit in which it will afterward pursue its course can be found. For by compounding the body's own motion with that motion which the impulse alone would generate, there will be found the motion with which the body will go forth from the given place of impulse along a straight line given in position.
> Corollary 4. And if the body is continually perturbed by some force impressed from outside, its trajectory can be determined very nearly, by noting the changes which the force introduces at certain points and estimating from the order of the sequence the continual changes at intermediate places. [Newton, 1999, 472]

These two corollaries pave the way to evaluating the effect of a perturbing force over a two-body system. The perturbing force is subdivided into a series of impulses acting at equal intervals of time. After each impulse it is possible to determine the velocity of the point mass and use this velocity as a new initial condition. Applying Proposition 17 one can then determine the parameters of the conic along which the body will move until the next impulse causes a successive change of parameters. In Nauenberg's words "repeated application of Proposition 17 determine the Moon's perturbed orbit as a sequence of elliptical arc segments joined together" [Nauenberg, 2001, 201–202]. It is this more general approach that Newton employed in determining the motion of the Moon's apogee. As Whiteside, in a seminal paper, and Nauenberg and Wilson in their recent studies show, in the Portsmouth papers Newton made several mistakes, and in fact his results on the Moon's apogee did not find their way in the *Principia* [Whiteside, 1975].

In analyzing the Portsmouth method Wilson and Nauenberg express a certain disagreement about the translatability of such a method in terms of differential equations. From Nauenberg's analysis it would seem that such a translation is quite straightforward. Nauenberg maintains that this method "corresponds to the variation of orbital parameters method first developed by Euler and afterwards by Lagrange and Laplace" [2001, 189]. Nauenberg is not the first one to have maintained this thesis. For instance, Chandrasekhar quotes with approval François Félix Tisserand, who affirmed that Newton undoubtedly had in his possession these equations for his treatment of lunar perturbations [Chandrasekhar, 1995, 57]. According to Nauenberg:

[14] I have followed in broad outlines the analysis of Proposition 17 given by Chandrasekhar [1995, 108].

> Newton's physical and geometrical approach leads directly to differential equations [...] for the parameters of the revolving ellipse [...] this method corresponds precisely to the modern method of the variation of orbital parameters attributed to Euler, Lagrange, and Laplace. [2001, 191].

Wilson is much more cautious and rather thinks that methods equivalent to Leibnizian integrations occur only sporadically in a demonstrative context which is basically driven by (often unjustified) physical and geometrical insights.

These brief remarks do not due justice to Nauenberg's and Wilson's sophisticated researches, but I hope they suffice to indicate how the basic question which we referred to above emerge in the context of this discussion. While I believe that a resolution of such historiographic issues can come only from the study of technical details of Newton's mathematics, I will try to inquire whether knowledge of the philosophical context can help us in looking at the analyses carried out by Nauenberg and Wilson from a slightly new perspective.

3.2. The context again

Our study of Newton's philosophical views in Section 2 reveals a context in which his demonstrations might be considered from a different point of view. We know that Newton was a follower of Greek exemplars: most notably, he was profoundly impressed by Pappus's tantalizing description of the method of analysis and synthesis in Book 7 of the *Collectio*. In late antiquity analysis (or "resolution") was conceived of as a method of discovery, or a method of problem solving, which, working step by step backwards from what was sought as if it had already been achieved, eventually arrived at what is known. This, and similar, rather vague definitions were aimed at describing in a general way a whole apparatus of geometric problem solving procedures developed by the Greeks. Synthesis (or "composition") goes the other way round; it starts from what is known and, working through the consequences, it arrives at what is sought. The axiomatic and deductive structure of Euclid's *Elements* was the model of the synthetic method of proof. Analysis (or *resolutio*) was often thought of as a method of discovery preliminary to the synthesis (or *compositio*), which, reversing the steps of the analytical procedure, achieves the true scientific demonstration. Analysis was thus the working tool of the geometer, but it was only with synthesis that one could achieve indisputable demonstration. In his numerous manuscripts related to the method of analysis and synthesis, Newton often quoted from the introduction to the seventh book of Pappus's *Collectio*. In a treatise on geometry that Newton composed in the early 1690s we find the following passage freely taken from Pappus:

> Resolution, accordingly, is the route from the required as it were granted through what thereupon follows in consequence to something granted in the composition. For in resolution, putting what is sought as done, we consider what chances to ensue [...] proceeding in this way till we alight upon something already known or numbered among the principles. And this type of procedure we call *resolution*, it being as it were a reverse *solution*. In composition, however, putting now done what we last assumed in the resolution and here, according to their nature, ordering as antecedents what were before consequences, we in the end, by mutually compounding them, attain what is required. And this method we call *composition*. [Newton, 1967–1981, VII, 307]

I believe that in the *Principia* one can individuate a tradition of geometrical problem-solving at work deeply rooted in Newton's reinterpretation of the classical tradition. Newton was actually reinterpreting this tradition into a completely new area: the geometrization of motion. His geometrical natural philosophy was thus very innovative, notwithstanding Newton's classicist rhetoric.

Rather than discarding Newton's treatment of force and motion as inferior compared to mid-18th-century analytic mechanics, we should try to be more respectful of what Newton was seeking and should try to understand which questions he was trying to answer. Rather than subjecting Newton's demonstrations to exigencies which emerged much later (such as generality of methods, algorithmic power, systematicity), it is better to take into consideration the tacit dimension—to use Polanyi's terminology—of such demonstrations.

In ancient and early modern mathematics what was required by the solution of a "problem" was the construction (by means considered as legitimate) of a geometric object from elements that were assumed as "given." The distinction between problems and theorems was highly significant for Newton's contemporaries: it was a distinction that went back to Euclid's *Elements*. The *Principia*'s propositions, as in the *Elements*, are neatly divided into theorems and problems. The propositions considered by Wilson and Nauenberg, namely, Propositions 26 and 28, Book 3—as well as Proposition 17, Book 1—are classified in the *Principia* as "problemata." Newton's purpose in these propositions was thus that of obtaining mechanically generated geometrical constructs that satisfied given dynamic conditions.

Further, we should notice what we said above about Newton's conception of a mechanically based geometry. In constructing geometric objects, the geometer mimics God's faculty of continuous providential intervention, which is manifested in the heavenly motions. The existence of the referents of geometry (e.g., planetary orbits) does not come from within mathematics, but is provided from outside. Otherwise said, for Newton geometrical objects are human fabrications, which are constructed mechanically. The purpose of the geometer is to give rise to constructions that mimic the real motions that exist in nature. The fact that Newton begins, as Wilson observes, his demonstrations in lunar theory with a simplified geometrical model of the Moon's orbit is thus perfectly in line with his aim of attaining a progressive succession of geometrical constructs that approximate the real motion of the Moon (this is the essence of what Cohen calls the "Newtonian style" in Cohen [1980]).

The peculiar geometrical character of Newton's problem-solving techniques, rooted as they are in the Pappusian tradition, does not imply, however, that in his mathematization of motion he never made recourse to algorithmic techniques. What we said above about Newton's dual publication strategy should make us aware of the plausible existence of hidden parts behind the printed text of the *Principia* (and we really need the keen mathematical eye of people such as Chandrasekhar, Nauenberg, Whiteside, and Wilson to spot these!). It should be observed in this context that part of the difficulty in reading the *Principia* resides in the fact that many of its demonstrations are *incomplete*, as we have seen in our cursory analysis of Propositions 26 and 28, Book 3. In the *Principia* one can find cases in which Newton skips important passages in his demonstrations, and very often such gaps can be filled only by recourse to algorithmic techniques (most notably, infinite series and integrations). In the printed text Newton usually obliquely refers the reader to methods whereby "curvilinear figures" can be squared.[15] Only in one instance an explicit reference to the "method of fluxions" occurs.[16] When we read the manuscripts that circulated in the Newtonian circle we find that Newton discussed the algorithmic methods necessary to fill the gaps present in the printed text of the *Principia* [Guicciardini, 2003]. Perhaps the most extraordinary manuscript is David Gregory's *Notae in Newtoni Principia Mathematica Philosophaie Naturalis*, where one can discover a great deal about Newton's ability to apply series and integrations to the science of

[15] See, e.g., Newton [1999, 529–533].
[16] [Newton, 1999, 884].

motion.[17] Newton's ability to apply fluxions and series to mathematical astronomy is evident both from an internal analysis of Newton's printed text and from knowledge acquired from his correspondents and manuscripts circulated within his circle.

Was Newton able therefore to mathematize mechanics in calculus terms? For instance, is Nauenberg right in stating that Newton's methods in lunar theory are "equivalent" to Euler's? These are ambiguous questions which must be qualified. An analysis of the *Principia* and knowledge of Newton's correspondence and manuscripts refutes the widespread belief that he was unable to apply series and fluxions to the science of motion, but does not imply—as Wilson shows—that he was in possession of tools comparable to those acquired by, say, Euler or d'Alembert. In translating Newton's demonstrations of the *Principia* into Leibnizian calculus terms, another widespread practice (see Chandrasekhar [1995]), we risk both betraying his style and overlooking the mathematical limitations of his methods. We risk projecting on his method of series and fluxions a modernity which is extraneous to it.

In the first place, one should note that Newton referred to his algorithmic youthful discoveries as a "method" of series and fluxions, not as a "calculus." He conceived and practiced his method within the 17th-century tradition of heuristic problem-solving techniques recently studied in Bos [2001]. One of the main concerns of 17th-century mathematicians was to develop methods aimed at determining properties of geometrical objects, most notably curves. Since Descartes's *Géométrie* (1637) such problems were approached by application of algebraic techniques. These techniques were, however, subsidiary to geometrical ones. As Bos shows, from Descartes' point of view, the equation was just part of the definition of a curve, since, in order to practice geometry, one had to exhibit a geometric construction of the curve. Descartes did not depart from the ancient Pappusian approach according to which a solution is known only if it can be constructed starting from geometric elements considered as given at the outset: "by 1650 an equation was a problem whose solution was a construction; by 1750 problems as well as their solutions were couched in terms of equations or analytical expressions" [Bos, 2001, 427]. So algebra, often referred to as "common analysis," was practiced within the framework of a tripartite method. The first step was—in accordance to Pappus's analytic method—to assume the solution as given. By assigning symbols to geometrical magnitudes (known, a, b, c, and unknown, x, y, z) this translated into the "discovery" of an algebraic equation ["aequationis inventio"]. The second step consisted in manipulating the equation in order to express the sought quantity (let us say, x) in terms of the givens (let us say, a, b, and c). But this did not end the process. As Bos remarks, one had to proceed with a geometrical construction that exhibited the solution, and there was an extensive debate concerning the legitimacy of constructions tools. The "construction of the equation" ["aequationis constructio"] consisted in constructing geometrically the magnitude x as function of the given magnitudes a, b, and c. This last, third, step was synthetic, since the sought magnitude was built from the givens. In a manuscript dating from the early 1690s Newton described this tripartite method as follows:

> if a question be answered [...] that question is resolved by the discovery of the equation and composed by its construction, but it is not solved before the construction's enunciation and its complete demonstration is, with the equation now neglected, composed. Hence it is that resolution so rarely occurs in the Ancient's writings outside Pappus's *Collection* [...] to be sure, it is the duty of geometers to teach unskilled men and mechanics, and resolution is ill-suited to be taught to the masses. [Newton, 1967–1981, VII, 307]

[17] Royal Society Library MS 210.

Thus, according to Newton, a problem is solved by the geometrical construction (or synthesis), while the resolution (the analytical step) consisting in the discovery and solution of the equation can be neglected, and—following the Ancient's practice—avoided in publication.

In his youthful studies Newton was continuing the Cartesian tradition by developing what he termed a "new analysis": an extension of the "common analysis" of the Cartesians. Newton's new analysis was the method of series and fluxions, and this method, as much as the Cartesian one, was subservient to geometry. Newton began with a specific geometric problem, by manipulating geometrical linelets he "discovered" a fluxional [differential, in Leibnizian terms] equation (first step), and the "quadrature" of such an equation (second step) ended the analytical part of the problem-solving procedure. Now the synthetic procedure had to follow, and the solution achieved in the second step was constructed synthetically. In the *Principia* Newton did not publish the second step: typically he showed the reader how a certain problem could be reduced to a "quadature" (i.e., the integral of a differential equation), he kept the integration technique hidden and proceeded to publish the synthetic geometrical construction of the solution. Thus the questions of Newton's use of calculus in the *Principia* should be set in terms respectful of the context in which he placed himself as a geometer.

Newton's use of fluxional method was part of a procedure that was basically geometric in character. The algorithmic method intervened only in certain crucial passages of the Pappusian analytic problem-solving technique. For instance, we have seen how Newton in Proposition 26 reaches a result by integration of $\sin(2a)$. However, the integration does not appear in print. In Proposition 28 Newton calculates curvatures by the method of fluxions, but, again, he does not explain to the reader how such curvature calculations can be carried on. As far as the inverse problem of central forces is concerned, we notice that in Proposition 41, Book 1, Newton reduces the problem to the integration of a couple of differential equations. In Corollary 3 to Proposition 41 Newton faces the problem of determining the trajectory of a body fired in an inverse cube central force field. His integration technique was revealed in a letter to David Gregory [Newton, 1959–1977, III, 348–349]. But in the printed Corollary 3 one can find just the "construction" of the solution attained by integration, i.e., a geometrical construction of the Cartesian spirals traveled by a body fired in an inverse cube force field [Erlichson, 1994b; Brackenridge, 2003]. Similarly, in his private papers (the Portsmouth calculation of the Moon's apogee motion), the calculations necessary to solve the three-body problem are carried on in a more explicit way compared with the printed Propositions 25–35, Book 3.

A study of Newton's mathematization of motion cannot be carried out without the technical work of fine historians such as Nauenberg and Wilson. However, such conceptualist researches can be fruitfully embedded in a contextualist setting, since the questions that Newton asked himself, the methods of proof that he accepted or privileged, the hierarchies that guided his research, the standards of publication he adopted were noticeably different from the ones we accept nowadays, and indeed different from the ones accepted by Leibniz and his successors. It is only by having access to this tacit dimension of Newton's methods, to the "background material" of his demonstrations, that we can appreciate the technicalities of the mathematical procedures of the *Principia*. Both the attempts to translate the *Principia*'s demonstrations into modern calculus and the criticisms based upon standards which developed later risk missing the point.[18]

[18] I hope the reader will understand that in no way do I wish to underestimate the excellence of Nauenberg's and Wilson's researches. What I want to advocate is the fruitfulness of a different approach which is complementary to theirs. Further, the

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fact that both these scholars are aware of the necessity of enlarging the scope of research along the lines indicated in my paper is evident to me from the many conversations and e-mail exchanges I had with Michael, and from the perceptive pages that Wilson devotes to Newton's preference for geometry in Wilson [2001, 168–172].

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Niccolo Guicciardini
E-mail address: niccolo.guicciardini@fastwebnet.it