Hist. Sci., xxxix (2001)

WHAT DID MATHEMATICS DO TO PHYSICS?

Yves Gingras
Université du Québec à Montréal

INTRODUCTION

The question of the relationships between physics and mathematics is as old as philosophy. In his *Physics* and his *Posterior analytics* Aristotle fixed the framework for discussions on the relations and distinctions between physics and mathematics as well as the nature of the mixed sciences right up to the Renaissance and well into the seventeenth century. It could even be argued that the basic impulse toward the mathematization of natural philosophy in the seventeenth century owes less to the "platonism" of Galileo and others invoked by Koyré than to the long tradition of discussions on the mixed sciences. These discussions tended to extend the use of mathematics to domains beyond those considered by Aristotle as the "more physical among the mathematical sciences",[1] that is, Astronomy, Optics and Harmonics. Mechanics was to be added following the Renaissance recovery of the Pseudo-Aristotelian *Mechanical problems*.[2] Without concern for what Aristotle 'really meant', or for the fact that his physics was not mathematical, philosophers offered different interpretations of his views on mixed sciences, some stressing the incompatibility of physics and mathematics, others pointing to their compatibility using examples taken from the mixed sciences. Renaissance discussions of the mixed sciences thus contributed to extending the latter's domain beyond the three canonical fields.[3] John Dee, for example, in his "Mathematical Preface" to the English translation of Euclid's *Elements of geometry* of 1570, insisted on the usefulness of mathematics for just about every domain of knowledge. And the seventeenth century saw the publication of many essays on the "usefulness of mathematics" running from the empirical Robert Boyle to the mathematical Isaac Barrow, followed at the turn of the century with essays by Fontenelle and John Arbuthnot.[4] Boyle's subtitle explicitly suggested that "the Empire of Man may be promoted by the naturalist's skill in Mathematicks (as well pure, as mixed)",[5] while for Barrow, the usual distinction between mathematics and the mixed sciences was artificial because mathematical objects "are at the same time both intelligible and sensible in a different respect". Thus, he considered that "mathematics, as it is vulgarly taken and called, is co-extended and made equal with physics itself".[6]

Barrow was not alone in entertaining this view. For John Wallis, for example, who worked on mechanics (a mixed science *par excellence*), it was obvious that physics was intimately related to mathematics. In the course of a discussion with Oldenburg, Wallis noted that he was surprised to learn that "the Society ['s members]

0073-2753/01/3904-0383/$2.50 © 2001 Science History Publications Ltd

in their present disquisitions have rather an Eye to the physical causes of motion, & the principles thereof, than to the mathematical Rules of it". He then commented that he considered his hypothesis on motion to be

> indeed of the *Physical* Laws of Motion, but *Mathematically* demonstrated. For I do not take the physical & Mathematical Hypothesis to contradict one another at all. But what is Physically performed is Mathematically measured. And there is no other way to determine the Physical Laws of Motion exactly, but by applying the mathematical measures & proportions to them.[7]

For Wallis the physics of motion *was* mathematical and he could simply not understand what Oldenburg meant by separating them or even giving the impression of opposing them. And Huygens, whose *Orologium oscillatorium* published in 1673 gave new examples of the geometrization of natural philosophy, was still complaining to the Marquis de l'Hôpital in December 1692 that:

> We find so few occasions to apply geometry to physics that I often find that surprising. For this, with mechanical inventions, is what merits most of our attention; otherwise, as Seneca said somewhere, we lose our intelligence in playing with futile calculations.[8]

Huygens thus makes explicit the relation between mathematics, mechanics and practical utility that is often present in the tradition of the mixed sciences.

Finally, Newton himself, who followed Barrow's lectures and succeeded him in the Lucasian Chair at Cambridge University, made clear the continuity between the mixed sciences and natural philosophy in his Optical Lectures of 1670–72 when he said about the use of mathematics that

> just as astronomy, geography, navigation, optics and mechanics are held to be mathematical sciences, though they deal with ... physical things, so although colours belong to physics, nevertheless scientific knowledge of them must be considered mathematical, in that they are treated through mathematical reasoning.[9]

Whereas discourses concerning the mathematization[10] of nature have been largely discussed, following Koyré's lead, through the lens of Platonic philosophical influences, the above considerations suggest that a more fruitful approach would be to see this process as the *extension* to other fields of the tradition of the mixed science.[11] As recent work has shown, this process had important repercussions on the transformation of the disciplinary boundaries between mathematics and natural philosophy in the seventeenth century.[12] But here, I would like to concentrate on what could be called the long term *unintended consequences* of the use of mathematics in physics, which have received scant attention from historians of science.[13] It is thus the *effects* rather than the causes (or reasons) of the mathematization of physics that I want to follow in this paper.

My starting point will be the publication of Newton's *Principia* which marks, conceptually, a radical departure from the then dominant tradition of a mechanical philosophy that explained phenomena, most often qualitatively, by contact forces. I will defend the thesis that by taking the mathematical route to natural philosophy, Newton initiated, or at least accelerated, a series of social, epistemological and even ontological consequences which, over the course of a century, redefined the legitimate practice of physics. As we will see, although these consequences were indirect and often only confusedly perceived by the actors involved, they led finally to the state of affairs we now generally take for granted: that physics *is* mathematical in its formulation. Far from being obvious, this idea was long debated in the eighteenth and even in the first half of the nineteenth century as more and more domains of physics lent themselves to mathematical formulations. By concentrating their attention on the 'winners', that is those who have accepted the mathematical conception of natural philosophy and physics, historians have not analysed the resistances to mathematization. In a recent book, for example, John Henry wrote that after the publication of the *Principia*, readers "took for granted the validity of mathematics for understanding the working of the world" and that "although his book met with some fierce criticism, not a murmur was raised against it in this regard".[14] As we will see, this was far from being the case but to recover these murmurs, one must look at actors who are now unknown precisely because they rejected the mathematization of physics and were thus excluded from the field (and its history) as it evolved in the eighteenth and nineteenth centuries. And it may be significant that only medicine and iatrochemistry have been examined as cases of resistance to mathematization, as if there could be little such resistance in physics after Newton's *Principia* made the power of mathematics 'obvious'.[15]

Sketching the elements of a larger research program, this paper will focus on the major effects of mathematization mentioned above: (1) social: the use of mathematics had the effect of excluding actors from legitimately participating in discourses on natural philosophy; and (2) epistemological: the use of mathematics in dynamics (as distinct from its use in kinematics) had the effect of transforming the very meaning of the term 'explanation' as it was used by philosophers in the seventeenth century. A third unintended consequence of the progress of mathematization, which we will only broach in the last section, was ontological: by its ever greater abstract treatment of phenomena, mathematization led to the vanishing of substances. Not only Cartesian vortices but also the luminiferous ether were dissolved in the acid of mathematics, and I have suggested elsewhere that the same process was at work in the transformation of the concepts of mass and light (photons and wave–particle duality).[16]

A QUESTION OF SCALE

To observe this ongoing process of mathematization one cannot limit oneself to a micro-analysis of a given situation and must, on the contrary, combine a local and a global perspective in order to find the directions taken by these transformations. As Duhem stated in another context:

> whoever casts a brief glance at the waves striking a beach does not see the tide mount; but under the superficial to-and-fro motion, another movement is produced, deeper, slower, imperceptible to the casual observer; it is a progressive movement continuing steadily in the same direction and by virtue of it the sea constantly rises.[17]

This paper will try to describe the rising tide of the mathematization of physics by calling attention to its largely unintended social, epistemological and ontological consequences. But before beginning our enquiry, it will be useful to avoid misunderstandings by saying a word about our methodology.

Over the past thirty years, the social history of science has developed, in part, on the basis of a strong reaction to what was perceived as a 'whiggish' and anachronistic view of the development of science and has tended to make very detailed examinations of episodes that were precisely delimited in space and time. This microscopic view has greatly enlarged our understanding but it has also had the consequences of focusing attention almost exclusively on actors' categories and of dissolving any analytical (conceptual) category forged for making sense of the *longue durée.* Another aspect of these developments has been the tendency to limit the analysis of events to an understanding of their immediate context and to inquire only into the reasons the actors may have had to do what they did. Though these questions are perfectly legitimate and very interesting in themselves, they do not cover the whole range of possible questions that an historian of science may wish to ask of his sources.

In this paper, I will for instance not address the question of *what* precisely 'natural philosophy' meant in Newton's days, or *why* different actors came to believe that natural philosophy should be mathematical.[18] As noted above, I wish to examine the *unintended consequences* of this choice, irrespective of the reasons various actors may have had in making it. For it is plain that actors' intentions and programs do not always square with the consequences of their actions. In fact, many sociologists would tend to think that most effects result from the unintended consequences of action.[19] And because this question is not only *local* but in a sense *global*, since these effects were not instantaneous but had repercussions over a long period of time, we do need categories that transcend those used by the actors at a given time and place. In the seventeenth and eighteenth centuries, for example, the French used the words 'physique' and 'physicien' where the British still used 'natural philosophy' and 'natural philosopher'. And though for Newton 'natural philosophy' may have been about God,[20] it, as we shall see below, did not stop his Continental readers from viewing his work as part of 'physique' or 'geometry' and, therefore, from putting God aside. Thus, for present purposes, I will use the two words interchangeably since the focus of our attention is not the meaning of those words at a given time and place (a meaning that in fact changed over time and place) but rather the reaction of various actors to the use of mathematics for understanding inanimate matter, whatever the name that was attributed to a given domain at a given time or place (physics, mechanics, dynamics, hydrodynamics, electricity, magnetism, etc., are all domains that in the course of time came to be included in the term 'physics').[21] And what has been said of 'physics' also applies to 'mathematics': its *content* obviously changed over time as well, being essentially geometry and proportion theory in the seventeenth century, calculus and differential equations in the eighteenth century, and then vectors, tensors and matrices in the nineteenth and twentieth centuries, to name only a few of the new tools used in physics at different times. The term 'mathematics' thus covers them all. In others words, the analytical categories used in historical inquiry can differ from the categories used by the historical actors for they depend on the nature of the questions framed. By being attentive to the scale (micro-, meso-, macro- in space as well as in time) at which a question is raised and by not confusing *analytical categories* with *actors' categories* and by not automatically transferring concepts or terms used at one level to another, one could more easily eschew artificial debates[22] about 'whiggishness' or anachronism.[23] This does not mean that the actors' categories are not important but that they are not *sufficient* for constructing historical narratives in the *longue durée*. And only such an approach covering a period roughly from 1700 to 1900 can make visible the social, epistemological and ontological consequences of the mathematization of physics.

MATHEMATIZATION AND EXCLUSION

Three weeks before the publication of Newton's *Principia*, Fatio de Duilliers, then in London, wrote to Huygens that some members of the Royal Society thought that "since the meditations [of Newton] all physics has been changed".[24]

Notwithstanding the fact that Fatio saw "physique" where his colleagues saw 'natural philosophy', his letter clearly suggests that there was already a feeling that the publication of the *Principia* was somehow revolutionary. One thing is certain: it launched a new phase in the ancient debate over the nature of the relations between physics and mathematics. Though Galileo preceded Newton in applying geometry to free fall, he did not concern himself with the efficient cause of that fall and left that aspect outside his mathematics. The counter-intuitive effects of the mathematization of physical phenomena only began to be perceived with the development of dynamics, that is, the mathematization of the concept of force, as the cause of change in the state of motion.[25] Though discussions concerning the social and epistemological consequences of the Newtonian approach were (though implicitly) often discussed in the same texts, I will treat them separately. In this section, I will concentrate on the threat that many practitioners saw in the ever-growing use of mathematics for their legitimate participation in discussions on matters of natural philosophy. It must be remembered that Descartes's physics and cosmology as exposed in his *Principles of philosophy* were essentially qualitative, a "mathematical physics without mathematics" to use Mouy's expression,[26] and were easily accessible to all those who liked to discuss natural phenomena in a simple and literary manner. As we shall see, they thus sensed, often confusedly, the threat that advanced mathematics posed to them, felt excluded from the discussion, and reacted accordingly.

In 1710, for instance, a Prague correspondent, reporting to the *Journal de Trévoux* on a recent book by the Count of Herberstein, noted with approbation that, according to the Count, one could abuse the use of mathematics in physics and that "some of our contemporaries push algebra, the science of curves, the search for centripetal and centrifugal forces to refinements that are not far from useless".[27] The Count and his correspondent may have had in mind the work of Pierre Varignon who, from 1699 on, published a series of papers in the *Mémoires* of the French Academy of Science using Leibniz's calculus to find the properties of central forces, thus translating (and generalizing) some of Newton's results in the language of analysis, without inquiring into or even commenting on the mechanical cause of gravitation.[28]

The *Journal de Trévoux*, like the *Journal des savants*, the *Mercure de France,* the *Journal encyclopédique* and the *Bibliothèque universelle* in France, or the *Gentleman's magazine* in England, was expressly targeted at a large audience of learned people interested in literary and scientific matters pursued as a stimulating source of discussion but not as a full time and systematic endeavour as performed, for example, by the members of the French Academy of Science.[29] The negative reactions of many members of this audience toward the use of mathematics in physics is, as we shall see, a sign that they did not consider themselves as outsiders to the discussions and that during the eighteenth century a boundary was slowly being established between those who were technically competent to discuss physical problems and those who were accustomed to explaining the 'causes' of phenomena in verbal terms.[30] Hence, mathematics was a threat to all those readers of magazines and members of provincial academies who were unfamiliar with precise mathematical formulations of physical problems and who preferred clear mechanical ideas to precise calculations based on what they considered as "metaphysical forces". While most of Newton's critics simply pointed out the absurdity of an attraction in a vacuum, one of the strongest disciples of the vortex theory of planetary motion, Privat de Molières, went further in trying to show the *mathematical* compatibility of the vortex theory with Kepler laws. Before him, Leibniz and Jean Bernoulli, among many other less well-known individuals, had also tried their hand, without success, at this problem.[31] In trying to combine a mathematical approach and a physical one based on contact action through a vortex, Privat de Molières was forced to make a compromise. Perhaps desperate to arrive at conclusive results, he noted in his 1733 memoir to the Paris Academy that natural phenomena are only "à peu près", that is, not very precise, so that there is no point in trying, like Newton, to reduce them to geometrical precision:

> However it will happen that it is only approximately that the points of the vortex will have this force which depends on the square of the distance and thus the area they span will also be only approximately as the time; but this will simply be more in conformity with the astronomical observations; thus the mechanical forces of the vortex give us the astronomical laws as they are in effect with

> a better precision than the purely metaphysical forces of Newton which give those laws with too much geometrical precision.[32]

Privat de Molières thus clearly preferred the "à peu près" of vortex theory to the artificial geometrical precision of Newton's "metaphysical forces". His critique of the excessive precision implicit in Newton's mathematization was well received by the permanent secretary of the Academy, Fontenelle who, in his annual summary of the activity of the *Académie Royale des Sciences* called attention to Molières's argument that, applied to phenomena, physical principles did not have geometrical precision: "M. l'Abbé de Molières astutely criticizes Newton on his extreme precision; physical principles are not so precise when we come to apply them to phenomena."[33]

This notion that nature does not suffer too much precision, was reminiscent of Aristotle who noted in the *Metaphysics* that "the minute accuracy of mathematics is not to be demanded in all cases, but only in the case of things which have no matter. Hence its method is not that of natural science".[34] This idea was still alive in the eighteenth century. The abbé Nollet, for example, explained to his audiences that "it is dangerous for a physicist to develop too great a taste for geometry" since in physics "one never finds either precision or certainty".[35] This was to take a road opposite to Newton's, who explicitly stated in his "Preface to the Reader" that "errors do not come from the art but from those who practise the arts"; and that "if anyone could work with the greatest exactness", he could develop an exact mechanic.[36] For only if nature is exact can it be understood through mathematics.

An excellent example of the growing difficulties attending those who, in the middle of the eighteenth century, wanted to contribute to the discussions on the nature of physical phenomena without using a mathematical approach is offered by the writings of Cadwallader Colden (1688–1776). A colonial administrator and politician, he had studied medicine in London before migrating to America where he had a successful career, becoming Lieutenant Governor of New York in 1761. A friend of Benjamin Franklin, he also had scientific and literary ambitions and corresponded with many well-known figures of his time. His major book, *The history of the five Indian nations depending on the Province of New York*, was published in 1727 and frequently reprinted. In 1745, he published in New York *An explication of the first causes of action in matter: and of the cause of gravitation*. He sent copies to his London friend Peter Collinson, a Fellow of the Royal Society of London, who gave one to the Society and distributed the rest across Europe using his correspondence network.[37]

The interest shown in the book by a cultivated public is suggested by its being reprinted in London the year after and by the publication in 1748 of a partial Dutch translation printed in Leipzig and Hamburg.[38] Colden also published an expanded edition of his views in London in 1751 under the new title, *Principles of action in matter, the gravitation of bodies and the motion of the planets, explained of those principles*. A French translation was published that same year in Paris.[39] Extracts also appeared in the *Gentleman's magazine*.[40]

Probably conscious that his theories of matter and gravity would be perceived as those of a dilettante, he insisted that he had been “constantly employed in business” and so “could not pursue his studies other ways than by way of amusement”.[41] In a letter to Collinson, Colden even mentioned that “times are very improper for the speculations in which I employ my thoughts” since “we are all in great hurry in preparing to attack the French Settlements in Canada”.[42] More importantly, Colden insisted in his essay that “with a competent skill in astronomy, equations may be formed for the motion of the planets, without any aid of the conic sections, or any other knowledge, besides the common rules of arithmetic and trigonometry”.[43] Clearly distancing himself from the mathematical treatment of gravitation by geometers, he insisted he “had more in view to convey his conceptions clearly to others, than to elegance in style; or the pomp of a demonstrative method”.[44] Despite the interest shown in his ideas on the cause of gravity, however, Colden attracted no disciple other than his son. In view of this, the fact that someone of Leonhard Euler’s stature took the time to “mercilessly pull the book to pieces”, to use the words of Colden’s biographer, is a significant indication that the frontier separating insiders from outsiders (the first thus becoming the ‘expert’ or ‘professional’, the latter the ‘amateur’ or ‘dilettante’), was still not clearly defined in the middle of the eighteenth century.[45]

Euler had obtained a copy of the book from the Reverend Caspar Wetstein, a member of Benjamin Franklin’s and Peter Collinson’s circles. Euler’s comments are contained in his letter to Wetstein where he noted that “the American philosopher” had little knowledge “of the principles of motion” and this “entirely disqualifies the author from establishing the true Forces requisite to the motion of the planets from whatever cause he may attempt to derive them”.[46] Euler’s reaction is particularly interesting for he was among those who believed in the existence of a fluid whose pressure could explain gravitation. But as he wrote to Le Sage, who had also tried his hand at a mechanical model of gravitation, “the theory of fluid movement is not enough studied yet to produce a complete explanation”.[47] In other words, a mechanical explanation of gravity had to take, even for Euler, a mathematical form, in his case through the laws of hydrodynamics.[48] Though it was intuitive, verbal physics was thus no longer a legitimate way of providing a physical explanation when it was inconsistent with the mathematical laws of physics.

In excluding Colden as “a man who has not entirely devoted himself to the study of [the cause of gravity]” and who ignored “the first principles of hydrostaticks”,[49] Euler made plain that the price of entry into the club of legitimate practitioners was an adequate knowledge of mathematics. As the frontier delimiting the field of activity became better defined, contributors who proposed new explanations of phenomena that did not take the field’s implicit rules into account were met with silence or dismissed without further argumentation as being beside the point (and outside the field). The historical dynamic of the creation of these boundaries, constitutive of scientific fields, varies of course with domains and the nature of the tools used to investigate objects. Whereas mathematics helped define the boundaries of rational mechanics (and later electricity and magnetism), in other fields, like chemistry for example, the sophistication and cost of instruments and reagents played a more important role than mathematics in defining the 'cost of entry' to the field. Once the boundaries were well defined and the gate keepers controlled access to the legitimate places of publication, outsiders could make their voices heard only in books or in magazines of general interest. Thus, Colden could still ask his friend Collinson to try to have his answer to Euler presented to the Royal Society (Collinson had told him that "Pro-Euler remarks were read before the Royal Society"), but having failed to have his views printed in the *Philosophical transactions of the Royal Society*, he was reduced to urging him to do his best to have them appear in the *Gentleman's magazine*.[50]

THE RISE OF A PRIVATE SCIENCE

For some, the critical stand toward the use of advanced mathematics in physics was related to their conviction that science had to be accessible to a wide circle of people. As noted by Shapin, "Boyle repeatedly remarked upon the relative inaccessibility of mathematics", the use of which restricts "the potential size of the audience".[51] In 1690, at a time when he was still trying to explain gravity by mechanical means, Varignon also noted that treating physics through geometry makes the former unintelligible to those untrained in the latter. Asking himself rhetorically if one could understand the mysteries of nature without the help of geometry, he concluded that the only thing he could do to help the general reader was to provide a general plan of his treatise without giving the details of his demonstrations. His *Nouvelles conjectures sur la pesanteur* were thus preceded by a *Discours sur la pesanteur* with a different pagination.[52]

For the Jesuit Louis-Bertrand Castel, for example, who was part of the editorial board of the *Journal de Trévoux* from 1720 to 1745, science had to be accessible to the common man and thus not only higher mathematics but fancy experimentations were to be excluded from proper scientific methods. One could read in his *Journal*, probably under his pen, that "the experiments capable of perfecting physics, ought to be easy to make and to repeat at any time, and almost by everyone".[53] His *Vrai système de physique de M. Isaac Newton* printed in 1743 was subtitled "*à la portée du commun des physiciens*", that is, accessible to the common physicist.[54] On Newton's prism experiments he noted, no doubt with some exaggeration, that "in order to make these experiments on the refraction of light correctly one must be a millionaire".[55] In his comment on Castel's *Traité de la pesanteur* published in 1724, the abbé de Saint-Pierre already insisted that it was important to make discoveries accessible to the common reader; the thing was difficult, he conceded, but necessary.[56] As late as the 1770s, we still find echo of this demand for 'democratic' apparatuses in Priestley's complaint that Lavoisier's instruments were too costly and too delicate to constitute a norm for chemical proof.[57] The 'privatization' of science thus took many roads, reaching new limits today with the construction of a *single* accelerator capable of producing the most esoteric kind of elementary particles.[58]

Castel was not alone in criticizing the inaccessibility of mathematical physics. Diderot, who was also on good terms with Castel,[59] distanced himself from his friend d'Alembert on this very question of the usefulness of mathematics in the natural sciences, first in his *Lettre sur les aveugles* of 1749 and two years later in *De l'interprétation de la nature*.[60]

Diderot's opposition to the use of higher mathematics in physics was also related to its exclusionary effects since this language could not be understood by the lay reader. He was convinced that the most abstruse book of natural philosophy, namely Newton's *Principia*, could in the space of a month have been made clear by its author, who in this way would have saved three years of labour spent by a thousand good-spirited fellows interested in understanding his discoveries. For Diderot it was important to make philosophy popular and to raise the people to the level attained by the philosophers. And to those who did not believe that it was possible to make all knowledge accessible to the multitude, he answered that they simply ignored what could be achieved using a good method and a long habit of work.[61]

This ideal of a 'public science' could only lead to a rejection of the use of advanced mathematics in physics, the understanding of which necessitated years of training. This philosophy of science was of course consistent with the existence of a social space of discussion that was easily accessible to the enlightened public of the literary and scientific magazines of the times. On the other hand, the extensive use of "transcendental" mathematics, to use Diderot's term, led directly to a closed space accessible only to those with the appropriate training in mathematics. Experimental physics was more easily accessible to what was called "the public" and Diderot had a clear preference for it though he was not uncritical of public demonstrations he considered superficial.[62] The members of the French Academy of Science for instance appreciated the fact that Nollet's *Leçons de physique expérimentale* made that science "accessible to a larger number of persons" than the more technical geometrical treatises.[63]

Although excluded from the mathematized sectors of physics, amateurs still thought they could legitimately comment on experimental matters. In 1769, for example, a reader of the *Journal encyclopédique* wrote a comment on a report of one of Nollet's experiments, asserting that despite all the respect he had for the Academy and Nollet's expertise, everyone in matters of physics "has conserved the right to propose his own opinion".[64] It was this very "right to an opinion" without having the proper prior training that the mathematization of physics was putting into question.

Reacting to Diderot but also to Buffon, D'Alembert clearly stated his views about the central role of mathematics in physics. In the introductory lines of his "Research on the precession of the equinoxes" of 1749, he wrote that, enlightened by the observation of nature, the *esprit de système* can suggest the causes of the phenomena but that "it is left to calculations to confirm, so to speak, the existence of these causes by determining exactly the effects they can produce and by comparing those effects to those discovered by experiment".[65] And in the concluding paragraphs he affirmed that

> it is not sufficient for a system to satisfy the phenomena only in a vague and general manner, or to provide plausible explanations of some of them: the details and the precise calculations are the touchstone; only they can tell if one must adopt, reject, or modify an hypothesis.[66]

Calculations were for d'Alembert the final arbiter and if they confirmed Newtonian attraction, for example, then every one would have to live with it (geometers as well as metaphysicians) even at the price of having to admit a new property of matter or of having to abandon a clearer idea of the virtue by which bodies attract each other as well as collide into one another. Though he was conscious of the possible excesses of the use of mathematics in physics,[67] it remained that for him, all things considered, calculation had precedence over "clear and distinct" mechanical ideas that were not confirmed by calculations.

D'Alembert went even further by believing that progress in the methods of calculations could lead to the progress of physics. For despite the tremendous difficulty of calculating the interactions between planets gravitating according to Newton's laws, he noted that "the continuous perfection which analysis attains day after day" gave reason to be optimistic that a solution to the problem would come.[68] Commenting on Newton's contribution to the difficult problem of the motion of the Moon, he observed that "fortunately" the "calcul analytique" had developed since Newton and had become more general and more practical ("plus commode") and thus offered the possibility of perfecting the work begun by "the great philosopher".[69] He also often insisted on the difficulty and time-consuming work of these calculations, thus suggesting that they could hardly be done by any dilettante and that only those who had tried their hand at such calculations could really understand them; the others had to content themselves with a superficial view ("idée légère") of these things.[70] Thus, and paralleling the "rise of public science" analysed by Larry Stewart,[71] mathematics contributed to the rise of a "private science", accessible only to the adequately trained. The outsiders, having to content themselves with a superficial understanding of what was really going on, could no longer be considered legitimate active participants and contributors to a now esoteric (as opposed to exoteric) field of knowledge. For d'Alembert, the era of verbal (or literary) physics was over; at least in matters concerning the Newtonian world system.

The growing exclusion of non-geometers from legitimate participation in discussions of physical problems is also clear in a letter of Clairault to Euler in 1749, in which he wrote, in the context of the debate over the shape of the Earth, that one should despise the objections coming from "newtoniens non-géomètres" and resist the temptation to answer them.[72] This dominating attitude contrasts with that of the astronomer Edmond Halley who, a century before, had written to Newton, then busy writing his *Principia*, that "philosophers without mathematics" were "by much the greater number" in the Royal Society.[73] In his *Dictionnaire de physique*, published in 1761, Paulian had also taken notice of the fact that it was now impossible "to become a physicist without a minimum knowledge of mathematics".[74]

THE RESENTMENT OF THE EXCLUDED

Though the mathematical way to physics represented first and foremost by Newton's *Principia* was largely accepted by those mid-eighteenth-century *géomètres* who devoted themselves to extending its applications to terrestrial and astronomical phenomena, there was still a vocal group of people who were more interested in verbal explanations than in mathematical calculations. For them, less talented or less interested in investing long hours in abstruse mathematical calculations, the legitimacy of their verbal contributions to physics was threatened by views like those put forward by d'Alembert and they did not leave the stage without protest even after 1750. Massière, for example, may have perceived himself as such an outsider, for he admitted writing on a scientific subject without being a *savant*. In 1759, he published his critical reflections on the system of attraction, a 400-page book which, he admitted, "owes its existence only to the boredom and idleness of the countryside".[75] Though he had not read the *Principia* but used Voltaire's and 'sGravesande's books on Newtonian physics as a substitute, he was nonetheless shocked by what he found: "I saw that this part of Newton's philosophy [on the movement of the planets] consisted only in calculations; and it seemed to me that this was not the trade of a philosopher."[76] Having noted that Newtonians calculate everything, he added: "for me, who am not a calculator, I must admit that I felt myself revolted against this new kind of philosophy."[77]

The ubiquity of geometry and equations in many physics treatises provoked Massières into asking if there was not some affectation in this manner of philosophizing on the part of their authors. And like Colden before him, he told his readers that "any man who knows what are right angles, obtuse angles and acute angles can understand the essential part" of his book and that he himself did not know enough algebra "to speak the language of the *savants*".[78] Declaring, following many others, the Newtonian conception of gravitation to be "occult", he echoed Castel's view, observing ironically that "to make the answer acceptable, there was only to add to it some calculations".[79]

The Comte de Lacépède, who presented himself as a member of the Academies of Dijon, Toulouse, Rome and many others, was also keen to ridicule attraction when he wrote that, though a "sensitive attraction" may seem obscure,

> following the example of the great Newton, I will envelop my hypothesis within geometric and algebraic veils to make it invisible to the eyes and criticisms of the uninitiated. If someone object to this obscurity, I will cite M. d'Alembert and others who modestly admit that there are propositions in the masterpiece of the English philosopher that stop even the most gifted geometers.[80]

Citing d'Alembert was deliberately ironic for, as we have seen, he was strongly opposed to the physics of the "à peu près". And as late as 1826, for example, in an attempt to explain gravitation in mechanical terms, J. Mangin, member of the Philomatic Society of Verdun, could still write:

> I know that all the analytical calculations of the defenders of this system [of attraction] are able to frighten many readers but it remains nonetheless true that all these calculations are only based on suppositions since the physical cause of attraction is unknown.[81]

About ten years later, Antoine-Louis Guénard Demonville tried in vain to have his papers on the true system of the world accepted by the French Academy of Science. Frustrated, he directed his venom at Denis-Siméon Poisson, denouncing the dictatorship of mathematics. As he wrote in the Preface to his book:

> M. Poisson is a mathematical monomaniac who will admit no new truth if he cannot find it already sketched in one of his axioms of algebra ... under how many mistakes will he not try to suffocate us in emptying his magazine of formulas![82]

When mathematics came to be applied to other fields such as architecture and the design of bridges and buildings, there were also strong reactions. In 1805, for example, the French architect Charles-Francois Viel even published a pamphlet titled *De l'impuissance des mathématiques pour assurer la solidité des bâtimens*. For him, the abuse of mathematics simply led to ill-constructed bridges that could not withstand the test of time like those that had been constructed by the ancients, who had never used sophisticated calculations to build their marvels.[83] And as we shall see below, even Faraday looked upon the mathematization of electricity and magnetism with a sceptical eye.

All these adverse and often aggressive reactions (and many others could be cited[84]) clearly show that there was real resentment from those who used to see themselves as part of an open intellectual space in which they exchanged views on matters of natural philosophy. Putting too much mathematics into physics thus restrained not only potential readers, as Boyle observed, but also potential contributors. In this process, the boundaries of what was a relatively open public space of discussion were slowly redefined in such a way that potential readers were increasingly limited to potential contributors, that is, to those with the appropriate training. In other words, mathematization contributed to the formation of a relatively autonomous scientific field, with its control of access mechanisms.[85]

We still find an echo of the debates on the importance of mathematics in physics in Jean-Baptiste Biot's Introduction to his *Traité de physique expérimentale et mathématique* of 1816, where he wrote that:

> Many people, in France and elsewhere, believe that Physics must be presented in a purely experimental form, without any algebraic apparatus. The English, so eminent in this science, think we use too much calculation and complicate

> it with our formulas instead of clarifying it. Many of them, who are very skilled and very exact, believe that the precision that we think we approach [using calculations] is purely ideal, since it goes infinitely beyond the limits to which these experiments are inevitably subjected. This question is fundamental and merits to be debated.[86]

He then went on to offer a detailed and strong twelve-page defence of the usefulness of mathematics in physics to which we will return below.

isolating faraday

Though mathematics came to dominate rational mechanics, it must be emphasized, as the time span covered here suggests, that the process was indeed very slow. Moreover, the mathematization of other fields like electricity or magnetism also had exclusionary effects which were again met with negative reactions.[87] Up to the middle of the nineteenth century, the study of heat, electricity and magnetism was relatively accessible to empirically- or qualitatively-minded people of the kind who liked to debate the nature of gravitation in the seventeenth and eighteenth centuries. They were then, though only for a limited time, protected from the recondite language of mathematics. In fact, the difference in the level of mathematical codification between gravitation and electricity in the mid-eighteenth century certainly explains in part the success of Benjamin Franklin as compared to the failure of his friend Colden, who occupied a similar social position and had a comparable intellectual training but chose to attack a field already dominated by geometers, instead of devoting himself to simpler empirical phenomena.

In 1831, the young James David Forbes, who two years later would become Professor of Natural Philosophy at the University of Edinburgh, had already understood that "in the present state of Science, a liberal basis of mathematical knowledge is indispensable to [the] successful prosecution [of the physical sciences]".[88] At the time of his nomination, he confessed to his mentor William Whewell that

> any doubt as to the propriety of viewing mixed mathematics as belonging to a natural philosophy class is at this moment peculiarly untenable: for the whole progress of general physics is happily so fast tending to a subjection to mathematical laws of that department of science, that in no very long time magnetism, electricity and light may be expected to be as fully the objects of dynamical reasoning as gravitation is at this present time.[89]

Mathematics was still making headway in physics and Forbes's teaching would prepare students accordingly.

Twenty years later, when Maxwell sent his paper "On Faradays's lines of forces" to the eponymous natural philosopher, the level of mathematics was already high and Faraday admitted to Maxwell that "I was at first almost frightened when I saw such mathematical force made to bear upon the subject and then wondered to see that the subject stood it so well".[90] Writing again eight months later he asked Maxwell:

> There is one thing I would be glad to ask you. When a mathematician engaged in investigating physical actions and results has arrived at his conclusions, may they be not expressed in common language as fully, clearly, and definitely as in mathematical formulae?[91]

Recalling that Faraday fashioned himself as a natural philosopher[92] and that he never used any mathematics for arriving at his fundamental discoveries, we can understand his uneasiness when confronted with Maxwell's formidable treatment of his intuitions. Like many of those who in the eighteenth century protested against the use of advanced mathematics in physics, Faraday, a lecturer at the Royal Institution, was accustomed to presenting his work to the public through experiments and images and he must have felt the exclusionary effect of Maxwell's advanced mathematics. In his paper "On the conservation of force", in which he suggested that gravitation must be explained by lines of force or by an ether, he was clearly on the defensive when he noted that "it may be supposed that one who has little or no mathematical knowledge should hardly assume a right to judge of the generality and force" of the principle of gravitation. But his answer was that mathematics "cannot of itself introduce the knowledge of any new principle".[93] Criticizing Faraday for misunderstanding the concept of potential energy, Ernst Brücke noted that "it is a long time since such a far reaching physical question has been touched upon wholly without the aid of mathematical apparatus — without the assistance of those wonder-working symbols whose brief rhetoric speaks more convincingly to the mind than the tongue of Cicero or Demosthenes".[94]

The central place of mathematics in physics had been explicitly stated by Maxwell just a year before Faraday expressed his views on the issue. In his inaugural lecture at Aberdeen in November 1856, Maxwell insisted that for him "natural philosophy is and ought to be mathematics, that is, the science in which laws relating to quantity are treated according to the principles of accurate reasoning", a conviction he repeated word for word four years later in his inaugural lecture at King's College.[95]

Thus, over a period of nearly two centuries, the progressive mathematization of various domains of physics had the effect of excluding as legitimate practitioners most of the readers (and sometimes contributors) of the scientific and literary magazines who used to talk about natural phenomena without using the language of mathematics. But as we have seen, they did not leave the place without reacting angrily to their progressive marginalization. By the middle of the nineteenth century, with the development of energy physics, electrodynamics and thermodynamics, a large part of physics had become the esoteric knowledge of a small group of students trained by the incumbents of physics chairs then developing in European universities and later by the members of physics departments of North American universities.[96] To quote Bachelard, where Castel and Nollet had "readers", Biot, Forbes and Maxwell had "students".[97] Combined with a tighter control of access to the membership of scientific academies and the emergence of specialized scientific disciplinary journals, these institutional settings would now define the boundaries of a private sphere where the legitimate practice of the discipline would carry on. These developments simultaneously constructed an "outside" where all those still interested in "explaining" gravity, squaring the circle or finding a *perpetuum mobile*, could search an audience for their theories without interfering with what was going on inside the field. It is significant that the Paris and Berlin Academies ruled out such topics in 1775, the Royal Society having done so earlier in 1749.[98] While Copernicus could defend himself against religious critiques of his system by claiming that "mathematics is for mathematicians", mid-nineteenth physicists could silence amateurs by claiming that physics is for (mathematical) physicists.

THE CHANGING MEANING OF 'EXPLANATION'

In addition to making the access to the practice difficult and time consuming, the mathematization of physics also had a more subtle epistemological effect that first became perceptible in the debate over the mechanical explanation of gravity. Though the debate over that question is now relatively well known,[99] the approach to this episode has concentrated on the various solutions and models proposed to save the validity of a mechanical and plenist cosmology from the mathematical objections first put forward by Newton. I believe, however, that the profound significance of that debate lies in the fact that it was the very meaning of the term 'explanation' that was at stake in the discussions concerning the legitimacy of hypotheses and in the contested interpretations of Newton's famous "hypothesis non fingo". This episode shows that the evaluation criteria for what was to count as an acceptable 'explanation' (of gravitation in this case) were shifting towards mathematics and away from mechanical explanations.[100] Confronted with a mathematical formulation of a phenomenon for which there was no mechanical explanation,[101] more and more actors chose the former even at the price of not finding the latter. This was something new. For the whole of the seventeenth century and most of the eighteenth, to 'explain' a physical phenomenon meant to give the physical mechanism involved in its production. Hence, Descartes could still reject Galileo's law of free fall because it was not based on a mechanical explanation of gravity.[102]

The publication of Newton's *Principia* marks the beginning of this shift where mathematical explanations came to be preferred to mechanical explanations *when the latter did not conform to calculations.* This shift in meaning helps explain the strong reactions against what were perceived as "occult" explanations on the part of partisans who based their cosmology on contact forces, which in turn implied that the universe had to be a plenum (or else constantly bombarded by particles). The nature of the shift of meaning implicit in Newton's approach to physics can be compared to the shift Kepler induced when he invented a "celestial physics", which was clearly a contradiction in terms in the context of scholastic natural philosophy wherein astronomy was distinct from physics.[103] And in point of fact, Maestlin plainly told Kepler that such a notion was contrary to common sense and good philosophy.[104] Likewise, in stating that since Proposition LII of Book II of his *Principia* proved by geometrical calculations that no plausible vortex movements could be made compatible with Kepler's laws and that all celestial phenomena could be understood using the law of universal gravitation, Newton was in practice saying that mathematics was replacing verbal formulations as the final arbiter and true explanation of phenomena. As Kepler had mixed physics with astronomy, Newton had mixed physics with mathematics and thus explained physical phenomena mathematically.[105] Whereas astronomers got used to the first, natural philosophers now had to adapt to the second.

Like Kepler's before him, Newton's category mistake was not long in being recognized.[106] As is well known, the review of the *Principia* in the *Journal des sçavans* in 1688 praised the author's geometry but concluded that to make his work "as perfect as it was possible" Newton now had to produce "a physics as exact as his mechanics", which, it was added, could be done only by substituting "true motion in place of those he has supposed".[107] In other words, only true mechanical contact between the parts of a plenum could be considered a *physical* explanation, whereas Newton had simply posited mathematical forces, which pertains to a different order of things, namely geometry, and thus could not constitute a true explanation of physical phenomena.

Though many authors criticized Newton for not giving a physical explanation of gravity, they rarely went beyond denouncing him for bringing back "occult qualities" or even "miracles". It is probably the Jesuit Castel who analysed in greatest detail what Newton was really doing with his new approach. As a devoted Cartesian, he was in fact well placed to see how Newton's explanations differed from those usually given by mechanical philosophers. He not only perceived the exclusionary effect of the higher mathematics used by Newtonians, as we have seen, but he also clearly pointed to the problematic nature of the explanations that Newton offered. And though a Fellow of the Royal Society of London, he was sometimes perceived as "moitié fou, moitié sensé", as Diderot said.[108] However, his often extreme opposition to Newtonian physics, though perceived as an embarrassment by many of his Jesuit colleagues and other less dogmatic followers of the Cartesian way in physics,[109] constitutes in fact an excellent *condensé* of what was so disconcerting from the point of view of the paradigm of mechanical explanations that defined the dominant schemes of perception and evaluation, that is, the scientific habitus[110] of those who reacted to Newton's *Principia.* Moreover, as his biographer pointed out, Castel probably represents "an historically significant majority opinion"[111] of those who read the *Journal de Trévoux* and similar literary journals and who were actively interested in the sciences along the lines developed by Descartes in his *Principia philosophiae*, that is, science as a kind of verbal physics based on clear and distinct ideas. This is the universe that Castel realized was being threatened by Newton and this is why he devoted an entire book to dissecting the logic of the mathematical route to natural philosophy.

Castel not only read the *Principia* many times, but he seems to have copied it in long hand since he could not secure a copy for himself.[112] He thus considered himself as having understood the book, though he could hardly understand why Newton constantly mixed up physics with geometry. His main line of attack was to make it plain that physics and geometry do not have the same status. Whereas he saw physics as essentially simple and concrete, he considered geometry as abstract and often mysterious. But for Newton, Castel says, "to think and to calculate, to reason and to calculate, to philosophize and to calculate, are all synonymous terms".[113] And it was because Huygens was more a geometer than a physicist that he had been seduced by Newtonian physics despite having been trained as a Cartesian.[114]

Commenting on the nature of Book III of the *Principia*, Castel noted that what Newton gives there as a "system of physics is in reality a system all mathematical". This makes the term 'physico-mathematical' entirely appropriate but, he adds, it remains to be seen if such a system "can be taken as a true physical system".[115] The implicit definition of the term 'physics' used here by Castel is one related to the idea that physics provides mechanical explanations which should not be confused with mathematical explanations, as Castel believed Newton was doing: "I have also remarked, he wrote, that this third book is physical only in the sense that the author adopts *as a physicist* the principles he had established *as a mathematician*".[116] After having stressed the difference between the physicist and the mathematician, he asked how Newton could transform an "*abstract*" mathematical proposition — like the one proved in Book One relating the elliptical movement of a body to the existence of a centripetal force declining with the inverse square of the distance — into a "*concrete reality*".[117] If he does it somewhere, he thought, it must be in Proposition XIII of Book III (on Kepler's law of areas). After having explained the proposition, he asked, "what is *physical* in this demonstration or in this explanation?" As far as he was concerned, he saw only a "*mathematical*" demonstration and had great difficulty seeing in it a "*physical demonstration*". One could admit that Proposition XIII links two facts together (the inverse square law and the ellipticity) but he insisted that the first does not explain the second, "one is not the cause, the reason of the other".[118]

Castel admitted that Descartes's vortices were not convincing but he insisted that at least "these things are physical". One could and should modify them to bring them closer to the truth, but one could not replace them "by *purely ideal reasons*, abstract and mathematical which have nothing to do with the idea of cause, of *physical*, *effective* and *operative* influence".[119] For him, there was not a single physical explanation in Newton's whole work because "*physical reasons* are necessary reasons of entailment, of linkages, of *mechanism*. In Newton, there is none of this kind".[120] And he added, in a *cri du coeur*:

> In truth, one will permit me to say, with the extreme respect one must have for Newton, that there is only geometry in his system and good physics will

> disappear if we allow him to continue.
>
> I admire his profound *geometrical reasoning*, but there is not (one must see it) a single word of physical reasoning in it all.[121]

Commenting on Proposition LI of Book II, which prepares the famous Proposition LII in which Newton shows through geometry that the Cartesian vortices are incompatible with Kepler's laws, he noted that "there is no physical idea that Newton does not automatically relate to a figure, a geometrical calculation, as if to hide the former under the envelope of the latter".[122] Castel repeatedly insisted on the distinction to be made between physics and geometry:

> Geometry is geometry only through the abstract simplicity of its object. Only that makes it certain and demonstrative. The object of physics is much vaster. That is what makes it difficult, uncertain and obscure. But this is essential to it: one is not a better physicist because one is the best of geometers.[123]

For Castel, the strategic mistake of the Cartesians had been to accept Newton's mathematical suppositions while trying to refute him. But to do that was to go on Newton's own terrain, where he could not be challenged:

> one must refuse all his principles or accept all their consequences. He is a geometer and a consequential one. This is to take him at his best. But, I repeat, he is not a physicist. Thus, one must stop him at the first step and show him that he makes suppositions, and false suppositions.[124]

Toward the end of the book, desperate for the possibility of physics returning to the "just notion of things" that still existed in Cartesian philosophy, he confessed his conviction that the confusion Newton had brought to physics was due to an abuse of geometry: "Will it be believed? It is the too intimate mix of geometry and physics which led to total confusion."[125]

Castel had rightly perceived that mathematics was at the core of Newton's physics and that this had led to the conflation of 'physical' and 'mathematical' explanations. He also understood that the second was abstract and the former concrete and that the more geometry would come to bear on physics, the more abstract it would become. With Newton and his followers, physics was no longer simple, natural and easy to understand as Castel thought it should be and it continued to "mix" mathematics increasingly with physics without offering those "physical" explanations that were the staple of the mechanical world view in which Castel had been trained.

In 1749, only six years after Castel had published his book, d'Alembert could close his *Introduction aux recherches sur la précession des équinoxes* with the remark:

> I will say nothing here of the explanation of the precession of the equinoxes given by the Cartesian vortex. The analysis of this explanation is not in the nature of this work and would also be out of season at a time when hypothesis and vague conjectures seem at last banished from physics.[126]

By explicitly excluding from the purview of his work the kind of mechanical explanation that was still seen by many as a legitimate part of physics, d'Alembert was suggesting that physicists should resist "the furor to explain everything, introduced into physics by Descartes",[127] thus claiming that from now on any 'explanation' had to take a mathematical form. For him, Newton had been the first to show "the art of introducing geometry into physics and to form, through the union of experience and calculation, an exact, profound, luminous and new science".[128] By contrast, Castel was saying that the price to pay for such a science was too high if it meant the abandonment of *physical*, that is mechanical, explanations of physical phenomena.

In 1752, only a few seasons after d'Alembert had declared that "vague conjectures" were at last banished from physics, some Cartesians tried to provide the public with "the best preservative against the seduction of what we nowadays call Newtonianism" by printing Fontenelle's *Théorie des tourbillons cartésiens*.[129] Echoing Castel's analysis, the editor wrote in his Preface:

> There is, so to say, two very different worlds; one mathematical, the other physical. The mathematical, which we can also call the metaphysical, only exists in the ideas of the geometer: he supposes the infinitely small, dots without dimensions, lines without width...; as well as vacuum and gravitation. All these suppositions are the basis of a calculation which without them could not be exact and which without this exactitude could not be demonstrative. But nothing of this can be found exactly in nature ... and this is a strange illusion to abuse of the abstractions in transposing them in the physical world as if they were real beings.[130]

Thirty years later, in their *Physique du monde*, De Marivetz and Goussier could still complain that the phrase "to calculate a phenomenom" was very improper and had been "introduced into physics by people who are better at calculations than at explanations".[131] The central role of mathematics in physics was thus still being contested and d'Alembert was conscious of the revolution initiated by Newton. In his *Essai sur les élémens de philosophie*, he explained that the generation which opposed the revolution, and of which Castel was an outspoken representative, finally "died or stayed quiet in the academies" while it was left to a new generation, of which he was part, to complete the revolution:

> when the foundations of a revolution are laid it is nearly always in the next generation that the revolution is completed, rarely before because the obstacles vanish instead of yielding; rarely long after because once the barriers are traversed the human spirit develops rapidly until he meets another obstacle that forces him to stop again.[132]

For his generation and the successive ones, the word 'explanation' had simply acquired a different meaning from the one it had previously held and the controversies that had surrounded the existence of vortices and occult qualities were thenceforth "out of season". This new meaning was also closely associated with the redefinition of what it was to do 'physics'. Living within the Newtonian worldview centred on mathematics, d'Alembert "could not read without astonishment, in some authors of physics, the explanations given of the variations of the barometer, of snow, hail and an infinity of other facts".[133] D'Alembert's astonishment was just the opposite of what had been earlier felt by Castel and other like-minded thinkers.

dissolving substances in the acid of mathematics

For those who promoted the extensive use of mathematics in physics, only that language made possible precision and generalization. Though critical of its consequences in physics, Diderot clearly understood this process: "the act of generalization tends to deprive concepts of their sensible aspects. As generalization advances, corporeal spectra vanish; notions move from the imagination to the understanding and ideas become purely intellectual."[134] On a more positive note, Biot explained in the Introduction to his *Traité de physique*, that forming equations was often the only way of making generalizations: "Could you, for example, solve the problems of physics where the volume of liquids is a variable element if you had not reduced to formulas the composed laws of their expansion?"[135] Moreover, like d'Alembert before him, he claimed that only through analysis can a theory really be tested:

> It is nothing to oppose [to a theory] a few particular phenomena often susceptible to diverse interpretations. It is by getting from the formulas the most subtle consequences, and the most distanced from the principles, and by testing them by experiments that one can really check if a theory is true or false, and if one can use it as guide or reject it as a deceiving system.[136]

In following a method based on analysis and experimentation, that is, in combining mathematics and physics, Biot thought he could do away with systems. For him, physics calculates more than it explains: "I do not say explain, everything can be explained, but calculate, that is, deduce mathematically."[137] Though he claims to eschew the word "explain", which in the context still referred to "mechanical explanation", his demands for mathematical "deduction" and calculation pointed in the direction of the changing meaning of the term, as we have discussed in the preceding section. For Biot, the logic of the manipulation of symbols helped the physicist by guiding his hands to the necessary conclusions. When a law is converted into an analytic formula, he says, "the simple play of the algebraic signs independently of any figure will suffice to direct you". Moreover, with such a formulation one can know "the abstract elements upon which laws depend, and read their influence in the formula, [and] discover the circumstances the most favourable to determine them directly".[138]

Nearly fifty years later, Maxwell provided a similar analysis of the role played by symbolic manipulations in physics. To avoid vagueness, he said, in his inaugural lecture at King's College, London in October 1860, "we must eventually make use of that method of expression which, by throwing away every idea but that of quantity, arrives at the utmost limit of distinctness. We cannot express physical facts except in a mathematical form".[139] He wanted to teach his students "not only the mathematical accuracy of expression of which all physical facts are capable, but the mathematical necessity of their interdependence".[140] He believed that

> transformation of symbolic expressions is most essential to physical science but it is in reality pure mathematics. Everything connected with the original [physical] question may be dismissed from the mind during those operations, and the mathematician to whom they are referred may be doubtful whether his results are to be applied to solid geometry, to hydrostatics or to electricity. But as we are engaged in the study of Natural Philosophy we shall endeavour to put our calculations into such a form that every step may be capable of some physical interpretation, and thus we shall exercise powers far more useful than those of mere calculations — the application of principles, and the interpretation of results.[141]

Through his teaching, Maxwell, like all those who before him promoted a mathematical physics, was thus contributing to the institutionalization of a practice of physics heavily connected with and dependant upon mathematics. But as the mathematics (the syntax) became more developed, Maxwell's wish to give a physical interpretation (a semantic) to every step of the calculations, a view strongly promoted by Whewell,[142] became progressively attenuated in the work of successive generations of physicists. Moreover, in addition to making possible abstraction and generalization, the manipulation of symbols discussed by Biot and Maxwell were to have an important if often undesired and disturbing ontological effect.

Albeit in a cryptic manner, Maxwell seems to have perceived the tendency toward more and more abstract kinds of explanations provided in physics when he told J. A. Fleming that "the progress of science was indicated by our making our terms mean less and less". By which he meant, according to Fleming in his Recollections of Maxwell, that "whereas older physicists talked of electric fluids, caloric, etc., we speak simply of electrification, heat, etc and decline to commit ourselves as to what electrification, etc., is".[143] As P. M. Harman has shown, Maxwell makes a "disjunction between the nature of substances and the framework of dynamical principles" and he asserts "the sufficiency of a purely symbolic or functional mode of representation".[144] The basis of this formal approach was the use of Lagrangian formalism, which as Joseph Larmor explained, allows one "to ignore or leave out of account altogether the details of the mechanisms, whatever it is, that is in operation in the phenomena under discussion".[145] Thus the use of more general and more abstract mathematical formulations played against the tendency to conceive matter in substantial terms, the emphasis being put on the relational aspects, which for Maxwell were "the most important things to know".[146] In other words, the syntactical structure of the theory affected and constrained the semantic interpretation of its terms.

The long history of mechanical explanations of gravity provides a good example of the decline of substantialist explanation.[147] For in contradiction to what Auguste Comte wrote in his *Cours de philosophie positive*, physicists did not abandon lightly their belief in a material medium explaining gravity.[148] On the contrary, we find numerous attempts over the course of the nineteenth century to provide a mechanical explanation that would get rid of action at a distance, an action that was in fact, to use Cassirer's vocabulary, simply a functional explanation of gravitational action. Well-known physicists like Faraday, William Thomson, as well as many other lesser known figures, like S. Tolver Preston and James Challis, filled numerous pages of the *Philosophical magazine* in the 1860s and 1870s with attempts at explaining gravitation mechanically. Interestingly enough, these essays, with the exception of Faraday's, were very mathematical in form, having incorporated the new definition of the legitimate physics, but were all unsuccessful at providing a coherent mathematical formulation. In 1900, H. A. Lorentz could still deem the subject worthwhile, noting that "every physicist knows Le Sage's theory", which postulated "ultramundane particles" colliding on planets and thus providing a mechanical explanation of gravitation.[149] However, the crucial point is that Lorentz approached the problem through mathematical analysis and concluded that a theory based on rapid vibrations of the ether as a cause of gravitation could not be accepted. With this verdict, he was again submitting this mechanical explanation to the acid test of a coherent mathematical formulation. Thus, at the turn of the twentieth century, such undertakings had completely lost their credibility within the physics community. Essays on this topic, very often written by engineers, were thereafter to be found in books and pamphlets not submitted to the gate keepers who limited the access to scientific journals to those who treated the legitimate problems of the discipline.[150]

Not long after having shown the impossibility of a substantial basis for gravitational action, the same Lorentz, commenting on the electromagnetic theory of the electron, observed that "by our negation of the existence of material mass, the negative electron has lost much of its substantiality".[151] The transformation of the concept of mass from Newton's definition as "quantity of matter" (and thus implicitly and intuitively independent of speed), to the electromagnetic and then relativistic concept of mass (and inertia) as a function of speed, clearly shows the active role mathematics played in this process of abstraction.[152] The electromagnetic ether, whose original role was to support waves (and explain their existence), also vanished after a century of unsuccessful efforts at its mathematization.[153] And once the central place of mathematics in the physical sciences became taken for granted, scientists could start wondering about "the unreasonable effectiveness of mathematics in the natural sciences", to use Eugene Wigner's expression, without bothering any more with the search for 'physical explanations' which occupied the minds of their eighteenth- and nineteenth-century forebears.[154]

CONCLUSION

In this paper, I have tried to show that the mathematization of physics had long-term social, epistemological and ontological effects on the discipline. A similar analysis could be made of the famous debate concerning the non-visualizability of quantum mechanical phenomena in the 1920s. One would then see that it was strictly analogous to the debate over vortices or the ether, for the disappearance of these substances had the effect of making gravitation and light propagation hardly *anschaulich*: their understanding depended essentially on mathematical formalisms.[155] Thus it is not very surprising that David Bohm, a strong advocate of a 'realist' (a better word would be 'substantialist') interpretation of quantum mechanics, wrote in the mid-1980s that "the current emphasis on mathematics has gone too far" and that "physics may have taken a wrong direction in giving so much emphasis to its formalism".[156] Though Bohm's views were very marginal at the time,[157] they remind us, in the end, that the question of the relationship between physics and mathematics is still being debated[158] and one could fruitfully follow its effects in contemporary physics.[159] And since there is no reason to think that these effects were limited to physics, the framework of analysis suggested here could be used to look at the effects of mathematics on other disciplines like chemistry and biology. From J. J. Sylvester and A. Cayley in the 1870s, who used advanced mathematics to understand molecules and isomers, to the emergence of quantum chemistry and mathematical biology, mathematics has had the tendency of redirecting the focus of inquiry towards the relational character of the elements, thus contributing to the transformation of concepts and practices.[160]

But only a more detailed analysis could show that the desubstantialization of matter was directly related to the mathematization process itself which distanced the meaning of the concepts from their original intuitive referents. Through their formal manipulation as mathematical symbols, concepts thus acquired a *relational* definition and lost their original substantial quality while gaining in generality.[161]

ACKNOWLEDGMENTS

This paper was written during my stay in Boston as a Fellow at the Dibner Institute for the History of Science and Technology in winter 2000. Special thanks are due to Jed Buchwald and Evelyn Simha for their hospitality at the Dibner. Many of my colleagues there and in the Boston area were generous enough to comment on a previous draft of the paper: Babak Ashrafi, Davis Baird, Mario Biagioli, Kenneth Caneva, Mordechai Feingold, Evelyn Fox Keller, Sam Schweber, Ana Simoes. I thank all of them very much, as well as my colleagues Raymond Fredette, Robert Nadeau, Claude Rosental and the two anonymous referees for their stimulating suggestions. Needless to say, I am solely responsible for the views presented here, and for the limitations and errors they may still contain. This research has been supported by a grant from the Social Science and Humanities Research Council of Canada.

REFERENCES

1. Aristotle, *Physics* II.2 194a 8–9.
2. W. R. Laird, "Patronage of mechanics and theories of impact in sixteenth-century Italy", in Bruce T. Moran (ed.), *Patronage and institutions: Science, technology, and medicine at the European court 1500–1750* (Rochester, 1991), 51–66.
3. There is a considerably large literature on the mixed sciences, but, to my knowledge, there is still no general account of its extension to fields other than the canonical ones (Astronomy, Optics, Harmonics) as suggested here. For the case of Galileo, see Peter Machamer, "Galileo and the causes", in R. E. Butts and J. C. Pitts (eds), *New perspectives on Galileo* (Dordrecht, 1978), 161–80; James G. Lennox, "Aristotle, Galileo, and 'mixed sciences'", in W. Wallace (ed.), *Reinterpreting Galileo* (Washington, D.C., 1986), 29–51; W. R. Laird, "Galileo and the mixed sciences", in Daniel A. Di Liscia, E. Kessler and C. Methuen (eds), *Method and order in the Renaissance philosophy of nature* (Aldershot, 1997), 253–70. More generally, see Paul Lawrence Rose, *The Italian renaissance of mathematics* (Geneva, 1975); Peter Dear, *Discipline and experience: The mathematical way in the Scientific Revolution* (Chicago, 1995); Jean-Marc Mandosio, "Entre mathématiques et physique: Note sur les 'sciences intermédiaires' à la Renaissance", in *Comprendre et maitriser la nature au Moyen Age: Mélanges d'histoire des sciences offerts à Guy Beaujouan* (Geneva, 1994), 115–38; Richard D. McKirahan, "Aristotle's subordinate sciences", *The British journal for the history of science*, xi (1978), 197–220; John J. Cleary, *Aristotle and mathematics* (Leiden, 1995); Gary I. Brown, "The evolution of the term 'mixed mathematics'", *Journal of the history of ideas*, lii (1991), 81–102; James W. Garrison, "Newton and the relation of mathematics to natural philosophy", *Journal of the history of ideas*, xlviii (1987), 609–27.
4. B. le Bovier de Fontenelle, "Sur l'utilité des mathématiques et de la physique et sur les travaux de l'Académie des Sciences", *Oeuvres complètes*, vi (Paris, 1994), 37–50; John Arbuthnot, "An essay on the usefulness of mathematical learning", in George A. Aitken (ed.), *The life and works of John Arbuthnot* (Oxford, 1892), 409–35; Boyle's 1671 essay "On the usefulness of mathematics to natural philosophy" is in vol. iii of *The works of the Honourable Robert Boyle*, ed. by Thomas Birch (London, 1744), 425–34. For Bacon's views on mathematics, see Graham Rees, "Mathematics and Francis Bacon's natural philosophy", *Revue internationale de philosophie*, xl (1986), 399–426.
5. For Boyle's views on mathematics see Steven Shapin, "Robert Boyle and mathematics: Reality, representation and experimental practice", *Science in context*, ii (1988), 23–58.
6. Cited by Antoni Malet, "Isaac Barrow on the mathematization of nature: Theological voluntarism and the rise of geometrical optics", *Journal of the history of ideas*, lvii (1997), 265–87, pp. 280–1. Barrow's Essay was first published in Latin in 1683. On the tradition of the mixed sciences in England, see J. A. Bennett, "Christopher Wren: Astronomy, architecture, and the mathematical sciences", *Journal for the history of astronomy*, vi (1975), 149–84.
7. Wallis to Oldenburg, 5 December 1668, in A. R. Hall and Marie B. Hall (eds), *The correspondence of Henry Oldenburg* (Madison and London, 1965–86), v, 221.
8. Huygens to Marquis de l'Hôpital, 29 December 1692, in C. Huygens, *Oeuvres complètes* (The Hague, 1888–1950), x, 354; the quotation from Seneca was "calculis ludimus, in supervacuis subtilitas tertur". On Huygens, see Joella G. Yoder, *Unrolling time: Christiaan Huygens and the mathematization of nature* (Cambridge, 1988).
9. Alan S. Shapiro (ed.), *The optical papers of Isaac Newton* (Cambridge, 1984), i, 86; cited by Allan Gabbey, "Newton's mathematical principles of natural philosophy: A treatise on 'mechanics'?", in P. M. Harman and Alan E. Shapiro (eds), *The investigation of difficult things* (Cambridge, 1992), 312–13.
10. I distinguish 'quantification' from 'mathematization'. The first refers to the production of numbers

for measuring phenomena through the construction of a metric (a graduated thermometer for example) whereas the second refers to the writing of abstract geometric or algebraic formulations (like the law of free fall or the law of refraction). The first can exist without the second and the latter can be formulated before the former.

11. For a useful survey of the traditional discussion of mathematization in physics, see H. Floris Cohen, *The Scientific Revolution: A historiographical inquiry* (Chicago, 1994). Koyré's view is presented in his classic paper "Galileo and Plato", published in 1943 and reprinted as chap. 2 of his *Metaphysics and measurement* (Cambridge, 1968). For a synthetic presention see Gérard Jorland, *La science dans la philosophie: Les recherches épistémologiques de Alexandre Koyré* (Paris, 1981).
12. See for example J. A. Bennett, "The mechanics' philosophy and the mechanical philosophy", *History of science*, xxiv (1986), 1–28; Dear, *op. cit.* (ref. 3); Mario Biagioli, *Galileo courtier* (Chicago, 1993).
13. For a general discussion of mathematization, see Salomon Bochner, *The role of mathematics in the rise of science* (Princeton, 1966); Enrico Bellone, *A world on paper* (Cambridge, Mass., 1982). A recent historical analysis of the role of mathematics in physics is provided by Elizabeth Garber, *The language of physics: The calculus and the development of theoretical physics in Europe, 1750–1914* (Boston, 1999). See also J. L. Heilbron, *Weighing imponderables and other quantitative science around 1800*, *Historical studies in the physical sciences*, Supplement to vol. xxiv/1 (1993). For case studies of mathematization, see Jed Z. Buchwald, "William Thomson and the mathematization of electrostatics", *Historical studies in the physical sciences*, viii (1977), 101–36; Norton Wise, "William Thomson's mathematical route to energy conservation: A case study of the role of mathematics in concept formation", *Historical studies in the physical sciences*, x (1979), 49–83. None of these works, however, emphasizes the *resistances* to mathematization.
14. John Henry, *The Scientific Revolution and the origins of modern science* (New York, 1997), 21.
15. Stephen M. Stigler, "Apollo mathematicus: A story of resistance to quantification in the seventeenth century", *Proceedings of the American Philosophical Society*, cxxxvi (1992), 93–126. Allen G. Debus, "Mathematics and nature in the chemical texts of the Renaisssance", *Ambix*, xv (1968), 1–28.
16. Yves Gingras, "La substance évanescente de la physique", *Actes du XX^{e} Congrès International d'Histoire des Sciences* (Liège, in press). It should be noted that by concentrating on what *mathematics* did to physics, I do not wish to suggest that the development of new instruments and new methods of experimentation did not also affect access to the practice of physics; that would be absurd. Although I cannot here develop this aspect of the transformation of the discipline, I do allude to it. As one reviewer suggested, one could also analyse what physics did to mathematics; but that of course would require writing a different, though complementary, paper.
17. Pierre Duhem, *The aim and structure of physical theory* (Princeton, 1954), 38–39.
18. For such an approach, see Andrew Cunningham, "How the *Principia* got its name: or, taking natural philosophy seriously", *History of science*, xix (1991), 377–92; see also his "Getting the game right: Some plain words on the identity and invention of science", *Studies in the history and philosophy of science*, xix (1988), 365–89. The author seems to suggest that it is historiographically 'illegitimate' to use words in ways that differ from how the authors themselves used them, as if words could have but a single meaning at any given time.
19. See for example Raymond Boudon, *The unintended consequences of social action* (New York, 1982).
20. On this see Cunningham, "How the *Principia* got its name" (ref. 18).
21. For a discussion of the changing content of 'physics', see Susan Faye Cannon, "The invention

of physics", chap. 4 of her book *Science in culture: The early Victorian period* (New York, 1978), 111–36; Thomas Kuhn, *The essential tension* (Chicago, 1977), 60–64; John L. Heilbron, "Experimental natural philosophy", in G. S. Rousseau and Roy Porter (eds), *The ferment of knowledge: Studies in the historiography of eighteenth-century science* (Cambridge, 1980), 357–87.

22. I think, for example, that much of the discussion about the 'non-existence' of the Scientific Revolution is based on a confusion of levels and categories of analysis.
23. For a recent discussion of that question see Nick Jardine, "Uses and abuses of anachronism in the history of the sciences", *History of science*, xxxviii (2000), 251–70. Though I essentially agree with the author, I would not use the term 'anachronism' to refer to the application of sociological categories to past events (p. 261). Since analytical categories are defined by analysts they need not have been part of the actor's repertoire. It is thus a category mistake to call them 'anachronistic' since they are not used as if they were actors' categories. Otherwise we would have to call anachronistic any explanations of past diseases invoking 'virus' or 'microbes' *before* the 'discovery' of these entities. I know of no historian ready to pay that price. And it should be clear that these categories do not prevent us from looking at how the actors themselves explained these diseases in the absence of the categories of 'virus' or 'microbes'. They are simply different levels of analysis.
24. Fatio to Huygens, 24 June 1687, in Huygens, *Oeuvres* (ref. 8), ix, 167–8.
25. On the concept of force, see Richard S. Westfall, *Force in Newton's physics: The science of dynamics in the seventeenth century* (London, 1971); Max Jammer, *The concept of force: A study in the foundations of dynamics* (Cambridge Mass., 1957).
26. Paul Mouy, *Le développement de la physique cartésienne, 1646–1712* (Paris, 1934), 144.
27. *Journal de Trévoux*, x (1710), 356.
28. On Varignon, see Michel Blay, *La naissance de la mécanique analytique: La science du mouvement au tournant des XVIIe et XVIIIe siècles* (Paris, 1992).
29. Jacqueline de la Harpe, *Le Journal des Savants et l'Angleterre, 1702–1789* (University of California Publications in Modern Philology, xx, no 6; Berkeley, 1941); Alfred R. Desautels, *Les Mémoires de Trévoux et le mouvement des idées au XVIIIe siècle, 1701–1734* (Rome, 1956); John N. Pappas, "Berthier's Journal de Trévoux and the philosophes", *Studies on Voltaire and the eighteenth century*, iii (1957), 13–63; Jean Ehrard and Jacques Roger, "Deux périodiques français du 18^e^ siècle: Le Journal des Savants et les Mémoires de Trévoux", in G. Bolleme *et al.* (eds), *Livre et société dans la France du XVIII^e^ siècle* (Paris, 1965), 33–59; C. Lennart Carlson, *A history of the Gentleman's Magazine* (Providence, 1938); Albert Pailler, *Edward Cave et le Gentleman's Magazine (1731–1754)* (Lille, 1975).
30. In his classic book *La formation de l'esprit scientifique* (Paris, 1938), Gaston Bachelard studied typical works of 18th-century science but only to contrast them with modern 19th- and 20th-century science. Completing this approach, I wish to follow the *process* of exclusion that transformed scientific practice and led to the situation described by Bachelard as "scientific" as opposed to what he called the "pre-scientific" spirit of the 18th century. It is obvious that I do not have to use these normative categories in order to describe that process. For a more detailed discussion of Bachelard's views in relation to our project, see Yves Gingras, "Mathématisation et exclusion: Socio-analyse de la formation des cités savantes", in Jean-Jacques Wunenburger (ed.), *Gaston Bachelard et l'épistémologie française* (Paris, in press).
31. For a detailed analysis of Leibniz's reaction to Newton's mathematization of natural philosophy, see Domenico Bertoloni Meli, *Equivalence and priority: Newton versus Leibniz* (Oxford, 1993). As he explains, Leibniz stressed "the insufficiency of purely mathematical laws [and] the need for physical explanations ..." (p. 24); see also Yves Gingras, "La dynamique de Leibniz: Métaphysique et substantialisme", *Philosophiques*, xxii (1995), 395–405. In the 1730s

Jean Bernoulli also devoted two prize-winning essays to the question of the physical cause of gravitation, trying to reconcile vortex motions with the mathematical laws of Kepler and Newton. For him, Newton's vacuum and attraction were "incomprehensible for a physicist" who had to "search the causes of the facts"; "Essai d'une nouvelle physique céleste" in *Opera omnia* (Geneva, 1742), iii, 266–7. For details, see William Shea, "The unfinished revolution: Johann Bernoulli (1667–1748) and the debate between the Cartesians and the Newtonians", William Shea (ed.), *Revolutions in science: Their meaning and relevance* (Canton, 1988), 70–92.

32. *Mémoires de l'Académie Royale des Sciences*, 1733, 311.
33. *Histoire de l'Académie Royale des Sciences*, 1733, 94.
34. Aristotle, *Metaphysics*, 995a, 15–18.
35. Cited by R. W. Home, "The notion of experimental physics in the early eighteenth-century", in J. C. Pitt (ed.), *Change and progress in modern science* (Dordrecht, 1985), 107–31, p. 124.
36. Isaac Newton, *Principia*, translated by I. Bernard Cohen and Anne Whitman (Berkeley, 1999), 381.
37. P. Collinson to C. Colden, 27 March 1747, in "The letters and papers of Codwallader Colden, iii: 1743–1747", *Collections of the New York Historical Society for the year 1919* (New York, 1920), 368.
38. Cadwallader Colden, *Principles of action in matter, the gravitation of bodies and the notion of the planets, explained of those principles* (London, 1751), preface. Also mentioned in C. Colden to Dr Betts, 25 April 1750, in "The letters and papers of Codwallader Colden, iv: 1748–1754", *Collections of the New York Historical Society for the year 1920* (New York, 1921), 204.
39. Cadwallader Colden, *Explication des premières causes de l'action de la matière et de la cause de la gravitation* (Paris, 1751). Since Colden was, through his relation with Franklin, in indirect contact with Abbé Nollet, the latter may have been at the origin of the translation.
40. *Gentleman's magazine*, December 1752, 499–500, 570–1, 589–90; and January 1753, 65–66.
41. Colden, *Principles of action in matter* (ref. 38), 3.
42. C. Colden to P. Collinson, 20 June 1745, in "The letters and papers of Codwallader Colden, iii" (ref. 37), 119. The Anglo-American fleet was then attacking the French settlement of the LouisBourg fortress which capitulated on 26 June.
43. Colden, *Principles of action in matter* (ref. 38), 2. A similar statement is also found in the 1745 edition, p. v.
44. *Ibid*., 3.
45. Alice M. Keys, *Cadwallader Colden: A representative eighteenth century official* (New York, 1906), 13–14.
46. Euler to Wetstein, 21 November 1752, *vera copia* in the letter from P. Collinson to Colden, 7 March 1753, in "The letters and papers of Codwallader Colden, iv" (ref. 38), 356.
47. Euler to Le Sage, 16 April 1763, in *Notice de la vie et des écrits de George-Louis Le Sage. Redigée d'après ses notes par Pierre Prévost. Suivie d'un opuscle de Le Sage sur les causes finales; d'extraits de sa correspondance avec divers savants et personnes illustres* (Geneva, 1805), 386.
48. On Euler's views on mechanical explanation, see Curtis Wilson, "Euler on action-at-a-distance and fundamental equations in continuum mechanics", in Harman and Shapiro (eds), *The investigation of difficult things* (ref. 9), 399–420.
49. Euler to Wetstein, *op. cit.* (ref. 38), 356.
50. On these exchanges, see "The letters and papers of Codwallader Colden, iv" (ref. 38), 378 (letter from Collinson, March 10, 1754,), 395–396 (Colden to Collinson, July 7 1753); the citation is

on p. 406 (Collinson to Colden Sept. 1, 1753). He seems not to have succeeded for I could find nothing on gravitation by Colden in the *Magazine* after the extracts from his book appeared in January 1753. In his letter to Colden on 10 March 1754, Collinson noted that "your answer to pro: Euler is not yett publis'd...", *ibid.*, 378.

51. Shapin, *op. cit.* (ref. 5), 42.
52. P. Varignon, *Nouvelles conjectures sur la pesanteur* (Paris, 1690). It is interesting to note that though historians have studied in detail Varignon's contribution to analytical mechanics, they pass over in silence this essay totally devoted to a mechanical explanation of gravity. Somehow in the 1690s, Varignon seems to have had a conversion to the mathematical approach and to have completely abandoned this project.
53. *Journal de Trévoux*, xlii (1742), 1093; George R. Healy, "Mechanistic science and the French Jesuits: A study of the response of the Journal de Trévoux (1701–1762) to Descartes and Newton", Ph.D. dissertation, University of Minnesota, 1956, p. 198, attributes, plausibly, the text to Castel, though it is not signed.
54. Père Louis Castel, *Vrai système de physique générale de M. Isaac Newton. A la portée du commun des physiciens* (Paris, 1743).
55. Cited by Bachelard, *op. cit.* (ref. 30), 230.
56. Cited by Donald S. Schier, *Louis-Bertrand Castel, anti-Newtonian scientist* (Iowa, 1941), 113.
57. J. Golinski, "Precision instruments and the demonstrative order of proofs in Lavoisier's chemistry", *Osiris*, n.s., ix (1994), 30–47 and *idem*, *Science and public culture: Chemistry and enlightenment in Britain, 1760–1820* (Cambridge, 1992), 138.
58. By contrast, the relatively wide distribution of X-ray apparatus at the end of the nineteenth century made that phenomena accessible to non-professional physicists, see Yves Gingras, "La réception des rayons X au Québec: Radiographie des pratiques scientifiques", in Marcel Fournier, Yves Gingras and Othmar Keel (eds), *Sciences et médecine au Québec: Perspectives sociohistoriques* (Sainte-Foy, 1987), 69–86.
59. Castel exchanged letters with Diderot on his *Lettre sur les aveugles*; see Schier, *op. cit.* (ref. 56), 48.
60. See John Pappas, "Lesprit de finesse contre l'esprit de géométrie: Un débat entre Diderot et d'Alembert", *Sudies on Voltaire and the eighteenth century*, lxxxix (1972), 1229–53; and more generally T. L. Hankins, *Jean d'Alembert: Science and the Enlightenment* (Oxford, 1970); Michel Paty, *D'Alembert* (Paris, 1998).
61. D. Diderot, "De l'interprétation de la nature", *Oeuvres philosophiques* (Paris, 1961), 177–244, p. 216.
62. See for example *ibid.*, 214 where he writes: "nos faiseurs de cours d'expérience ressemblent un peu à celui qui penserait avoir donné un grand repas parce qu'il aurait eu beaucoup de monde à sa table."
63. *Histoire de l'Académie des Sciences*, 1745, 28; cited by Pierre Brunet, *Les physiciens hollandais et la méthode expérimentale en France au XVIIIe siècle* (Paris, 1926), 132.
64. *Journal encyclopédique*, February 1769, 131.
65. J. R. D'Alembert, "Introduction aux recherches sur la précession des équinoxes et sur la nutation de l'axe de la Terre dans le système newtonien", in *Oeuvres complètes* (Geneva, 1967), i, 437–50, p. 437. On Buffon's critiques of mathematics, also published in 1749, see Jacques Roger, *Buffon* (Paris, 1989), 263–5.
66. D'Alembert, "Introduction", 450.
67. See for example *ibid.*, 344, 353.
68. *Ibid.*, 438.
69. D'Alembert, "Discours préliminaire ou analyse des recherches sur différents points importants du

système du monde”, in *Oeuvres complètes* (ref. 65), i, 349–91, p. 355.

70. See for example, *ibid.*, 352, 356, 358, 438; see also Michel Paty, “Rapports des mathématiques et de la physique chez D’Alembert”, *Dix-huitième siècle*, no. 16 (1984), 69–79.
71. On the diffusion of Newtonianism among the public, see Larry Stewart, *The rise of public science: Rhetoric, technology, and natural philosophy in Newtonian Britain, 1660–1750* (Cambridge, 1992).
72. Clairault to Euler, 19 June 1749, in Adolf P. Juskevic and René Taton (eds), *Correspondance de Leonhard Euler avec A. C. Clairault, J. D’Alembert et J. L. Lagrange* (Basel, 1980), 186. On the debate over the shape of the Earth, see John L. Greenberg, *The problem of the Earth’s shape from Newton to Clairault* (Cambridge, 1995). For a fascinating cultural history of this period see Elizabeth Badinter, *Les passions intellectuelles I: Désirs de gloire (1735–1751)* (Paris, 1999).
73. Halley to Newton, 29 June 1686, in H. W. Turnbull (ed.), *The correspondance of Isaac Newton*, ii: *1676–1687* (Cambridge, 1960), 443. Mordechai Feingold analysed the debate on the place of mathematics in the Royal Society in “Mathematicians and naturalists: Sir Isaac Newton and the Royal Society”, in Jed Z. Buchwald and I. Bernard Cohen (eds), *Isaac Newton’s natural philosophy* (Cambridge, Mass, 2000), 77–102.
74. Cited by Brunet, *Les physiciens hollandais* (ref. 63), 123.
75. M. Massière, *Réflexions critiques sur le système de l’attraction* (Nice, 1759), p. v.
76. *Ibid.*, p. viii.
77. *Ibid.*, p. x.
78. *Ibid.*, p. xvii.
79. *Ibid.*, 402.
80. Bernard-Germain Etienne de La Ville Cte de Lacépède, *Théorie des comètes pour servir au système de l’électricité universelle, suivie d’une lettre critique sur l’attraction* (London and Paris, 1784), 66.
81. J. Mangin, *Le tombeau de l’attraction universelle ou démonstrations incontestables de la fausseté du système de l’attraction newtonienne* (Verdun, 1826), 13.
82. Antoine-Louis Guénard Demonville, *Vrai système du Monde*, Part 2 (Paris, 1837), Avis de l’auteur.
83. Charles-François Viel, *De l’impuissance des mathématiques pour assurer la solidité des batimens et Recherches sur la construction des ponts* (Paris, 1805). For a general discussion of the debates accompanying the use of mathematics in the design of bridges, see Eda Kranakis, *Constructing a bridge: An exploration of engineering culture, design, and research in nineteenth-century France and America* (Cambridge, 1997); and Antoine Picon, *L’invention de l’ingénieur moderne: L’École des Ponts et Chaussées 1747–1851* (Paris, 1992), 76.
84. Heilbron notes, for example, that the Coffee House Physical Society “banned everything that smelt of mathematics”, Heilbron, “Experimental natural philosophy” (ref. 21), 364. In *Weighing imponderables* (ref. 13), 31, he notes that at the end of the 18th century, the German textbook-writer F. A. C. Gren, “who had been brought up in the older qualitative, inclusive natural science ... no doubt felt menaced by the calculators”. Other reactions are noted on pp. 147–9. For another example of an exchange over the appropriate use of mathematics in physics, see the discussion between Franz Ernst Newman and Ludwig Moser in Kathryn M. Olesko, *Physics as a calling: Discipline and practice in the Königsberg seminar for physics* (Ithaca, 1991), 93–95.
85. Pierre Bourdieu, “The scientific field and the social conditions for the progress of reason”, *Social science information*, xiv/6 (1975), 19–47. The formation of a field necessarily implies a form of “boundary work” concerning the proper attribution of domains and methods to ‘physics’, ‘mathematics’ or ‘applied mathematics’: including or excluding mathematical techniques in

the study of physical phenomena was a way of imposing the legitimate definition of a field as well as the legitimate methods of inquiry. On boundary work, see Thomas F. Gieryn, *Cultural boundaries of science* (Chicago, 1999).

86. Jean-Baptiste Biot, *Traité de physique expérimentale et mathématique* (Paris 1816), p. xi. On Biot, see Eugene Frankel, "J. B. Biot and the mathematization of experimental physics in Napoleonic France", *Historical studies in the physical sciences*, viii (1977), 33–72.
87. Heilbron, "Experimental natural philosophy" (ref. 21), 367–75.
88. J. D. Forbes to W. Whewell, 29 May 1831, cited by Crosbie Smith, "Mechanical philosophy and the emergence of physics in Britain", *Annals of science*, xxxiii (1976), 3–29, p. 25.
89. J. D. Forbes to W. Whewell, 8 August 1833, cited by Smith, *op. cit.* (ref. 88), 27. On Whewell's attitude towards the relation between physics and mathematics, see Harvey W. Becher, "William Whewell and Cambridge mathematics", *Historical studies in the physical sciences*, xi (1980), 1–48; Menachem Fish, "A philosopher's coming of age: A study of erotetic intellectual history", in Menachem Fish and Simon Schaffer (eds), *William Whewell: A composite portrait* (Cambridge, 1991), 31–66.
90. Faraday to Maxwell, 25 March 1857; P. M. Harman (ed.), *The scientific letters and papers of James Clerk Maxwell* (Cambridge, 1990), 548.
91. Faraday to Maxwell, 13 Nov. 1857; *ibid.*, 552, note 13.
92. Iwan Rhys Morus, "Different experimental lives: Michael Faraday and William Sturgeon", *History of science*, xxx (1992), 1–28.
93. M. Faraday, "On the conservation of force", *Philosophical magazine*, 4th ser., xiii, no. 86, April 1857, 225–39, p. 238.
94. E. Brücke, "On gravitation and the conservation of force", *Philosophical magazine*, 4th ser., xv, no. 86, February 1858, 81–90, p. 82.
95. Harman, *op. cit.* (ref. 90), 429, 671.
96. On the physics discipline, see Kenneth L. Caneva, "From galvanism to electrodynamics: The transformation of German physics and its social context", *Historical studies in the physical sciences*, ix (1978), 63–169; David B. Wilson, "Experimentalists among the mathematicians: Physics in the Cambridge natural sciences tripos, 1851–1900", *Historical studies in the physical sciences*, xii (1982), 325–71; Olesko, *op. cit.* (ref. 84); Christa Jungnickel and Russell McKormach, *Intellectual mastery of nature: Theoretical physics from Ohm to Einstein* (Chicago, 1986); Robert Silliman, "Fresnel and the emergence of physics as a discipline", *Historical studies in the physical sciences*, iv (1974), 137–62; R. Sviedrys, "The rise of physics laboratories in Britain", *Historical studies in the physical sciences*, vii (1976), 405–36; D. Kevles, *The physicists: The history of a scientific community in modern America* (New York, 1978); Yves Gingras, *Physics and the rise of scientific research in Canada* (Montreal and Kingston, 1991).
97. Gaston Bachelard, *L'activité rationaliste de la physique contemporaine* (Paris, 1951), 42.
98. James E. Mclellan III, *Science reorganized: Scientific societies in the eighteenth century* (New York, 1985), 360 (his chap. 7 deals with tighter control of access to membership in scientific academies); on specialized scientific journals, see *ibid.*, 257–9, and J. E. McLellan, "The scientific press in transition: Rozier's Journal and the scientific societies in the 1770s", *Annals of science*, xxxvi (1979), 425–49.
99. Pierre Brunet, *L'introduction des théories de Newton en France au XVIIIe siècle: Avant 1738* (Paris, 1931); A. J. Aiton, *The vortex theory of planetary motion* (London, 1972); Henry Guerlac, *Newton on the Continent* (Ithaca, 1981), chap. 3; Alexandre Koyré, *Newtonian studies* (London, 1965).
100. For an excellent discussion of changing notions of explanation in the history of physics, see Stephen Gaukroger, *Explanatory structures: Concepts and explanation in early physics*

and philosophy (Hassocks, 1978). For the case of mathematics, see Michael S. Mahoney, "Changing canons of mathematical and physical intelligibility in the later 17th century", *Historia mathematica*, ii (1984), 417–23.

101. For the different interpretations given by historians to the term 'mechanical philosophy', see Floris Cohen, *op. cit.* (ref. 11), 142–5. Here I mean by 'mechanical explanation' one that provides an efficient cause based on contact forces; two roads were open: if void was admitted, that force could be obtained through corpuscular interactions (as in Lesage's theory of gravitation); if void was excluded it could be through the action of a fluid (as in Euler's theory) or through the movement of corpuscules of different size filling all space as in Descartes's system. From this point of view, Newton's mathematization of gravitation was a *demechanization* of the world picture and not a mechanization as suggested by E. J. Dijksterhuis, *The mechanization of the world picture* (Princeton, 1986).
102. William R. Shea, "Descartes as critic of Galileo", in Butts and Pitt (eds), *New perspectives on Galileo* (ref. 3), 139–59.
103. R. S. Westman, "The astronomer's role in the sixteenth century: A preliminary study", *History of science*, xviii (1980), 105–47; for a detailed analysis of the sixteenth-century debate on the relation between natural philosophy and astronomy and on the nature of astronomical explanation, see N. Jardine, *The birth of history and philosophy of science: Kepler's "A defence of Tycho against Ursus" with essays on its provenance and significance*, rev. edn (Cambridge, 1988).
104. A. Koyré, *La révolution astronomique* (Paris, 1961), 364.
105. On Newton's style of mathematical physics, see I. Bernard Cohen, *The Newtonian revolution* (Cambridge, 1980); P. A. Kroes, "Newton's mathematization of physics in retrospect", in P. B. Scheurer and G. Debrock (eds), *Newton's scientific and philosophical legacy* (Dordrecht, 1988), 253–67.
106. This is a 'category mistake' from the point of view of the actors. For us of course, it is no longer a category mistake since we have accepted Newton's view of what physics is. The same applies to Kepler's "celestial physics".
107. *Journal des sçavans*, 2 August 1688, 154; on the first reviews of the *Principia*, see I. B. Cohen, "The review of the first edition of Newton's *Principia* in the *Acta eruditorium*, with notes on the other reviews", in Harman and Shapiro (eds), *The investigation of difficult things* (ref. 9), 323–53.
108. Cited by Schier, *op. cit.* (ref. 56), 199.
109. Note that the actors used the categories of "Cartesians" and "Newtonians" in a variety of ways. A common feature to all is, I think, that the first *must* involve a mechanical explanation of all phenomena in the sense defined above (ref. 101) while the other can content itself with a mathematical formulation and leave unanswered the question of the mechanical action.
110. On habitus, see Pierre Bourdieu, *The logic of practice* (Stanford, 1990).
111. Schier, *op. cit.* (ref. 56), 58.
112. *Ibid.*, 89.
113. Castel, *op. cit*. (ref. 54), 13.
114. *Ibid.*, 37. On Huygens's reaction to Newton's *Principia*, see Roberto De A. Martins, "Huygens's reaction to Newton's gravitational theory", in J. V. Field and Frank A. J. L. James (eds), *Renaissance and revolution: Humanists, scholars, craftmen and natural philosophers in early modern Europe* (Cambridge, 1993), 203–13.
115. Castel, *op. cit*. (ref. 54), 52.
116. *Ibid.*, 94.
117. *Ibid.*, 95, italics in the original.

118. *Ibid.*, 97.
119. *Ibid.*, 98–99.
120. *Ibid.*, 121.
121. *Ibid.*, 121.
122. *Ibid.*, 253.
123. *Ibid.*, 304.
124. *Ibid.*, 302.
125. *Ibid.*, 348.
126. D'Alembert, "Introduction aux recherches sur la précession des équinoxes et sur la nutation de l'axe de la Terre dans le système newtonien", in *Oeuvres complètes* (ref. 65), i, 437–50, p. 450.
127. D'Alembert, *Essai sur les élémens de philosophie*, in *Oeuvres complètes* (ref. 65), i, 115–348, p. 345.
128. *Ibid.*, 341.
129. B. le Bovier de Fontenelle, *Oeuvres* (Paris, 1996), vii, "Préface de l'éditeur", 377–82, p. 377.
130. *Ibid.*, 378.
131. Baron de Marivetz et Goussier, *Physique du monde* (Paris, 1780), v, 57, cited by Bachelard, *La formation de l'esprit scientifique* (ref. 30), 231.
132. D'Alembert, *op. cit.* (ref. 127), i, 341. This views recalls Max Planck's when he wrote: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it"; *Planck's own scientific autobiography and other papers*, transl. by F. Gaynor (New York, 1949), 33–34.
133. *Ibid.*, 346.
134. Diderot, *op. cit.* (ref. 61), 216.
135. Jean-Baptiste Biot, *Traité de physique expérimentale et mathématique* (Paris 1816), p. xii. On Biot, see Frankel, "J. B. Biot and the mathematization of experimental physics in Napoleonic France" (ref. 86).
136. Biot, *Traité* (ref. 135), p. xiv.
137. *Ibid.*, p. xxiii.
138. *Ibid.*, p. xv.
139. Harman (ed.), *op. cit.* (ref. 90), i, 671.
140. *Ibid.*, 671.
141. *Ibid.*, 672.
142. Fish suggests that, for Whewell, "the formal system employed should fully 'cash out' empirically", *op. cit.* (ref. 89), 45.
143. University College London, Fleming Coll. Ms Add 122/37. My thanks to Sungook Hong for giving me access to his notes taken from this manuscript.
144. P. M. Harman, *Metaphysics and natural philosophy: The problem of substance in classical physics* (Brighton, 1982), 145–6.
145. Cited by Harman, *ibid.*, 146.
146. Cited in *ibid.*, 132. On the meaning of the use of Lagrangian formulations, see Martin J. Klein, "Mechanical explanations at the end of the nineteenth century", *Centaurus*, xvii (1972), 58–82; Mario Bunge, "Lagrangian formulation and mechanical explanation", *American journal of physics*, xxv (1957), 211–18.
147. E. Cassirer, *Substance and function* (New York, 1953), provides a philosophical analysis of this process.
148. A. Comte, *Cours de philosophie positive* (Paris, 1968), ii, 340.

149. H. A. Lorentz, "Considerations on gravity", *Amsterdam Koninklijke Akaemie Physica*, ii, (1900), 559–74. On Le Sage, see Samuel Aronson, "The gravitational theory of Georges-Louis Le Sage", *The natural philosopher*, iii (1964), 53–74.
150. For the period from 1700 to 1900, very few proposed explanations of gravity were published in journals controlled by the physics community. I will later publish a more detailed history of the various attempts at a mechanical explanation of gravity. For a brief survey, see F. H. van Lunteren, "Gravitation and nineteenth-century physical worldviews", in P. B. Scheurer and G. Debrock (eds), *Newton's scientific and philosophical legacy* (Dordrecht, 1988), 161–73. Outside the field of physics, attempts at providing a mechanical explanation still goes on; for relatively recent examples, see Dewey B. Larson, *Beyond Newton: An explanation of gravity* (Portland, 1964); René de Puymorin, *L'origine de la gravitation* (Paris, 1975).
151. H. A. Lorentz, *The theory of the electron* (New York, 1953), 43; cited by M. Jammer, *Concepts of mass in contemporary physics and philosophy* (Princeton, 2000), 36.
152. For the conceptual evolution of the concept of mass, see Max Jammer, *Concepts of mass in classical and modern physics* (Cambridge, Mass., 1961) and his more recent *Concepts of mass in contemporary physics and philosophy* (ref. 151).
153. For a survey of these developments, see G. N. Cantor and M. J. S. Hodge (eds), *Conceptions of ether: Studies in the history of ether theories, 1740–1900* (Cambridge, 1981).
154. E. P. Wigner, "The unreasonable effectiveness of mathematics in the natural sciences", *Communications on pure and applied mathematics*, xiii (1960), 1–14. For more examples of such preoccupations by scientists and for a philosophical approach to the question, see Mark Steiner, *The application of mathematics as a philosophical problem* (Cambridge, Mass., 1998). Husserl also provided a philosophical analysis of the meaning of the mathematization of nature in Edmund Husserl, *The crisis of European sciences and transcendental phenomenology* (Evanston, 1970), 21–59.
155. Arthur I. Miller, "Redefining *anschaulichkeit*", in Abner Shimony and Herman Feshbach (eds), *Physics and natural philosophy* (Cambridge, Mass., 1982), 376–411; Daniel Serwer, "*Unmechanischer zwang*: Pauli, Heisenberg, and the rejection of the mechanical atom, 1923–1925", *Historical studies in the physical sciences*, viii (1977), 189–256.
156. David Bohm and F. David Peat, *Science, order, and creativity* (New York, 1987), 7, 9.
157. Bohm's view are now undergoing a revival; see Peter R. Holland, *The quantum theory of motion* (Cambridge, 1993), and Russell Olwell, "Physical isolation and marginalization in physics: David Bohm's cold war exile", *Isis*, xc (1999), 738–56.
158. For a recent *critique* of the lack of physical explanations in the modern mathematical approach to physics, see Daniel Athearn, *Scientific nihilism: On the loss and recovery of physical explanation* (Albany, 1994).
159. For very recent examples, see *Nature*, cccciv, issue of 2 March 2000, 28–29; *Science*, cclxxxvii, issue of 7 January 2000, 49–50.
160. See, for example, Karen Hunger Parshall, "Chemistry through invariant theory? James Joseph Sylvester's mathematization of the atomic theory", in Paul H. Therman and Karen Hunger Parshall (eds), *Experiencing nature* (Dordrecht, 1997), 81–111; Ana Simoes and Kostas Gavroglu, "Quantum chemistry *qua* applied mathematics: The contributions of Charles Alfred Coulson (1910–1974)", *Historical studies in the physical and biological sciences*, xxix (1999), 363–406, and *idem*, "Quantum chemistry in Great Britain: Developing a mathematical framework for quantum chemistry", *Studies in history and philosophy of modern physics*, xxxi (2000), 511–48; Giorgio Israel, "The emergence of biomathematics and the case of population dynamics: A revival of mechanical reductionism and darwinism", *Science in context*, vi (1993), 469–509.
161. Yves Gingras, "La substance évanescente de la physique" (ref. 16).