Mathematisches Forschungsinstitut Oberwolfach

Report No. 10/2006

Mini-Workshop: On the Reception of Isaac Newton in Europe

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February 19th – February 25th, 2006

Abstract. A three-volume book on all aspects of the European reception of Isaac Newton's work is in preparation, for publication in 2008 by Continuum Press (London). This workshop involved the important subset of contributors working on Newton's pure and applied mathematics and aspects of his physics, and almost all of the participants are to be authors of one of the chapters. The meeting gave everyone an opportunity not only to rehearse their contributions as far as they had developed them, but also to discuss and sort out overlaps and divisions of labour between their respective chapters, as well as with many other chapters to be written by authors who were not present. The ease and intensity of these exchanges would not have been possible by emails alone, however many of them were sent; the personal contact was crucial. In other words, pure Oberwolfach.

Mathematics Subject Classification (2000): 01A45, 01A50, 01A55.

Introduction by the Organisers

'"Helpful" is not quite the word that comes to mind when considering Isaac Newton'. (Alan Shapiro)

A widely known legend is encapsulated by Alexander Pope's famous two-liner: 'Nature, and Nature's laws lay hid in night. God said, let Newton be! and all was Light.' The built in claim is somewhat oversimplified even for Britain; and it certainly does not capture the complex web of enthusiasm, acceptance, doubt, emulation, objection and opposition that characterises the Continental reactions. The need to consider the actual states of affairs within Newtonianism in general is especially significant, since from the 1740s onwards Britain became breathtakingly mediocre in mathematical research, so that almost everything that mattered was done in Europe.

The purpose of the workshop was to outline and study the details of these complexities. The summaries below show that we divided up the post-Newtonian corpus largely by subject matter and topic. Two presentations dealt with important contemporaries; Leibniz, and the Bernoulli family with Hermann: the book will contain more chapters of this kind. In addition, only one chapter tackled a community as such, namely the French mathematicians from 1780 to 1830: several chapters of the book will be of this type.

While we tried to take Newton's own contributions to be more or less known and clear, Newton's own presentations of his theories, often cryptic in the extreme, forced us back to his texts on many occasions. An important hierarchy of interpretation was exposed: what we today think that Newton was (not) saying, and what we today think that Newton's successors thought that he was (not) saying.

The mini-workshop brought together 15 participants from seven countries, including the two editors of the book. Funds made available by the US Junior Oberwolfach Fellows Funds allowed one of our younger members to take part. Most sessions lasted 75 minutes, including up to 30 minutes of discussion. The measure of further consultation in the evenings and on afternoon walks was considerable. Some subsets of us were already quite well known to each other, but all of us made new contacts and friendships, which will help signally in the preparation of the book over this and the next year.

Workshop: Mini-Workshop: On the Reception of Isaac Newton in Europe

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Abstracts

The reception of Newton's method of fluxions in Europe

N. Guicciardini

Newton's impact on eighteenth-century European calculus is usually judged negatively in comparison with Leibniz's achievement. For instance, according to Morris Kline:

> Excessive reverence for Newton's geometrical work in the *Principia*, reinforced by the enmity against the Continental mathematicians engendered by the dispute between Newton and Leibniz, caused the English mathematicians to persist in the geometrical development of the calculus. But their contributions were trivial compared to what the Continentals were able to achieve using the analytical approach ([1], 392).

The received view is not altogether wrong, but after recent research is in need of revision and better qualification.

First of all, it is apparent that the work carried on by some of Newton's disciples was far from being purely geometrical and remote from European algorithmic style. One can think about works by Roger Cotes, Brook Taylor, James Stirling, Abraham de Moivre and Thomas Simpson as examples of a way of doing mathematics well attuned with European contemporary work. Their work was noted and used in Europe. Even Colin Maclaurin – the great champion of geometrical rigour – devoted the second book of his *Treatise of fluxions* (1742) to the 'computations in the method of fluxions'[2].

The active presence of a group of 'analytical' fluxionists in Britain, who practiced infinite series manipulations in a formal style which one usually attributes to Euler and who dealt with quadrature techniques which demanded considerable skill in manipulating symbols, should not be a surprise to those who are aware of the tensions inherent in Newton's mathematical work.

While in the *Principia* (1687) Newton gave prominence to a geometrical style to which he is usually associated, several of his tracts in new analysis, such as the *De quadratura* (1704) and the *Methodus differentialis* (1711), were carried on in terms which are symbolical.

One has further to recognize that the communities of European and British mathematicians were fragmented in a complex way: any attempt to reduce them to two well-defined opposing groups is refuted by historical evidence[3]. One can think about the role played by French mathematicians such as Pierre Varignon and Pierre Rémond de Montmort who acted as intermediaries between Newton and the Basel group.

Once we have a clearer idea of the complexities of the Newtonian heritage we can move on and consider the impact of the results achieved by the Newtonians on European mathematics. In my chapter I will chart the influence of certain results on integration achieved by Newton, Cotes, de Moivre, Saunderson, and of several results due to Maclaurin.

Further, I will consider the reception of fluxions in France. Here we find two topics of interest. The former is Buffon's translation (1740) of Newton's *Method of series fluxions*, the latter is the initiatives of a group of Jesuits active in Paris at the mid of the century who promoted translations of works on fluxions by Edmund Stone and Maclaurin. These philo-British initiatives were opposed by academicians around Fontenelle. These debates illustrate that endorsing or opposing Newton's method was a question that polarized the French communities around contrasting ideas on how a mathematical natural philosophy should be framed[4][5][6].

I will then move to Italy. I will compare the French Jesuits' campaign in favour of fluxions with the more moderate acceptance of Newton's method by the Italian mathematicians who worked with, or were influenced by, Jacopo Riccati. It seems that these Italian mathematicians (Suzzi, Vincenzo Riccati, Girolamo Saladini, Maria Gaetana Agnesi) did not want to renounce to the advantages of the notations in terms of differentials and integrals, while adopting a foundational outlook which opened to some of Newton's typical concepts. Their motivations for doing so—as the social and academic context in which they operated reveal–might be related to a desire to affirm autonomy from the Basel school, which had dominated the Chair of mathematics in Padua from 1707 to 1719[7][8][9].

In order to complete my research project I have to devote more work on the French and Italian groups. I have to pay attention to other countries as well. It has been also agreed that Newton's work on series and its European reception will be dealt with in the chapter on algebra, as this classification adheres more strictly to the disciplinary boundaries then in use. There is a smooth transition from my chapter, which will cover the period until *ca.* 1770, to Grattan-Guinness' chapter on French science. It would be also very profitable if I could co-ordinate my chapter with the chapters on Leibniz and on the Bernoullis—since the priority dispute was the arena which allowed Europeans to make a first encounter with Newton–and with the chapter on the development of Newtonianism in France—since J. B. Shank might want to consider the role played by Castel and his polemic with Fontenelle.

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Newton's Algebra

J. Stedall

During 1669 Newton made extensive annotations to the *Algebra ofte Stel-Konst* (1661) of Gerard Kinkhuysen. The notes were never published, but in 1683–84 Newton used them again in compiling his Lucasian lectures. It is unlikely that these lectures were ever actually delivered, but the notes for them were edited and published by William Whiston in 1707 as the *Arithmetica universalis*. Thus the *Arithmetica universalis* follows quite closely the structure and content of Kinkhuysen's *Algebra*, a typical seventeenth-century textbook on equation-solving, based on sixteenth-century material with the addition of a few new ideas from Descartes and van Schooten. This gives Newton's *Arithmetica universalis* the appearance of being much more elementary than in fact it is. To the traditional ideas expounded by Kinkhuysen, Newton added some far reaching insights of his own. Often, however, he described rules or procedures without explanation, giving rise to extensive commentary later on. The first discussion of Newton's material appeared in Leibniz's review in the *Acta eruditorum* in 1708, which was eventually followed by several continental editions of the *Arithmetica universalis* in which the commentary far exceeded the original material. In this lecture I provided a preliminary survey of editions, commentary, and discussion of Newton's text during the eighteenth century, but much more research still remains to be done.

The *Arithmetica universalis* is entirely concerned with the traditional subject matter of algebra, namely equation-solving. Hence the title, 'universal arithmetic', or 'symbolic' or 'specious' arithmetic. However, in the course of the seventeenth century, algebra had come to acquire another meaning, usually described by the phrases 'analytic art', or 'analysis'. In this meaning, algebra was understood not as simply a technique, but as a powerful tool for analysing mathematical problems. Newton's 'De analysi' of 1669 took up this new meaning, and applied it by means of infinite series, or as Newton described them, infinite equations.

It can be argued that the discovery of the general binomial theorem and infinite series was as important to the progress of seventeenth-century mathematics as the calculus itself. Further, in 'De analysi', Newton moved mathematics irrevocably away from the old geometric methods towards a new algebraic language, in which all well known mathematical quantities - sines and logrithms, for example - could be expressed algebraically. Any study of Newton's algebra and its influence must therefore take into account not only his work on equations, as set out in *Arithmetica universalis*, but also the new insights of *De analysi*, and their consequences.

Once again, Leibniz was the first to recognize the importance of Newton's discoveries, when he first encountered them in letters from Newton in 1676. By the early eighteenth century, several continental mathematicians, Euler in particular, were using the binomial theorem and infinite series freely in their work, and later Lagrange attempted to use them as the foundation of the calculus itself. Again, there is a story here that requires considerable further research if we are to tease out the detail of Newton's influence.

Celestial Mechanics and Gravitational Theory

M. Nauenberg

In 1687 the first edition of Newton's *Principia* appeared and completely transformed the contemporary understanding of celestial mechanics and gravitation. In Book 1, he gave, for the first time, a complete physical and mathematical description for the origin of Kepler's three empirical laws for planetary motion which were based on the careful observations of Tycho Brahe summarizing the knowledge of astronomers at the time. To accomplish this great feat Newton had to extend thee geometrical methods developed by the Greeks, particularly Euclid and Apollonius, to encompass the limit of ratios of vanishingly small quantities which in the *Principia* were represented by short lines and by arcs of curves. To justify his celebrated 1666 'moon test' (Prop. 4 Book 3), which compared the gravitational force of the earth on the moon to the force of gravity on the earth, he applied the law of universal gravitation, a centrepiece of the *Principia*, to show that a spherical distribution of mass acts as if the total mass is concentrated at its centre (Props. 72–74 , Book 1).

In Book 3 of the *Principia*, Newton applied his mathematical theory of orbital mechanics to the motion of planets and satellites to establish the validity of the universal theory of gravitation in astronomy. He showed that the best available data at the time for the periods and major axis of the planets and the Jovian and Saturnian satellites were in agreement with Kepler's third law (Phenomenon 2–4, Book 3), and then applied Cor. 6 of Prop 4, Book 1, to show that 'if the periodic times are as the three half powers of the radii, the centripetal force will be inversely as the squares of the radii'. In addition, he applied Cor. 1 of Prop. 45 of Book I to show that the near immobility of the aphelia of the planets (Prop. 14, Book 3) also implied a gravitational inverse square force between the planets and the sun. Next, he proceeds to calculate subtle deviations from Kepler's laws which had been found by astronomical observations in the motion of the planets and the moon. He considers various approximate mathematical methods to treat the perturbations due to solar gravity on the motion of the moon around the earth, and the effects of the interactions between the planets rotating around the sun. Although he succeeds in many cases, he failed, in spite of considerable efforts, to account for the precession of the apsides of the moon orbit by a factor of two. This failure became the first major challenge for the early mathematicians and astronomers who read the *Principia*.

When European mathematicians like Bernoulli, Hermann, Varignon, Huygens, Leibniz and others first read the *Principia*, however, they had considerable difficulties understanding Newton's novel mathematical ideas which combined geometrical quantities with his concept of limits, the fundamental basis of his calculus. These mathematicians had been introduced to a similar calculus by Leibniz, and they had to translate first Newton's mathematical language into their own before they could understand it and make further progress. Thus, Hermann, for example, applied the result of Newton's derivation of Kepler's area law to provided a solution of for the elliptical orbit of a body acted on by a central inverse square force in terms of the solution of what we call a differential equation. Likewise Bernoulli, who criticized the incompleteness of Hermann's solution, also derived the elliptic orbit by integrating Newton's expression in Prop. 41 Book 1 of the *Principia* for the case of an inverse square force. Remarkably, Newton had left out this important solution from the *Principia* and this has caused considerable confusion which remains to the present time. Some British mathematicians like Gregory were able to contact Newton, and get help from him to overcome obstacles in understanding the *Principia*, but this appears not to have been the case with European mathematicians. Bernoulli, for example, criticized the incompleteness of the fundamental Cor. 1 of Prop. 13 Book 1, were Newton claimed to have a proof of the solution of the inverse problem: given the gravitational force to show that the resulting orbit is a conic section. He also communicated to Newton an error he had found in Prop. 10 of Book 2. In both cases Newton made corrections in the next edition of the *Principia* (1713) without acknowledging Bernoulli's important contributions.

Relations with the Bernoulli and his school were further aggravated when the priority dispute on the invention of the calculus between Newton and Leibniz erupted in 1711. By the 1740s, serious reservations arouse regarding the general validity of the inverse square law for gravitational force because of the failure of Newton's approximation of the solar perturbation to account for the rate of precession of the lunar apside. The first to question on this ground the validity of this law was one of the foremost mathematicians in Europe, Leonhard Euler. He remarked that 'having first supposed that the force acting on the Moon from both the Earth and the Sun are perfectly proportional reciprocal to the squares of the distances, I have always found the motion of the apogee to be almost two times slower than the observations ...', and concluded that 'the centripetal forces in the heavens do not follow exactly the law established by Newton'. The French mathematician Alexis-Claude Clairaut proposed that an additional force which varied with distance inversely as the fourth power, possible due to Cartesian vortices, was also in effect. Ultimately the French Academy of Sciences proposed a prize for the solution of this problem, and both D'Alembert and Clairaut solve it around 1746 by considering higher order contributions to the solar perturbation.

The importance of this result can hardly be overestimated. In admiration Euler declared in a letter to Clairaut that '...the more I consider this happily discovery, the more important it seems to me. For it is very certain that it is only since this discovery that one can regard the law of attraction reciprocally proportional to the squares of the distance as solidly established, and on this depends the entire theory of astronomy'. For the next 100 years mathematicians worked primarily in developing more powerful methods to solve the many body problems discussed in Newton's *Principia*. Lagrange and Euler introduced the method of variation of orbital parameters which was applied successfully to many astronomical problems. Originally this method had been developed by Newton, but it was left mostly unpublished (see Portsmouth papers in Whiteside's Newton's *Mathematical Papers*, vol 6, edited by D. T. Whiteside) except for some succinct remarks in Cor. 3 and 4, Prop 17 Book 1. By the late 1700s Lagrange and Laplace had written major treatises on Celestial Mechanics summarizing the mathematical progress that had been made. Questioned by Napoleon where God entered in his celestial scheme, Laplace famously responded that ' I do not need this hypothesis'. Newton had claimed that from time to time, God needed to interfere in order to maintain the stability of the solar system, but Laplace claimed that he had been able to give a proof of this stability mathematically. Later, however, this proof was shown to be flawed by the work of Henry Poincare.

Lunar Tables

N. Kollerstrom

In the early decades of the 18th century, Isaac Newton's recipe for obtaining the longitude of the moon came to be adopted by various European Ephemeris–makers. It soon became evident that these were more accurate than the earlier procedures. They flourished until the continental theories of Mayer and Euler developed, which were superior. Was Newton's lunar theory deduced from his theory of gravity? That was always hard to answer. He first published the theory in 1702, and then in 1713 with the 2nd Edition of his *Principia* claimed that its equations could be derived from gravity theory. It took a while before anyone tried to use it, because it was so much more complicated than other methods, having many more equations.

Newton obtained his lunar theory from Flamsteed, who had obtained it from north-country astronomers around the young Jeremiah Horrocks. This theory was Keplerian, that is, using elliptical motion, whereas the Moon as pulled more strongly by the Sun than by the Earth does not adhere at all well to a Kepler-ellipse model. The centre of the lunar ellipse was placed on an epicycle, and the upshot was that both its eccentricity and its apse line position oscillated approximately once per six months. This major oscillation replaced what had been called the 'evection' and to it Newton added various other equations. Altogether he devised a seven-step procedure, as was much used in lunar tables of the eighteenth-century.

In Paris, Pierre Lemonnier produced some fine tables in 1743, using this theory. Edmond Halley played apart in getting the Newtonian theories transmitted to the Continent. The great theorists such as d'Alembert were always sceptical as to whether gravity theory had played any part in deriving this theory, even while they had to concede that it worked fairly well. They just could not believe that epicycle-wheels, such as Newton's *Principia* had displayed in its 2nd edition lunar section (Book III, Prop. 37) belonged to gravity theory. They were able to demonstrate analytically, that is, algebraically that which Newton had expressed in a geometric format. There was agreement that the inequality known as the variation (discovered by Tycho Brahe) had been well derived by Newton from gravity theory, but of other components people were less confident.

Newton in 1687 deduced the relative mass of the Moon as $\frac{1}{20}$ that of the Earth whereas it is actually $\frac{1}{81}$, that is, he made it enormously too heavy. This was somewhat corrected in the 2nd edition of 1713 to $\frac{1}{40}$, where he reduced his error to merely a 100 percent overestimate of the lunar mass. As the largest error in the *Principia* this had some major implications for its Book III, for example in his Precession of the Equinoxes computation. The error derived from his attempt to estimate the tidal-pull ratio of Sun and Moon, which he obtained as 1 to 4.5, with the Moon pulling more strongly. Daniel Bernoulli in 1740 estimated this ratio as more like 1 to 2.5 which is much nearer the modern value. Thereby d'Alembert was able to obtain a much more exact estimate of the Earth to Moon mass ratio.

Newton's instructions for finding lunar longitude were written out in words, that is, he was not able to or never did express them as trigonometric equations, as did d'Alembert and Clairaut. Memory of the existence of any Newtonian recipe for finding longitude soon disappeared from the history books, maybe because the Horroxian model he had used was kinematic and too evidently not derivable from his inverse-square law of gravity, not reappearing until the 1975 essay by D. T. Whiteside. In the 1750s lunar theories were derived on the Continent, exact enough to win Britain's longitude prize (they were exact to one arc minute of lunar longitude). The lunar tables of Edmond Halley published in 1749 were the last of the 'Newtonian' ones and these soon became out of date.

The Influence of Newton's *Principia* on the Development of Continuum Mechanics and Rigid Body Dynamics on the Continent

G. K. Mikhailov

Newton's *Principia* does not touch practically Continuum Mechanics in its modern sense, but includes some investigations in the hydraulic theory of fluid motion that can be considered as related to Continuum Mechanics. In the second book of the first edition of *Principia* (1687) Newton gave basic ideas of the similarity theory, developed corpuscular models of hydroaerodynamic drag suffered by bodies in rare and dense fluids, the first (wrong) approach to a hydraulic theory of water efflux from vessels (with a corresponding jet reaction force), the first theory of sound and water wave propagation and water oscillations in vertical tubes, proposed a continuous model for a viscous fluid. The third book of *Principia* includes also his theory of tides and notion of the spheroidal form of liquid celestial bodies (the Moon).

In the second edition (1713) Newton improved his theory of water efflux and jet reaction, introduced the notion of jet contraction and slightly changed the analysis of the sound propagation.

A sharp discussion on Newton's theory of water efflux from vessels occurred on the Continent during the first half of the 18th century (especially by Johann I and Daniel Bernoulli). Johann Bernoulli and Daniel in his early work (1724) supported the first (wrong) variant of Newton's theory. However, Daniel developed later a thorough system of theoretical hydraulics on the basis of the principle of living forces (1738) and confirmed Newton's improved theory of jet reaction under water efflux from vessels. Investigations on the jet contraction were advanced during the 18th century, mainly in Italy and later also in France. Newton's well-known error in solving the problem on rotation of a viscous fluid was corrected by D. Bernoulli (and repeatedly by G. G. Stokes a century later).

Newton based his theory of sound on a vague idea concerning propagation of pulse in an elastic medium and for the first time applied here the momentum principle to an infinitesimal element of a continuous medium. However, Newton expounded his theory in such an obscure way that Cotes did not understand it and even interchanged two fundamental Propositions (47 and 48) in the second edition of *Principia* (Only recently, in 1981, Cannon and Dostrovsky showed that this change was wrong.) Nevertheless, Newton obtained correctly a formula for the sound velocity corresponding to the wrong assumption of isothermal sound propagation (the adiabatic process of the sound propagation was discovered by Laplace only a century later).

Systematic application of Newton's law of dynamics written in fixed Cartesian coordinates increased the possibility of solving various problems of mechanics. We find such formulations of the laws of motion in some papers from the end of the 1730s. However, it was probably Maclaurin who first evaluated the importance of such a use of Newton's law, announcing it as a general *principle* (*Treatise of fluxions*, Book I, §466– §469, printed in 1737, but published first in 1742). The next fundamental step toward the foundation of continuum mechanics was made by Euler who proposed to apply Newton's law of dynamics written in fixed Cartesian coordinates directly to an infinitesimal element of a continuous (fluid or rigid) body. It was his famous 'Nouveau principe de mécanique' (*Mémoires de Berlin*, 1750–52) that allowed him to construct the modern hydrodynamics of an ideal fluid (*Mémoires de Berlin*, 1755–57). It is interesting that an English magazine published an abstract of Euler's memoir under the title 'On the general and fundamental principle of all mechanics', whereon all other principles relative to the motion of solids or fluids should be established, and called it a grand principle.

Using his *principe nouveau* and Segner's detection of three principal axes of rotation of rigid bodies, Euler established also the dynamics of rigid bodies (*Theoria motus corporum solidorum seu rigidorum*, 1765). In one of the following papers ('Nova methodus motum corporum rigidorum determinandi', *Novi commentarii*

Acad. sci. imp. Petrop., 1775–76) Euler formulated a system of six equations determining the motion of any body, that covers both the principles of linear momentum and of moment of momentum, and thus completed the construction of general equations of dynamics. Truesdell called these equations Euler's fundamental laws of mechanics.

Newton's 'Axiomata sive leges motus': Some General Remarks on their Reception and Development in 18th Century's Mathematical Physics and Philosophy

H. Pulte

From J. L. Lagrange and J. E. Montucla to E. Mach and even to T. S. Kuhn it has been a commonplace in the history of science that Newton inaugurated a 'revolution' not only in celestial mechanics (especially by his theory of gravitation), but also in rational mechanics in general (that is, with respect to the principles of mechanics). Newton, however, did not claim that his *axiomata sive leges motus*, either separately or in conjunction, were really new. (Indeed, he summed up his discussion of these principles with the comment: 'Hactenus *Principia* tradidi a mathematicis recepta & experientia multiplici confirmata'.) But he did emphasize their axiomatic status, that is, he wanted to make clear that his natural philosophy–in contrast to the 'hypothetical philosophy' of Descartes, Leibniz and others–was built upon true and unshakeable mathematical principles. While Newton's 'classical image' of science ('Euclideanism', as Lakatos called it) was generally shared by scientists and philosophers throughout the 18th century, the 'Newtonian' axiomatization of mechanics (as it is labelled nowadays) was by no means understood as*Newton's* achievement within the continental reception: In general, Newton's first law was not attributed to Newton at all, but to Galilei, Descartes, Huygens or others. The third law was also not understood as a genuine Newtonian one, and we know today that earlier formulations can be found in the works of Marci, Hobbes, Digby, White and others.

Therefore, the second law deserves special attention: either this law was understood as original, or the special combination of the three laws, or the (modern) claim that Newton axiomatized rational mechanics was unfamiliar to the continental reception.

An examination of the relevant sources both on the mathematical principles of rational mechanics and on their philosophical interpretation reveals that the third alternative applies to most of the scientists and philosophers who were interested in the ongoing foundational debate about rational mechanics. Continental mathematicians usually traced back the second law to Galilei, and later to Varignon or Hermann. One of the main obstacles to accept the second law as a universal principle of mechanics was Newton's own doctrine of (perfectly) hard bodies in nature. This doctrine excluded the application of force as the rate of change of momentum to the collision of hard bodies (because this would imply infinite forces), and it had to be conquered before the modern form of 'Newton's second law' could gain the status of a general principle of mechanics.

It was not before 1750, when Euler published his 'Découverte d'un nouveau principe de mécanique', that the so-called second law of Newton was introduced as a 'unique fundament' (Euler) in this sense, expressed with second derivatives and in Cartesian coordinates. Euler, however, never accepted Newton's concept of a directive force as a primary one. Throughout his life, he tried to base a quasi-Newtonian mathematical theory of mechanics on a (by and large) Cartesian theory of matter: It was one of the leading principles of his scientific metaphysics that matter is basically passive, and that all changes in nature have to be explained by 'matter and motion' alone, that is, without introducing Leibnizean or Newtonian forces. In Euler's case, this restriction is based on ontological arguments, in other cases (like Maupertuis', or d'Alembert's, for example) the concept of force is rejected on epistemological grounds.

In general, the continental reception of Newton's *Principia* within mathematical physics up to Lagrange shows no inclination to stress originality with respect to the principles of mechanics. An examination of German academic philosophy, as far as it was interested in the foundations of mechanics, reveals a similar picture: Newton's second law plays no role in the philosophical discussions on the foundations of mechanics, and the *Principia* is not understood as an important contribution to this discussion. This holds true (and is not very surprising) for Leibniz's adherent and transformer Wolff, but also for philosophers like Bilfinger, Reusch, Thimmig and others as well as the physics textbooks inspired by them: Newton's three laws were not perceived as an original or even final axiomatization of mechanics in general. Kant, in his *Metaphysische Anfangsgründe der Naturwissenschaft* (1786), perpetuates this view, though this work is frequently described as an attempt to give a metaphysical foundation of Newton's *Principia*: The *axiomata sive leges motus* are not presented as a 'synthetical' basis of the science of motion, and the second law is neither 'deduced' philosophically nor mentioned at all. It is the Kantian Fries who, in his work *Die mathematische Naturphilosophie nach philosophischer Methode bearbeitet* (1822) for the first time tried to give a philosophical foundation of mechanics that did justice to Newton's original foundation of the principles of mechanics.

To sum up: Though Newton's *Principia* was obviously most successful as a textbook on rational mechanics in an empirical respect, it was neither perceived during the 18th century as a unique achievement with respect to its foundational claims, nor were the principles formulated in the *Principia* understood as sufficient to solve all problems of mechanics. That the *Principia* inaugurated a 'revolution' of rational mechanics is a legend which came about at the end of the century with Lagrange and Montucla and was perpetuated in the later textbooks of the Laplace-Poisson tradition. From here it seeped into philosophy of science and also into history of science, and Kuhn accepted it with gratitude as an outstanding example of his own historiography. This, however, does not improve the legend. Truesdell, Hankins, Grattan-Guinness and others have shown during recent decades, that the dichotomy of 'revolutionary science' and 'normal science' is at best misleading: with respect to the foundations of mechanics, the 18th century was not 'normal', because the 17th century (including Newton's *Principia*) was not 'revolutionary'.

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The Reception of Newton's Theory of Light and Colour

A. E. Shapiro

The story of the reception of Newton's theory of light and colour is a long and complex one. It can be divided into roughly four periods, assimilation from 1672 to 1704, acceptance from 1704 through the 1720s, dominance for the next 100 years, and post-wave theory from about 1830. Here I will focus on the first two periods.

The period of assimilation, 1672–1704, focused exclusively on Newton's initial publication, 'A new theory about light and colours', in the *Philosophical Transactions* in 1672 and the extensive correspondence concerning the theory published there through 1676. Three issues concerned Newton's critics, almost all of whom were in France: (1) the nature of colour, in particular, the nature of whiteness, primary or simple colours, and colour mixing; (2) unequal irrefrangibility; and (3) the nature of light, that is, whether it consists of emitted corpuscles, as Newton evidently believed, or a disturbance in a medium. Newton always insisted that one must distinguish between his scientific theories and hypotheses, such as his support of the emission theory of light. It was only in the acceptance phase immediately after the publication of the *Opticks* that the nature of light was not an issue, and Newton's distinction was accepted. Afterwards, it gradually collapsed. The French Jesuit Ignace Gaston Pardies and Christiaan Huygens in 1672 and 1673 were concerned with all these issues. Huygens never accepted Newton's idea that colours were innate to sunlight, but he quickly accepted the idea that it consisted of rays of different refrangibility and even helped to propagate that idea. During the course of this period the concept of unequal refrangibility was gradually accepted, especially among mathematical scientists and the Scottish. Leibniz was initially opposed to Newton's theory, but he soon inclined to accepting it. He was troubled by Edme Mariotte's experiment that showed that Newton's primary colours were not immutable as he had claimed (1681). The initial diffusion of Newton's theory on the Continent was hindered by its publication in English.

The period of acceptance centered on the *Opticks* (1704) and its Latin translation, *Optice* (1706), and papers published by John T. Desaguliers in collaboration with Newton in the *Philosophical Transactions* in 1717 and 1722. The theory was rapidly accepted in Italy: Giovanni Poleni tested and taught it in 1707, and in 1707–08 Celestino Galiani and Francesco Bianchini confirmed it. Acceptance in Germany was almost as quick: Leibniz in 1704 provisionally accepted the theory until Mariotte's experiment was tested. Johann Bernoulli confirmed and supported it by 1710, and in that same year Christian Wolff endorsed it in his textbook. A crucial event in the acceptance of Newton's theory was Christian Wolff's anonymous call in 1714 in *Acta eruditorum* for Newton to respond to Mariotte's experiment. In 1714 Desaguliers, under Newton's direction, demonstrated that a ray of a single colour remains unchanged after a second refraction, thereby refuting Mariotte's claim. In 1716 Desagulier's experiment was published in *Philosophical Transactions* together with an unsigned introduction by Newton. In 1717 Wolff announced Desagulier's confirmation in *Acta eruditorum* thereby making the result widely known.

Acceptance came more slowly in France because of the high regard for Mariotte's experimental work. In 1712 Nicolas Malebranche adopted Newton's theory of colour in his *Recherche de la vérité*. In 1715 a delegation from the *Académie des Sciences* witnessed Desagulier's experiment, and in 1716 and 1717 Jean-Jacques Dortous de Mairan published a confirmation of Newton's theory. The acceptance of Newton's theory in France can be marked by the publication of a French translation *Traité d'Optique* in 1720 and 1722.

Newton's theory was also quickly accepted in the Netherlands. In 1708 Herman Boerhaave, the influential Professor of medicine at the University of Leyden, endorsed it. However, the most influential Dutchmen in propagating Newton's theories were Willem Jacob 'sGravesande and Petrus van Musschenbroek who, in a sequence of widely read and translated textbooks from the 1720s to the 1760s, advocated an experimental, Newtonian natural philosophy.

The widespread acceptance of Newton's theory should not obscure the persistent opposition to it. The case of Giovanni Rizzetti shows how after the early 1720s such opponents were isolated from the mainstream. Rizzetti criticized Newton's theory and experiments in a paper in 1721 and a book in 1727. Desaguliers responded to them in the *Philosophical Transactions* in 1722 and 1728. Rejection of Rizzetti's work was widespread: Georg Friedrich Richter in Leipzig in 1724, Nicolas Gauger in Paris in 1728, and Francesco Maria Zanotti and Francesco Algarotti in Bologna in 1729. The nature of this widespread opposition not only to Newton's theory but to Newtonian natural philosophy must be investigated; it included the French Jesuit Louis-Bertrand Castel (1735), Jean-Paul Marat (1779) and Goethe (1791).

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The Newtonian Optics of Moving Bodies

J. Eisenstaedt

Since the time of Galileo, the relativity of motion is a central issue in physics. But how does it apply to light? From Bradley's account of aberration in the first part of the 18th century, the velocity of light has been observed to be independent of the velocity of the source and, later on, of that of the observer. As well the usual interpretation of Newton's dynamics supposes that the velocity of light is constant, a result which implies that light would be neither subject to Galileos's kinematics nor to Newton's dynamics. Thus the usual status of light is incoherent with Newton's theory which must then be completed.

At the end of the 18th century, a natural extension of Newton's dynamics to light was developed but immediately forgotten. A body of works completed the *Principia* with a relativistic optics of moving bodies, the discovery of the Doppler-Fizeau effect some sixty years before Doppler, and many other effects and ideas which represent a fascinating preamble to Einstein relativities.

It was simply supposed that 'a body-light', as Newton named it, was subject to the whole dynamics of the *Principia* in much the same way as were material particles; thus it was subject to the Galilean relativity and its velocity was supposed to be variable. Of course it was subject to the short range 'refringent' force of the corpuscular theory of light —which is part of the *Principia*— but also to the long range force of gravitation which induces Newton's theory of gravitation. The fact that the 'mass' of a corpuscle of light was not known did not constitute a problem since it does not appear in the Newtonian (or Einsteinian) equations of motion.

It was precisely what John Michell (1724–1793), Robert Blair (1748–1828), Johann G. von Soldner (1776–1833) and François Arago (1786–1853) were to do at the end of the 18th century and the beginning the 19th century in the context of Newton's dynamics. Actually this 'completed' Newtonian theory of light and material corpuscle seems to have been implicitly accepted at the time. In such a Newtonian context, not only Soldner's calculation of the deviation of light in a gravitational field was understood, but also dark bodies (cousins of black holes). A natural (Galilean and thus relativistic) optics of moving bodies was also developed which easily explained aberration and implied as well the essence of what we call today the Doppler effect. Moreover, at the same time the structure of — but also the questions raised by— the Michelson experiment was understood.

Most of this corpus has long been forgotten. The Michell-Blair-Arago effect, prior to Doppler's effect, is entirely unknown to physicists and historians. As to the influence of gravitation on light, the story was very superficially known but had never been studied in any detail. Moreover, the existence of a theory dealing with light, relativity and gravitation, embedded in Newton's *Principia* was completely ignored by physicists and by historians as well. But it was a simple and natural way to deal with the question of light, relativity (and gravitation) in a Newtonian context.

Einstein himself did not know of this Newtonian theory of light and he did not rely on it in his own research. But he was not very far from these ideas: he wrote to Freundlich in 1913 that 'it was rather natural that the idea of a bending of light appeared at the time of the theory of emission'. But it was not his way to his relativity. This theory will not bring us to the 'Einsteinian relativities' but it is still a most interesting and simple approach to it. It brings us to several qualitative effects that have been obtained —and verified— in the context of Einstein's theories of relativity whose context of discovery is anyway essentially the evolution of electrodynamics in the 18th century.

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The Persistence of Mechanical Imagery: 'Newtonian' Ideas about Electricity and Magnetism

R. W. Home

Newton never wrote a systematic treatise on either electricity or magnetism, but it is clear from both the occasional references to these subjects in his published works and the much more extensive references to them in his unpublished manuscripts that he had a life-long active interest in them both. The unpublished manuscripts make it clear that his understanding of both categories of phenomena much more closely resembled that of seventeenth-century mechanical philosophers than it did that which modern discussions of eighteenth-century 'Newtonianism' would lead us to expect. For most if not all of his life, Newton assumed that the spaces between the particles of which ordinary bodies were composed was filled by a subtle elastic fluid analogous in some ways to the pneuma of the Stoics. He saw 'electricity'—that is, the attraction that substances such as glass or amber exert, after they have been rubbed, on nearby light objects, which was the only known electrical phenomenon until the later years of Newton's life—as a consequence of the subtle matter within the rubbed body being agitated by the rubbing and driven out into the space surrounding the body. Any light objects that this matter encountered would be swept along as the agitation weakened and the subtle matter collapsed back into the body from which it had originated. Newton's most extensive discussions date from the first years of the eighteenth century and were prompted by some dramatic experiments devised by Francis Hauksbee that considerably expanded the range of known electrical phenomena. In Newton's opinion, Hauksbee's experiments demonstrated the existence and activity of the subtle matter, not only in bringing about the attraction but in the production of light and perhaps also in several other important categories of natural phenomena. So far as magnetism was concerned, Newton followed Descartes in ascribing the attractions and repulsions exerted by magnets on each other or on pieces of iron, and also the directive power whereby a freely suspended magnet orientated itself in relation to other nearby magnets, to circulating streams of a peculiar subtle matter that passed axially through a magnet from one pole to the other and then returned through the external air.

Though Newton's published references to electricity and magnetism were brief and scattered, they would have been enough to reveal to his eighteenth-century audience that his views on these subjects were as just described and so were very similar to those generally accepted at the time. Hence their impact would have been limited to perhaps reinforcing, through the intellectual authority Newton wielded, a style of thinking that we do not generally associate with him.

At a more general level, Newton's methodological prescriptions did have a discernable effect. Faithful to Newton's dicta, various eighteenth-century physicists tried to discover the laws according to which the forces of electricity and magnetism acted. Unfortunately, nature proved unkind, and not until late in the century, in the work of Coulomb, did they achieve success. Earlier attempts to determine the law in the magnetic case, most notably the definitive efforts of the Dutch physicist, Musschenbroek, foundered through taking gravity too literally as an analogy and trying as a result to measure the force exerted by one whole magnet on another, rather than (as Coulomb eventually did) the force between two isolated magnetic poles. Musschenbroek from his measurements could only conclude that there was no general law covering the action of one magnet on another! In the electrical case, all efforts to fix a law were beset by problems resulting from leakage of charge. There was also much uncertainty over the physical meaning of degree of electrification and over the conditions that needed to be reproduced in order to obtain comparable measurements. Until the 1750s or even later, most people continued to attribute both electricity and magnetism to flowing streams of subtle matter, in the way that Newton had done. Meanwhile, a range of striking new phenomena associated with electricity, including its transmissibility along conducting lines and, most dramatically, the powerful shocks delivered by the Leyden jar, cried out for explanation. Following the work of Benjamin Franklin, Aepinus developed a theory of electricity that much more closely followed Newtonian methodology than did Newton's own ideas about electricity. Aepinus's theory retained the idea that ordinary matter was pervaded by a subtle matter responsible for the phenomena of electricity, but assumed that this was composed of particles that exerted forces on each other and on particles of ordinary matter at a distance, while the latter were also assumed to exert forces on each other. In order to render the theory consistent with the commonest of phenomena, Aepinus had to assume that the force between particles of ordinary matter was a repulsion rather than an attraction. He made no attempt to explain any of the forces he invoked, or any assumptions about the laws according to which they varied with distance, he simply added up the forces acting between static accumulations of fluid in a variety of situations. By doing so, he managed to provide coherent, semi-mathematical explanations for all the principal phenomena of eighteenth-century electrical science.

In his great treatise published in 1759, Aepinus went on to develop a theory of magnetism analogous to his theory of electricity. In this case, too, he was able to account in a coherent manner for all the principal known phenomena. Once again, the theory assumed various forces acting at a distance, this time involving particles of a second subtle fluid that he supposed pervaded iron and other magnetic bodies. Once again, Aepinus offered no explanation for these forces. Instead, he quoted Newton's methodological dictum about first discovering the forces that acted in the world, before worrying about the causes of those forces.

Aepinus's work initially had little influence, but in the 1780s his ideas were taken up by Coulomb, Volta and others, and quickly became the basis for mathematically formulated sciences of electricity and magnetism analogous in many ways to Newton's account of the planetary motions. In the process, Newton's own, very different ideas about electricity and magnetism were left far behind.

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The Reception of Newton's Views of Space and Time

R. DiSalle

Newton saw the physical world as unfolding against the background of 'absolute space' and 'absolute time', and he held that the laws of motion could determine the 'absolute motions' of bodies against this background. Most of his greatest scientific contemporaries, on a number of scientific and philosophical grounds, held that motion, space, and time are essentially relative. Yet Newton's view eventually succeeded over their objections, and was never seriously threatened again until physics itself underwent a fundamental change, with the work of Einstein. One reason was, undoubtedly, the empirical success of universal gravitation, and the increasing plausibility, in consequence, of the notion that space is nearly empty of matter—not, in particular, filled by the fluid vortex by which Newton's opponents had hoped to explain planetary motion. An equally important reason, I suggest, was the growing recognition that the relativistic view was not compatible with the basic principles of mechanics, as then understood. Newton's most influential defenders, including Leonhard Euler, emphasized and developed a point urged by Newton himself, namely, that concepts of inertia and force employed in mechanics presuppose certain conceptions of space and time. This was a view which, perhaps surprisingly, transcended serious differences with Newton over other prominent metaphysical questions, such as the possibility of a vacuum or the plausibility of action at a distance. Philosophical objections to the reality of space and time faded from prominence until the late 19th century, when they arose again as part of a general epistemological critique of the Newtonian laws of motion.

A major part of the history of the reception of Newton's ideas, then, is the increasing predominance, at least in scientific literature, of physical considerations over the philosophical considerations that had originally motivated the relativist view. But another part concerns the connection of the theory of space and time with broader metaphysical issues, including the epistemological foundations of geometry, the origins and structure of the physical universe, and the place of theological ideas in the scientific understanding of space. Newton's ideas had particular implications concerning God's presence in physical space, and these had an obvious and significant impact on continental discussions that went well beyond the controversy over absolute and relative motion, and reached an audience well beyond those with a technical understanding of Newton's physics.

Leibniz and Newton

H. Breger

Mathematics: In 1676 and 1677 there was an exchange of letters between Leibniz and Newton on mathematics. This correspondence became famous later during the priority dispute. Newton as well as Leibniz communicated only results (most of which had already been known before); both retained their methods. Newton probably failed to understand that there was a unified method behind Leibniz's particular results. Mistakes in copying the letters and delay in their communication led to suspicions later in the priority dispute. As for the question whether Newton influenced Leibniz concerning the foundation of the calculus, the point has to be made that there was no foundational problem in Leibniz's analysis: We have to take into account that the meaning of analysis changed since the 17th century. As far as I know Leibniz did never talk about an infinitesimal synthesis; his claim that all results could be proved with the Archimedean method is true. In fact there was not really a conceptual opposition between Leibniz's and Newton's version of the calculus; it was pretty easy to translate one into another. Newton did not mind that his calculus uses a notion of physics in mathematics. Leibniz as a philosopher with a strong interest in a *characteristica universalis* aimed from the very beginning at a higher level of abstraction; this was the deeper reason for his development of an appropriate notation.

Physics: Leibniz's reception of the *Principia* is the main issue here. According to Leibniz, he read a review of Newton's *Principia*, when he was on his journey through South Germany, Austria and Italy. He immediately reacted with three articles in the journal *Acta Eruditorum*. Only some months later, when he was in Rome, he saw Newton's book. This claim made by Leibniz has been drawn into doubt in 1993. In one of the three articles, Leibniz developed a theory of planetary motion based on the idea of a vortex surrounding the sun. No doubt, Leibniz had received the stimulus for his theory by Newton's *Principia*. Given the achievements of Galileo (on uniformly accelerated motion) and Huygens (on centrifugal force) and taking into account that the inverse-square law had already been quoted in the review, Leibniz's achievement was to show that the vortex theory was still a possible explanation, although there were some difficulties.

Newton's and Leibniz's correspondence: The direct correspondence between Newton and Leibniz was continued in 1693; in these letters, there was no real controversy and no substantial exchange, but rather mutual politeness and esteem. In 1692, Leibniz stated in one of his letters that Newton was one of the first geniuses of the century. Throughout Leibniz's correspondence there is quite a high esteem for Newton. The atmosphere changed only at the end of Leibniz's life, when he heard about the composition of the *Commercium epistolicum*. During that period Leibniz as well as Newton said some things which were not fair.

The Leibniz-Clarke debate: This debate started in November 1715 and ended only with Leibniz's death in November 1716. Clarke discussed his letters with Newton; so it is in fact a Newton-Leibniz correspondence in disguise. The correspondence was several times translated and reprinted during the 18th century; in that period the correspondence provided a kind of philosophical foundation for the priority dispute. Leibniz defended a theory of relational (not relative) time and relational space as well as the relativity of motion against Newton's theory of absolute time and absolute space and absolute rotational motion. The bucket experiment did not play a role in this debate. Clarke argued for repeated intervention by God in the course of nature in order to restore motion which had been destroyed previously by friction, whereas Leibniz argued for the conservation of what we call kinetic energy. This gave rise to a discussion on miracles and natural laws; Leibniz argued for the principle of sufficient reason, whereas Clarke claimed that God as well as human beings could change the course of nature by free decisions. Finally it was discussed whether there is a mechanical explanation for gravitation or not.

The Bernoullis

D. O'Mathuna
(joint work with D. Speiser)

The part of the Bernoulli story of concern to us covers a period lasting over a century (1672–1782). It begins in 1672 when Jacob I made his commitment to investigations in Mathematics and Mechanics and ends with the death of Daniel (Jacob's nephew) in 1782—almost a century after the appearance of the *Principia*. In this survey our attention will be focused on works of Johann I, Nicolaus

I, Daniel and Jacob Hermann, a disciple of Jacob I, whose work, especially that on Newtonian mechanics, cannot be separated from that of the Bernoullis. The decision to exclude discussion of the Newton—Leibniz controversy, in which Johann I played such a prominent role, will be adhered to.

Though there is no evidence of any Newtonian influence in his work, we must start the story with Jacob I (1654–1705). Over the last twenty years of is life he was the pioneer in the development of Leibniz's ideas both in Calculus and in Mechanics. His proudest achievement in calculus was what he termed his 'Theorema aureum', the curvature formula, while in mechanics, his main work was his analysis of the elastic beam, involving a general form of the constitutive relation and an independent derivation of the angular momentum law. But it is through his students, Johann I, his much younger brother whom he early initiated into Leibnizian Calculus and his favored student, Jacob Hermann, that interaction with Newtonian ideas is to be sought.

It would be misleading to think of Johann I (1667–1748) solely as an adversary of Newton. Perhaps the most significant extension of Newtonian mechanics in the first half of the eighteenth century was the introduction and application by Johann I of the Newtonian Law at the local level in the treatment of fluid Mechanics. The significance of this development in Johann's basic *Hydraulica* was recognized at once by Euler (his former student)—and in our time was rediscovered by Truesdell. In spite of his many contributions in the field, Johann remained uncomfortable with the concept of gravitation namely, action at a distance without a plausible mechanism of transmission. That Newton was also uncomfortable with the idea was something Johann appears to have been unaware of. Another landmark in his Newtonian work will be noted below.

Closer to Newton in many respects was Nicolaus I (1687–1759) a nephew of both Jacob and Johann, who also learned his mathematics from Johann. In his reading of the *Principia* he noted an error in the solution calculated by Newton for one of the problems he had posed. On a visit to London in 1712 he communicated with Newton to draw his attention to this. Newton immediately acknowledged the error and incorporated the correction in the second (1713) edition of the *Principia.* Newton remained on cordial terms with Nicolaus I and had him elected a Fellow of the Royal Society. An invitation to the honor of FRS was also extended to Johann.

With Jacobs favorite student, Jacob Hermann (1678–1733) two roadmarks need to be highlighted. In 1716 he had published his book on mechanics titled *Phoronomia.* This book, which is also considered the first textbook in mechanics, shows heavy influence from Newton and the *Principia.* It is divided into two parts, the first devoted to the mechanics of solid bodies and the second to that of fluids, where there are many direct references to the *Principia.* Prior to that, in 1710, Hermann had published the first recorded analytic approach to the Kepler problem. His analysis was to integrate directly the second order differential equation expressing Newton's Law of Motion. The analysis was perfectly valid but deficient in that he had omitted the first constant of integration. This was immediately rectified in the response from Johann Bernoulli, who then went on to perform an analysis of the problem in its full generality. Hermann's paper would appear to be the first systematic integration of a second order differential equation of motion, thereby proving that under Newtons gravitational law the orbits are conic sections. Whether Newton had actually 'proved' this, is still being argued. What is clear is that Newtons derivation was based on geometrical considerations rather than on an integration of a differential equation.

In the general analysis of Johann Bernoulli, he derived what we now call the first integrals of energy and angular momentum, and introduced the new independent variable, later to be known as the true anomaly, reflecting the angle subtended by the planet at the focus of the orbit, with the baseline pointing to perihelion. The solution is expressed in terms of trigonometric functions of the true anomaly — the form in use ever since. His approach may be considered the beginning of analytical mechanics.

With Daniel Bernoulli (1700–1782) we may consider the year 1728 as a turning point. In his early work prior to that, there is what may be considered a certain aloofness in respect to Newton. The first item of Daniel wherein there is reference to Newton, is the paper dealing with the parallelogram law for the composition of forces. However the question dealt with, is an axiomatic one with no analytic content. Following the publication of his treatise *Exercitationes ...* in Venice in 1724, Daniel with his brother, Nicolaus II, arrived in St. Petersburg in 1725 to join the Academy. In 1727 Euler arrived. At a meeting of the Academy in March 1728 Daniel responded to a discourse with De Lisle on a basic question in astronomy. In the course of that discussion he shows that for him the treatment of such issues is primarily a mechanical problem based on Newtons Laws—of gravitation and of mechanics. He goes further to say, *inter alia*: '...mais nous en devons l'entier developement à Mr. Newton, cet illustre Anglois, qui toutes les nations nomment l'ornament de notre siècle...'. When taken with his prior aloofness, this enconium to Newton can be considered as a 'conversion' which can safely be attributed to Euler.

This incident preceded Daniel's two very productive decades when he was in constant interaction with Euler. In his book *Hydrodynamica*, as well as in his prize winning Essays to the Paris Academy, the spirit of Newton is pervasive. His essay on tides (as well as those of Euler and Maclaurin) was included in the reissue of the *Principia* in Geneva in 1742. This reissue was done at the instigation of Prospero Lambertini then serving as Pope Benedict XIV. A striking example showing inspiration from Newton, was Daniel's conjecture of the inverse square law for electrostatics—later confirmed by Coulomb.

Thus the Newton-inspired work of Daniel Bernoulli belongs to the first phase of Newtonian influence between the publication of the *Principia* (1687) and the appearance of Euler's paper of 1750 where Newton's Second Law is stated, using Cartesian coordinates in a inertial frame as we know it today. This would also mark the shift of the center of Newtonian influence from Western Europe to St. Petersburg.

Newton in the *Encyclopédie*

K. Maglo

By the middle of the 18th century, the reception of Newton's science on the Continent had changed dramatically, particularly, in the work of mathematicians and experimentalists. The old generation that rejected Newton's theory of gravitation disappeared and a fresh generation of scientists emerged with new ideas including a synthesis of the by-then available knowledge about the world. D'Alembert, for one, was not only the editor of the mathematical part of the *Encyclopédie*, but he was also, together with Euler and Clairaut, a key player in the transformation of the science emerging in Newton's *Principia*. In his famous 'Discours Préliminaire' to the *Encyclopédie*, d'Alembert saluted Bacon, Descartes, Newton and Locke as the four 'main geniuses which the human mind should recognize as its masters and to which the Greeks would have certainly built statues, even if to achieve this, they would have had to demolish those of some conquerors.' On very specific issues, however, Newton was the *primus inter pares*. It was he, for example, who deepened the experimental philosophy, merely foreseen by Bacon and Descartes, and thus created, with his experiments both in optics and his mathematics-based explanation of 'the system of the world', a scientific field of immense potentialities for new generations of scientists (Article 'Experimental'). He was the destroyer of the Cartesian vortex account of gravity which in itself is 'one of the most beautiful and ingenious hypotheses ever imagined in science'. (Discours Préliminaire)

But there is no univocal description of Newton in the *Encyclopédie*, just as there is no monolithic view of Descartes. Various articles present different images of Newton according to the scope of his contribution, or lack thereof, to a specific subject. In some entries, Newton, the giant among the giants, is also the one who left his revolution incomplete, who made mistakes or systematically failed, for instance, in fluid mechanics. In others, he is the giant on the 'shoulders' of Huygens and Barrow who are said to have cleared the way for him, respectively, in the development of the theory of central forces and the theory of fluxions. The Encyclopedists seem to conceive of their project not only as a means of popularizing science but also as a way of intellectually transmitting a legacy to posterity. Despite their polemical tone and biases here and there, they appear to show concerns about fairness in contrasting Newton's achievement with that of his predecessors, contemporaries and successors: 'The Encyclopédie, conceived with the goal of communicating to posterity the history of the discoveries of our own century', declared d'Alembert in the article Lune, 'must for this very reason do justice to every one . . . '. Actually, the perceptions and interpretations of Newton's achievement in the Encyclopédie is very often mediated by the work of Newtonians such Whiston, Boerhaave, s'Gravesande, Pemberton, Cotes, Le Seur, and Jacquier, Maclaurin, Voltaire, Maupertuis and Mme du Chatelet. On key controversial issues, the Encyclopedists tend to distance themselves from 'Newtonians'.

In many instances, the Encyclopédie seems to have created for the new 'Société de Gens de Lettres' the opportunity to attempt to redefine Newtonianism. Thus it will be fruitful to reconstruct Newton's reception in the *Encyclopédie* by considering the five following points: 1) what was perceived as the core of Newton's doctrine and which determines the meanings of Newtonianism; 2) the loose ends and certain aspects of Newton's theories; 3) the points of contention in his teachings; 4) his errors, failures and limits; 5) the philosophy of theory-change brought about by his science. It is worth noticing, however, that the heterogeneous portrayals and occasional implicit and explicit criticisms of Newton in the *Encyclopédie* are far from overshadowing the great admiration for the man; even behind his mistakes and failures shines forth 'an ingenious and resourceful mind that no one ever before him has possessed to such a high degree' (D'Alembert, article 'Fluide').

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French Mathematics and Mathematical Physics, 1780–1830

I. Grattan-Guinness

1. *Context.* The French dominated mathematics from the early 1780s after the deaths of Euler, d'Alembert and Daniel Bernoulli. The revolution of 1789 helped the situation in that science and especially engineering institutions were re-formed and extended. They were joined by the new *École Polytechnique* (1794); while not a research school, many of the figures mentioned below taught and/or examined there, and most of the new generation were also students there. By the time under consideration here Newton's main works had been in print for nearly a century; so it is important to distinguish his active influence, positive or negative, upon French work from the presence of his ideas as uncontroversial parts of the pertaining furniture.

2. *The calculus.* The dominant theory was Leibniz's differential and integral calculus together with Euler's differential coefficient. Especially from 1790s onwards some attention was given to Lagrange's algebraic dependence upon Taylor series, from which the derived functions of a given function were defined from the coefficients. Newton's theory was known on the Continent, and the French were polite about limits; but (like Newton himself) they only used limits as a notion,

and his kinematic reading of them were usually ignored. Some authors used limits mixed in with other theories: for example Carnot (1796), Ampère (1806) and especially Lacroix (1797+) who as an encyclopédiste liked to present all available theories. Cauchy's new formulation of the calculus during the 1820s is founded upon a theory of limits, and does not seem to owe much to Newton.

3. *Algebra.* Books appeared quite regularly, including from principal textbook authors such as Bézout and Lacroix. Universal arithmetic was not adopted, but Newton's book was translated in 1802 by N. Beaudeux. Newton's innovations were noted, especially for the general binomial theorem, sums of powers of roots, and approximation to roots; he had become part of the furniture in algebra.

4. *Mechanics.* Three traditions were evident by the late 18th century; Newton's three laws and inverse square; energy conservation and energy / work exchange; and 'analytic' (algebraic), using d'Alembert's, least action, and virtual velocities principles. D'Alembert's principle was usually deployed to make Newton's $F = ma$ into a theorem, not an assumption. Newton was most effective in celestial and planetary mechanics, but aided by Euler's trigonometric series expansions as the main method to develop perturbation and orbit theory. His mechanics also served quite well in terrestrial mechanics; for example, projectiles. Newton was credited especially for his law of universal gravitation (Laplace *Exposition* (1796+), and other writers). Great interest fell upon equipotential surfaces and attraction theory: Newton (and Maclaurin) were known as pioneers, but analytical methods were preferred (especially by Laplace and Legendre), for their greater generality.

5. *Optics.* In the 18th century various people used both Newton's corpuscular theory and wave theories. Newton was mentioned concerning his approach, and in particular over the issue of the number of prinary colours in the visible spectrum. Laplace adopted his kind of theory from 1805 (initially for atmospheric refraction), as part of his new general programme of raising the status of physics by developing a mathematicised molecular version of it. Malus was the best theorist (for example, in polarisation). But by the mid 1820s this approach was being eclipsed by Fresnel's wave theories: while mechanics in general inspired several of Fresnel's moves (decomposition, conservation of energy, and so on), Newton's optics was then going out of use.

6. *Heat diffusion.* Biot (1804) and especially Fourier (1807+) used 'Newton's law of cooling' as the basis for heat diffusion, as refined by figures such as Amontons and Rumford. Fourier (1833) acknowledged Newton and the successors; but he was positivist (in Comte's later sense) over the nature of heat, rejecting speculation as to its nature.

7. *Sound.* In 1802 Laplace picked up Newton's fudge over 'correcting' the speed of sound, and gave this problem to his followers. The main results were due to Poisson (1807) on velocity, and especially to Laplace (1816, 1821) on the two specific heats of a gas. The influence of Newton here was rather negative.

8. *Electrostatics and magnetism.* Coulomb (1785+) adopted both central forces and an inverse square law of attraction and repulsion, emulations doubtless inpired by Newton's mechanics. Poisson acted much the same (1812+), relying much

on Coulomb; he also took a comparable stance over magnetism (1824 +), even beginning with a mention of Newton. When Ampère took up in 1820 the new electromagnetism and especially discovered electrodynamics, once again central forces and inverse squaredom formed parts of his emulation. However, he said little about Newton until strongly praising him in the opening of his book (1826).

9. *General and historical sources.* The main general history was by Montucla but completed by Lalande as *Histoire des mathématiques*; the most relevant volume is 3 (1802). They mentioned Newton fairly frequently, and quite warmly; for example, fluxions 'sont à la vérité plus exactes' than indivisibles, although it was contaminated with kinematics. There was quite a lot on infinite series, both British and continental. Newton's mechanics was noted but not strongly, and the part of *Principia* on hydrodynamics was judged to be the weakest. Lalande treated astronomy in volume 4 (1802), praising for Newton on the flattening of the Earth, precession and lunar theory. The section on optics was large, but with little on mathematics and only a few pages at the end on theories (where Lalande was neutral). Bossut's various chatty historical writings were quite objective about Newton, even neutral over the priority dispute. The main texts are his 'Discours préliminaire' to the *Encyclopédie méthodique* (1784) and his two general *Histoires* (1802, 1809). In his report on the progress of science since 1789 Delambre (1809) praised Newton, as with Laplace especially for universal gravitation. He gave more details especially in his *Histoire de l'astronomie au dix-huitième siècle* (1827).

Biot developed a strong partisanship for Newton. He even found the treatment of sound to be 'a sort of inspiration'! In his *Essai* on science since the French Revolution (1803) he praised universal gravitation as usual. Later he also produced a 70-page biographical article on Newton for Michaud's *Biographie universelle* (1822), which was translated into English in 1833. He helped his grandson-in-law Lefort to make an edition of the 'Commercium epostolicum' in 1856, though partly for Lefort to dispute Newton's conclusions about Leibniz's dishonesty!

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Fatio and the ***Principia***

S. Mandelbrote

This talk considered the relationship between Isaac Newton and Nicolas Fatio de Duillier (1664–1753), through the prism of Fatio's involvement in the editing of the *Principia* (first published in 1687). It noted that eighteenth-century historians of philosophy and Newtonianism had themselves remarked on Fatio's part in the history of gravitational theories. In particular, the Genevan natural philosopher and critic of Newton, George-Louis Le Sage played an important role in the rediscovery of Fatio's books and manuscripts, which interested him because of his concern with accommodating modern theories to ancient notions of gravitation.

The remarkable intellectual precocity of Fatio, and his early collaborations with Cassini (in the study of zodiacal light) and Huygens (on a number of mathematical and physical topics) were sketched. By the time that Fatio came to England in summer 1687, he was fêted as a result of the part that he had played in foiling an attempt by French spies to seize the Dutch stadhouder, William of Orange. Fatio's reception in England between 1687 and 1689 have to be placed in the context of the revolutionary debates of those years, which culminated in the Dutch invasion of autumn 1688 and the establishment of William on the throne of England. Although Fatio initially acted as a go-between for Huygens and prominent English scientists, his links with the Royal Society soon focussed on the question of gravitation, made prominent by the recent publication of the *Principia*. It was suggested that the appeal of Fatio to Newton was the prospect that he offered of finding a fully mechanical account of the causes and working of gravity. At the same time, Fatio was one of few contemporaries who had successfully acquired facility with the calculus and was capable of following the mathematics of the *Principia*. He also proved useful to Newton as a translator of French alchemical works. This intellectual collaboration, rather than any emotional attraction, formed the basis for the work that Fatio embarked on with Newton in the early 1690s. At the heart of that endeavour, was Fatio's offer to edit a new edition of the *Principia* and the work that he undertook towards doing so. This represented a response to some of the problems of incompleteness and unclarity that were apparent in the first edition. Fatio worked through several propositions, correcting minor errors and, most interestingly, adding references that brought out the relationship between Newton's arguments and ancient sources. In this respect, his editorial work was of a piece with Newton's work in the early 1690s to clarify the parallels between his own understanding of celestial mechanics and that possessed by Greek mathematicians and Stoic philosophers. The breakdown of Fatio's work on the *Principia* was attributed to developments in Fatio's own career, rather than to changes in his relationship with Newton. It was pointed out that new evidence suggested the continuing cordiality of Fatio and Newton, and drew attention to Fatio's continuing role in the propagation of Newtonian ideas, in particular through the calculus controversy.

The example of Fatio provided a case study that highlighted several concerns that might affect a project on the reception of Newtonian ideas. In particular, it drew attention to the role of personal contacts and friendships in the making of Newton's initial reputation. It showed the importance of the European community of natural philosophers, and the networks of exchange and discussion that had developed within it, for the first reception of Newton's work. This took place as much through correspondence, conversation, and semi-public meetings as it did through publication. It was marked by a competitive environment, not only between natural philosophers who considered themselves to be Newton's rivals, but also between challengers for the intellectual affection of the new philosophical star. Above all, the early reception of the *Principia* was shaped by the perceived incompleteness of that work's arguments and by the problems that it seemed to raise for the form and history of mathematics and natural philosophy. At a later stage, when the careers of Fatio and Newton diverged again, the place of a variety of institutions and media in shaping the image of Newton and the role of his followers could also be identified. Discussion of this longer history of the relationship of Newton and Fatio raised the question of the importance of skill in shaping responses to Newton, and the problem of keeping up with the pace of philosophical change that his work provoked.

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