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Hylarie Kochiras

Janvier 2017

The author

Hylarie Kochiras, whose research interests in history and philosophy of science focus particularly upon Isaac Newton’s metaphysics and natural philosophy, is a Fondation Maison des Sciences de l’Homme, Fernand Braudel-IFER Postdoctoral Fellow at host institution École Normale Supérieure de Paris (Labex TransferS, Mathesis, République des savoirs (USR 3608), ENS-CNRS-Collège de France). Previously, she held fellowships at the Cohn Institute of Tel Aviv University, at Bucharest’s New Europe College, and at the Center for Philosophy of Science at the University of Pittsburgh. She was a visiting scholar at the University of Athens, and Visiting Assistant Professor at the University at Buffalo.

The text

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Informations et soumission des textes :

wpfmsh@msh-paris.fr

Fondation Maison des sciences de l’homme
190-196 avenue de France
75013 Paris - France

http://www.fmsh.fr
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Abstract

This article pursues two objectives through a close reading of Newton's 1713 General Scholium. First, it examines his relationship to the canonical mechanical philosophy, including his response to criticism of his own theory that that canonical philosophy's requirements motivated. Second, it presents an interpretation of Newton's own mechanical philosophy, glimpsed in draft material for the General Scholium: he takes the natural world to be a machine operating by causal principles that arise only within systems and that require mathematical methods because they fundamentally involve interdependent and thus co-varying quantities. Newton's realism about impressed forces links the two objectives examined.

Keywords

force, Isaac Newton, General Scholium, mechanical philosophy, machine

Le Scholium General de Newton et la philosophie méchanique

Résumé

Une lecture attentive du Scholium General de Newton permet de préciser deux points. En premier lieu, il est possible de préciser le rapport de Newton avec la philosophie mécanique canonique, y compris la réponse qu'il a donnée à ceux qui critiquaient sa théorie. En deuxième lieu, il est possible de comprendre ce qu'était la philosophie mécanique de Newton lui-même. D'après un manuscrit du Scholium Generale, il considérait que le monde naturel était une machine fonctionnant grâce à des principes causaux qui se présentent uniquement dans des systèmes et qui exigent des méthodes mathématiques car ils concernent des quantités interdépendantes et co-variantes. Le lien entre ces deux points est un réalisme quant aux forces imprimées.

Mots-clefs

force, Isaac Newton, Scholum General, philosophie méchanique, machine

Sommaire

Introduction

When Newton composed the General Scholium, which would serve to conclude the *Principia* from 1713 onward, his longstanding dispute with the orthodox mechanical philosophers had reached its most truculent phase. Among those philosophers, Leibniz was surely foremost in Newton's mind as he composed the Scholium, their dispute over who had first discovered the calculus having both embittered their personal relations, and intensified their substantive disagreements about natural philosophy and the method proper to it. Yet this group of theorists included "maintainers of vortices" generally, those who postulated whirlpools of matter as causing the celestial bodies to move in their characteristic paths. That meant Descartes, of course, whom the General Scholium clearly had in its sights, but also theorists like Huygens, whose Cartesianism had become quite dilute[1]. All of these philosophers, however diverse in other ways, were united in denying unmediated causal interaction between spatially separated bodies and in insisting instead that material contact action is the mechanism responsible for all changes of place. Their kind of mechanical philosophy may be called 'orthodox' in virtue of the dominance their notion of mechanism achieved[2]. From the moment the Principia's first edition was published in 1687, such theorists had criticized its gravitational theory for failing to employ what they deemed the only intelligible mechanism—and moreover, for failing to provide any physical mechanism at all. What they considered a vice, however, Newton considered a virtue. With his most famous words, *hypotheses non fingo*[3], he meant to commend the restraint he had shown in the face of inadequate evidence—while simultaneously implicating the orthodox mechanical philosophers in deep methodological error. Indeed, while the General Scholium is a text exceedingly rich in its scope, one of its chief aims was, undeniably, to assert the primacy of Newton's method, and with it his conception of natural philosophy itself, over that of his hypothesis-feigning critics.

This chapter examines Newton's relationship to the mechanical philosophy, as conveyed in the General Scholium. The mechanical philosophy of the orthodox kind, and Newton's relationship to it, will be one main component of my discussion. The next section therefore provides an overview of that philosophy, considering first the philosophies that Robert Boyle classified as mechanical, and then theorists who would make the requirement of an intelligible physical mechanism the masthead of their methodological dispute with Newton. Then, in section 3, I consider Newton's method, concluding that his dispute with the orthodox mechanical philosophers about the proper use of hypotheses may be seen in part as a dispute about whether mathematical necessity obtains in the physical world and furthermore overrides physical necessity there. After noting the reaction provoked by the Principia's first edition and by some events preceding the General Scholium, I turn directly to that text. Section 4 thus examines those parts of the General Scholium that respond to the orthodox mechanical philosophers. There, after noting some consequences he faced for having shone a spotlight upon method, I ask why Newton responds to his critics as he does, concluding that common goals and a shared realism provide the answer. The other main component of my discussion, addressed in section 5, will be a quite different mechanical philosophy—Newton's own. For according to my interpretation, Newton conceived of his own theory as a mechanical philosophy, though in a very different sense than that of the vortex theorists, and one of the drafts for the

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[1] The phrase 'maintainers of vortices' was written by Roger Cotes, editor of the *Principia's* second edition, in his letter to Newton of 28 March, 1713, concerning editorial matters.

[2] In using the term 'orthodox' to designate mechanical philosophies that prohibited unmediated action at a distance (and did so by insisting that material contact action is universally at work in local motion), I mean to acknowledge the existence of other sorts mechanical philosophies, not least Newton's. My usage is thus different from that of many commentators. Because the prohibition against unmediated distant action was so dominant, many commentators have invoked it when characterizing the mechanical philosophy generally, some examples being the following: J.E. McGuire (1972, p. 523 n. 2) once cited the belief that "contact action is the only mode of change" as a necessary condition uniting diverse conceptions of the mechanical philosophy; William Harper and George Smith (1995, p. 123); Margaret Osler (2000, p. 171) described the mechanical philosophy as one seeking to explain the phenomena (in terms of matter and motion without recourse to any kind of action-at-a-distance"; Andrew Pyle (2002, p. 181) characterized the mechanical philosophy negatively "in terms of the fourfold denial of (1) action at a distance, (2) spontaneity, (3) immanent or irreducible teleology and (4) incorporeal causes"; Andrew Janiak (2008, p. 53) characterized the prohibition as "a crucial norm of the mechanical philosophy (in all its guises)".

[3] Acknowledgements of the connection reach back to Cohen (1969); A.R. Hall (1980, p. 148).

General Scholium provides a cameo appearance of that sense.

The mechanical philosophy and its orthodox form

The backdrop to the mechanical philosophy

Before the rise of the mechanical philosophy, the two main traditions for understanding natural phenomena were the Aristotelian ideas carried forward from scholastic thinkers, and the vitalist theories, as they eventually came to be called, that had flourished during the Renaissance. Both persisted into the early modern period, meeting different responses from the theorists who looked to machines when trying to understand nature. Aristotelian ideas, including the notion of prime matter, the doctrine of hylomorphism, and teleological explanations, were the ones that those theorists typically set themselves against[4]. Vitalism, meanwhile, which postulated a vital principle, spirit, or soul as the source of life, did not provoke a comparable reaction. Even the most mechanically-minded philosopher would consider human beings categorically different from pulleys and clocks, and would accordingly admit the need to distinguish generally between animate and inanimate things. It is notable, especially in connection with Newton, that vitalist ideas might co-exist with those from other traditions, even for the same phenomena. Whereas Descartes had invoked vitalist ideas for human beings but mechanist ones for everything else, Newton did not compartmentalize his pursuits in that manner[5]. He continued to pursue alchemical and vitalist ideas in connection with phenomena that included gravitational effects, even as he developed a theory completely independent of those ideas. It was long after he had formulated his laws of motion and gravitational theory, for instance, that he mused in an unpublished text, "We cannot say that all nature is not alive"[6]. I will return briefly to Newton's vitalist ideas in a subsequent section.

The mechanism of material contact action and Boyle's mechanical philosophers

The notion of the mechanical philosophy has been strongly associated with material contact action in terms of its causal mechanism, and with Descartes and Boyle in terms of its practitioners[7]. It was Boyle who enthusiastically promulgated Henry More's term, 'the mechanical philosophy', using it interchangeably with 'the corpuscular

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[4] A couple of qualifications are in order here. The first concerns the manner in which these philosophers presented themselves in relation to the Aristotelians. Although the sort of criticism seen in a passage below, where Boyle dismisses substantial forms as "incomprehensible" were certainly common, some thinkers also saw reason to downplay the differences. Descartes took that tack, for instance, in seeking acceptance of his *Principles of Philosophy*, linking his explanations of matter to Aristotle: "In attempting to explain the general nature of material things I have not employed any principle which was not accepted by Aristotle and all the other philosophers of every age. So this philosophy is not new, but the oldest and most common of all. I have considered the shapes, motions and sizes of bodies and examined the necessary results of their mutual interaction in accordance with the laws of mechanics." (*Principles of Philosophy*, Part Four, §200; CSM vol. I, p. 286.)

[5] Amid the substantial secondary literature on vitalism and alchemy, a few sources especially pertinent to Newton may be noted. McGuire's influential article of 1968 examines Newton's active principles in connection with forces; B.J.T. Dobbs' 1991 book is considered by many the locus classicus, in virtue of being the most sustained examination of Newton's alchemical thought. Paula Findlen's 2000 article compares Newton's alchemical ideas to those of Kircher, while Lawrence Principe's article of the same year and volume compares them to those of Boyle. Karin Figala's article of 2002 considers the alchemical thought of Newton and those influential upon him to try to understand the *Opticks*' suggestion about the internal structure of aggregate bodies. Newman's works (1994; 2006) focus upon the alchemical ideas of Eirenaeus Philalethes, while also considering their influence upon Newton.

[6] Draft variant of what would become Query 31, dated to c. 1705 by McGuire. University Library, Cambridge (ULC) Add. 3970, fol. 620r. The text, written in English, is quoted in McGuire (1968, 171), with his discussion found in those same pages. McGuire dates the text, which he identifies the text as a draft variant of what appeared as Query 23 in the 1706 *Optice* and as Query 31 in the 1717/18 Opticks, to c. 1705. (The slash date is due to a change in custom for marking the new year. According to an older tradition, the new year did not begin until March 25th, with the Feast of the Annunciation. The newer practice, ushered in with the change from the Julian to the Gregorian calendar (a change made in England in 1752), was to take January 1st as the beginning of the new year. Texts written during the transitional period before this new custom had fully taken hold, and between January 1st and March 25th, are often dated with a slash between the years.

[7] In her 1952 article, "The Establishment of the Mechanical Philosophy", Marie Boas traced the concept of the mechanical philosophy back to ancient sources, but investigated the seventeenth century by focusing primarily upon Boyle. Other influential, mid-century accounts of the mechanical philosophy include Dijksterhuis (1961) and Westfall (1977). Recent literature on mechanical philosophies includes a number of sophisticated investigations, attentive to diverse conceptions and to continuities as well as differences with Aristotelian and other competing views; some of the works, including Roux (2013), are mentioned in other notes to this chapter.

philosophy'[8]. Boyle applied these terms to a range of theories, his list of mechanical philosophers including not only plenists but also some atomists, such as Gassendi[9]. He considered them unified by their attempts to explain natural phenomena via causal principles quite different from the Aristotelians' formal and final causes—causal principles that he designated via the slogan *matter and motion*. Those principles were, more specifically, the size, shape, motions, and juxtapositions of the parts of matter, along with their local motions; and he interchanged the terms 'corpuscular philosophy' and 'mechanical philosophy' because he considered the properties just noted to be the salient features of machines. "The Motion, Size, Figure, and Contrivance of their own Parts" may be called "the Mechanicall Affections of Matter," Boyle wrote in the preface to *The Origine of Forms and Qualities (according to the Corpuscular Philosophy)*, "because to Them men willingly Referre the various Operations of Mechanical Engines"; and those mechanical qualities suffice to explain nearly everything, he continued, that the Aristotelians "either left Unexplicated, or Generally referr'd, to I know not what Incomprehensible Substantial Formes"[10].

What exactly is the principle of motion that Boyle intends in his slogan? For many of his contemporaries, the only intelligible causal principle for local motion was pressure or impact at bodies' surfaces, such that spatially separated bodies would causally interact only through an intervening material medium. It was an efficient cause—an action that directly precedes and precipitates its effect—and one so familiar from observing billiard ball collisions and other common events that it seemed to require no explanation[11]. Many theorists concluded from this that material contact action was the sole causal principle at work in all local motion; these were, once again, the orthodox mechanical philosophers. Although Boyle does hedge on some points (often skirting questions about the void, for example), for the most part he expects material contact action to be the sole causal principle for local motion. In a text investigating attraction by suction he writes, "I have not, yet, observ'd any thing which shews attraction cannot be reduc'd to pulsion"[12].

Descartes was of course one of the plenists prominent on Boyle's list. His immensely influential physics is regarded as the paradigm mechanical philosophy[13], and the material contact action fundamental to it is often called 'Cartesian mechanism'. While that term is perhaps best avoided, since its sense of mechanism had atomist as well as plenist proponents, Descartes' identification of matter with extension along with the associated plenum underwrote the possibility that contact action is the sole causal mechanism involved in local motions; for a plenum ensures that between any non-adjacent bodies, a medium exists to effect their causal interaction. Like Boyle, Descartes was sanguine about the range of phenomena explicable by his material efficient causes. Having "considered the shapes, motions and sizes of bodies and examined the necessary results of their mutual interaction in accordance with the laws of mechanics", he explains, he has found that all phenomena separate from minds, including all of the "remarkable effects which are usually attributed to occult properties", have "purely

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[8] On the introduction of the term 'mechanical philosophy' and its diffusion, see Sylvia Berryman (2009, 244 n.40), who notes several clarifications of the history noted by Peter Anstey and by Alan Gabbey.

[9] Boyle's list appears in The Proemial Discourse to the Reader of the 1667 edition of *The Origine of Formes and Qualities* (Boyle, 1991, p. 10). Various scholars have commented upon Boyle's tactical goal of promoting harmony among the theorists he classified as mechanical philosophers, by emphasizing similarities in their views.   For a recent discussion of Boyle's list and of his tactical goal, see Garber (2013, §3 and §2, respectively).

[10] Robert Boyle (1666, Preface, pp. 16-17).

[11] As Locke writes in his Essay (II.xxiii.28), the communication of motion by impulse is familiar to us from experience, and yet the cause of it remains obscure. Henry More also acknowledged it as mysterious, and had only a metaphor to offer, suggesting that one body rouses the other, as if from sleep (More to Descartes, letter of 23 July, 1649, in Adam & Tannery, vol. 5, 383; translation by Gabbey,1990, 27-28). Leibniz, meanwhile, explains the communication of motion in terms of his living force, the *vis viva*.

[12] Boyle: "An Inquiry into the Cause of Attraction by Suction" (in Boyle, 1725, vol. II, 711).

[13] Roux makes the point by recalling the 1685 *Entretiens sur la pluralité des mondes* of Bernard le Bovier de Fontenelle (also the author of the first biography of Newton): "Thanks to a well-known parable that begins with an analogy between nature and an opera, Fontenelle lets his Marquise...note how philosophy became mechanical. When she inquires about the actors of this transformation, he unswervingly answers: 'Descartes and a few other Moderns.'" (Roux, 2013, p. 63; she cites Fontenelle, *Entretiens sur la pluralité des mondes*, in *Œuvres complètes*, vol. II, p. 21.) In the same article, Roux traces the narrowing of the field of innovators generally: "Until the middle of the seventeenth century, the lists of innovators are very long, and they include the authors who had attempted a reform of Aristotelianism from the end of seventeenth century on, mostly, but not only, in Italy; but after 1660, only a few great names remain, including, at the forefront, Descartes."(Roux, *ibid*., pp. 58-59)

corporeal causes"[14]. Magnetism, for instance, is not due to any sympathies or antipathies, but to the twisting of striated material particles[15]. And planetary motions are explained in terms of the vortex, all actions being effected by material contact; vortices of dense matter push the celestial bodies along in their characteristic paths.

Although the denial of unmediated distant action was sufficiently widespread that we can speak of the orthodox mechanical philosophy, there were nevertheless many mechanical philosophies, invoking mechanics and machines in different ways[16]. Furthermore, although the anti-Aristotelian aspects of the theories that Boyle listed as mechanical philosophies do render them a group of some sort, when compared to one another, their differences become more striking. One point of variability concerned the question of whether a true void is possible, for again, Boyle's list included atomists as well as plenists. Another variable concerned machines. In what sense, exactly, were they relevant for understanding the natural world? When Boyle looked to machines, what he noticed were physical properties of their material parts, such as shapes and sizes, along with the physical means by which motion was transferred among them. Others, however, were more struck by the proportions that were preserved through changes in correlated quantities, as in the law of the lever, and the mathematical forms in which the laws of mechanics were expressed. Huygens is notable here, for though he did insist upon contact action as a universal causal mechanism, he also expected theories and results to be guided by mathematical principles of mechanics. As for ontology, those on Boyle's list did agree in some important respects about secondary causation, that is, causes within the natural world, tending to explain phenomena in terms of efficient causation, carried out by the various properties of a uniform matter. Yet there were also exceptions, as with Gassendi, who explained cold and heat by positing calorific and frigorifick particles. Finally, one may note that method, though a great preoccupation for many early modern thinkers, was not a gatekeeper for Boyle when classifying philosophies as mechanical. What signified were explanations invoking matter and motion as the causal principles, not how the explanations were reached, as one sees in comparing Descartes' metaphysical foundation for physics to Boyle's approach, or at least, his approach when wearing his experimentalist hat. For like many others, Boyle often came close to designating intelligibility the marker of truth.

Subsequent developments in the mechanical philosophy

Two periods may be distinguished for the vortex theory's development and defense after Descartes. The first period is marked by attempts to respond to the problems inherent to his formulation of the theory. Since it assumed a cylindrical vortex, for example, Descartes' formulation had difficulty explaining why bodies fall to earth, implying instead that they should move toward the axis around which the fluid matter supposedly circulated[17]. The second period is marked by reactions to Newton's *Principia*. While it was not unreasonable to defend a vortex theory after 1687, even among those for whom the Principia was within reach[18], and while critics noticed flaws in

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[14] *Principles of Philosophy,* Part Four; CSM vol. I, §200, p. 286, and §187, p. 280.

[15] *Principles of Philosophy*; the remarks about occult qualities and sympathies are again from Part Four, §187 (CSM vol. I, p. 280); that concerning the striated particles is from Part Three; §93 (CSM vol. I, p. 262).

[16] In an article from 1972, McGuire identifies a variety of notions of mechanism, including the rejection of occult qualities; the view that experimental methods as well as first principles must be used when investigating nature; that nature must be conceived of dynamically; that it is governed by immutable laws; and a number of others. McGuire takes each of these to characterize some mechanical philosophy, but along with the requirement of contact action, which in that article he considers to be a necessary condition uniting all mechanical philosophies; see McGuire, 1972, p. 523, n. 2. Gabbey also provides an extensive list, which includes the following: theories explaining phenomena non-qualitatively, in terms of the motions and configurations of the parts of a uniform matter; those treating the universe and its component systems as machines; those aiming to mathematize representations of phenomena; those postulating necessary laws of nature and motion; and those theories excluding everything spiritual or immaterial from the investigative domain. (See Gabbey, 2002; pp. 337-38; also 2004, p. 15. Gabbey does not take these meanings to be unified by any shared necessary condition, notably contact action. Relatedly, in her discussions of the "old" and "new" philosophies, Roux emphasizes the fluidity of these categories, which varied by time and place (see Roux, 2013, esp. pp. 58-59).

[17] On the Cartesian vortex's shape, I am following H. Snelders (1989, p. 212). Newton would note the problem at various points, including when drafting his General Scholium. In "Draft A" (called "Manuscript A" by the Halls, in Newton, 1962, chapter 8) he writes, "Moreover I have not yet disclosed the cause of gravity nor have I undertaken to explain it since I could not understand it from the phenomena. For it does not arise from the centrifugal force of any vortex, since it does not tend to the axis of a vortex but to the centre of a Planet." (Newton, "Draft A" (MS. A) of the Scholium Generale, the Halls' translation; 1962, pp. 352).

[18] The difficulty of Newton's treatise was one significant factor in sustaining the vortex theory, even in England,

the treatment of fluid motion on which his attack on the vortex theory depended[19], the mathematics of Newton's inverse-square force were acknowledged as unassailable. Consequently, while the vortex theory would continue to be defended well into the eighteenth century, its formulations were increasingly marked by efforts to accommodate aspects of Newton's theory[20]. The two vortex theorists of particular interest, here, both early readers of his theory, were Huygens and Leibniz.

Huygens is especially interesting for understanding both Newton's own methods and the substantive aspects of his response to the orthodox mechanical philosophy. Newton's elder contemporary and a key figure in helping to establish the methods of mathematical physics[21], Huygens combined his mathematical methods and classical deduction[22] with the method of hypotheses (in contemporary terminology, the hypothetico-deductive method). The method had been described by others, including Jacques Rohault, who emphasized that the theorist must be prepared to admit a conjecture "absolutely false" should it be found "contrary to one single experience"[23]. Huygens, who pushed physics forward by employing mathematics in a way that Rohault had not and who is seen as having effected a transition certain Cartesians and Newton, describes the method in the preface to his *Treatise on Light*[24]. The theorist secures the path toward probabilistic knowledge, Huygens explains, by deducing detectable consequences from the physical hypothesis, by determining whether the phenomena correspond, and by seeking new evidence; "one can imagine and foresee new phenomena which ought to follow from the hypotheses which one employs"[25]. Like Rohault, Huygens held that the sort of mechanism figuring in the hypothesis would be an intelligible one, specifically, material contact action[26].

Huygens' theory of gravity was a sophisticated vortex theory that aimed to avoid some of the problems plaguing Descartes' formulation. He developed its fundamentals long before publishing it, drafting *De gravitate* in 1668 and participating in the following year's debate about gravity in Paris, where the anti-Cartesian mathematician Gilles Personne de Roberval argued for the causal claim that Huygens would later attribute to Newton: that an intrinsic, mutual attraction was most probable[27]. By the time that Huygens did finally publish his theory—as the 1690 *Discourse on the Cause of Gravity*, which he attached to his *Treatise on Light*—he had read the *Principia*, but that did not cause him to withdraw his allegiance to the vortex theory. On some points, he found Newton's results fully convincing; insofar as the mathematics went, he accepted the inverse-square force for the planetary orbits, and his earlier uncertainty about Kepler's first two rules was wiped away[28]. However, he also had some strong misgivings, and while some of his doubts concerned mathematical techniques, those of

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where a translation of Rohault's influential and far more accessible *Traité de la physique* came into use. The translator was a Newtonian—Samuel Clarke—who included no evaluative remarks of the vortex theory in the first, 1697 edition, but began to do so in later editions, with the 1710 edition presenting vortices as fictional devices. See E.J. Aiton (1972, pp. 71-72), who cites M.A. Hoskin (1962, p. 353, 357) on the several editions and Clarke's notes.

19. See Aiton (1972, pp. 110-113; 260); Smith, 2007, §7.

20. On these efforts and on the eventual decline of vortex theories, see Aiton (1972, chapters 9 and 10, respectively).

21. More detailed discussions of this and related issues may be found in Koyré (1953); Gabbey (1980); Yoder (1988); Snelders (1989); Harper and Smith (1995); Guicciardini (1999, esp. chapter 5); Smith (2002).

22. On Huygens' use of classical deduction, see Shapiro, 1989, p. 229-230.

23. Rohault (c.1618 - 1672); his *Traitè de physique*, 1696 [1671], p. 20 and 22; see also discussion in Aiton (1972, p. 68, which pointed me toward these passages).

24. I have drawn the point about Rohault's method, as compared to that of Huygens (and also his insistence upon an intelligible mechanism) from Aiton. Commenting upon the question of whether Rohault and Malebranche should be seen as the founders of mathematical physics, Aiton concludes in favor of Huygens: "The methodology of Rohault and Malebranche has inspired Mouy to claim these Cartesians as the true instigators of mathematical physics: *Scientia* (Bologna), 1930, 18: 233. Huygens seems to have a better claim, for he applied to his hypotheses not only the method of mathematics but mathematics itself." (Aiton, 1972, p. 68).

25. Huygens, Preface to *Treatise on Light*; trans. Silvanus P. Thompson, 1912 [1690]; pp. vi-vii.

26. It should be noted, however, that though Huygens does often cite the metaphysically-motivated criterion of intelligibility as favoring the vortex theory of gravity, that theory also helped point him toward some striking achievements, being instrumental to his conclusion about Saturn's ring for example. On that and other achievements motivating Huygens' confidence in the vortex theory, see Henk Bos (1972, p. 609).

27. For details on the participants and their positions in the debate, see Aiton (1972, p. 75); Snelders (1989, pp. 210-211).

28. Some theorists accepted the first and third laws, while doubting the second, but Huygens doubted both of the first two rules; see the editor's remarks in the Avertissement preceding Huygens' manuscript *Projet de 1680-1681, partiellement exécuté à Paris, d'un planétaire tenant compte de la variation des vitesses des planètes dans leurs orbites supposées elliptiques ou circulaires, considération de diverses hypothèses sur cette variation*; vol. 21, pp. 112-113. See also Russell (1964, p. 19).

particular interest here concerned the *Principia's* gravitational force[29].

Huygens expected a theory to designate a physical cause, in particular an intelligible one, as he made clear both early and late. In replying to critics at the 1669 debate in Paris, including Roberval, who had proposed attraction as the most probable cause, Hugyens explains, "I exclude attractive and expulsive qualities from nature because I seek an intelligible cause of gravity"; to attribute a body's fall to attractive qualities, he elaborates, would be tantamount to saying that it has no cause at all[30]. Much later, in 1690, the opening sentence of his *Discourse on the Cause of Gravity* again cites an intelligible cause as the quarry. What sort of cause does he consider intelligible? One possessing the qualities described by the orthodox mechanical philosophy: "To find an intelligible cause of gravity" he explains, one must suppose bodies to consist in "the same matter, to which one attributes neither a quality nor an inclination to approach one another, but instead only the different sizes, shapes and motions"[31].

Given his expectation that a theory provide a physical cause, it is not suprising that Hugyens interpreted Newton's remarks about attraction as constituting a claim about gravity's physical means of operation. And given that he expected the theory's physical cause to be intelligible, it is not surprising that he treated dismissively the claim that he believed Newton to be asserting—that which he had heard from Roberval years ago at the Paris debate, namely, that bodies have an intrinsic power enabling them to attract one another from a distance, without any intervening medium. Although Newton had included various caveats in hopes of forestalling such inferences, Hugyens was apparently unpersuaded, writing to Leibniz in 1690: "Concerning the cause of the tides given by Mr. Newton, I am not at all satisfied with it, nor with all of the other theories that he builds upon his principle of attraction, which to me seems absurd"[32]. Newton would not have escaped criticism, however, even if Huygens had taken his caveats at face value. For in that case Huygens would have complained that Newton had failed to provide any physical cause, intelligible or otherwise. That is to say, he would have made much the same complaint that he had made earlier, in reaction to Newton's 1672 paper on light and colors (as noted below)[33]. That was also the complaint seen in the *Journal des Sçavans*'s review of the *Principia*; because he had failed to provide a physical cause, Newton had not produced a natural philosophy but only a mechanics.

The other vortex theorist of particular interest here is of course Leibniz, who as Newton's eventual rival in the priority dispute was central to the polemical atmosphere in which the General Scholium was composed. Leibniz's natural philosophy is most closely aligned with that of other orthodox mechanical philosophers with respect to the causal principle of material contact action. While his highly innovative system put him at odds with other theorists on many points, he had no trouble praising the physics "which teaches that nothing is moved naturally except through contact and motion, and so teaches that...everything happens mechanically, that is, intelligibly"[34].

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[29] For a discussion of other objections, in particular that concerning Newton's means of representing force geometrically, see Guicciardini (1999, the end of §5.3 on p. 125; §5.5 and 5.6). Huygens' objections to some of Newton's ideas did not produce any ill will between them; see Snelders (1989, p. 215).

[30] "A la premiere objection je respond que i'exclus de la nature les qualitez attractiues et expulsiues parce que ie cherche une cause intelligible de la pesanteur, car il me semble que ce seroit dire autant que rien que d'attribuer la cause pourquoy les corps pesants descendent vers la terre." Huygens, *Réplique de Huygens du 23 octobre 1669 aux observations de Roberval et Mariotte; Débat de 1669 sur les Causes de la Pesanteur*: reprinted in vol. 19 of Oeuvres complètes; p. 642 (my translation and paraphrase). A much fuller discussion may be found in Aiton (1972, chapter 4, esp. pp. 76-78).

[31] "Pour trouver une cause intelligible de la Pesanteur; il faut voir comment il se peut faire, en ne supposant dans la nature que des corps qui soient faits d'une mesme matiere, dans lesquels on ne considere aucune qualité ni aucune inclination à s'approcher les uns des autres, mais seulement des differentes grandeurs; figures, & mouvements." Huygens, *Discours de la cause de la pesanteur, p. 129; as reprinted in Oeuvres complètes de Christiaan Huygens*, vol. 21, p. 451.

[32] «Pour ce qui est de la Cause du Reflus que donne Mr. Newton, je ne m'en contente nullement, ni de toutes ses autres Theories qu'il bastit sur son Principe d'attraction, qui me paroit absurde, ainsi que je l'ay desia temoignè dans l'Addition au Discours de la Pesanteur. Et je me suis souvent etonnè, comment il s'est pu donner la peine de faire tant de recherches et de calculs difficiles, qui n'ont pour fondement que ce mesme principe. Je m'accommode beaucoup mieux de son Explication des Cometes et de leur queues». (Huygens to Leibniz, 18 November 1690 ; Oeuvres, vol. 9, p. 538). See also discussions in Cohen (1980, p.81); Guicciardini (1999, p. 122); Smith (2007, §10).

[33] Huygens' criticisms included not only methodological and epistemological matters, including the use of hypotheses and the kind of knowledge one could expect within natural philosophy, but also some of Newton's scientific claims. There is of course a substantial literature about Newton's 1672 paper and the critical reactions to it.

[34] Leibniz, *Against Barbaric Physics: Toward a Philosophy of What There Actually Is and Against the Revival of the Qualities of the Scholastics and Cimerical Intelligences* ; *(c.1710-1716,*

As he details in his correspondence with Clarke, he ultimately grounds his causal principle of contact action and its supporting criterion, intelligibility, is his principle of sufficient reason.

Given the deep metaphysical roots of Leibniz's natural philosophy, he had no nagging doubts when repudiating what he understood to be Newton's principle of attraction[35]. He understood it as a causal principle, one that could be specified in only two possible ways, both unacceptable. One of the possibilities he saw was divine (primary) causation; writing to Nicolas Hartsoeker a couple of years before the *Principia's* second edition appeared), he charged that although the task at hand was "to find out a natural cause", Newton had taken refuge in a "miracle"[36]. The other possibility that Leibniz saw was an unintelligible natural (secondary) cause, a body's intrinsic power to attract other bodies across a void. As he wrote in his final letter to Clarke, he considered Newton's forces to be "attractions, properly so-called"; and he went on to charge that "those who assert these kinds of operations must suppose them to be effected by miracles, or else they have recourse to absurdities, that is, to the occult qualities of the schools, which some men begin to revive under the specious name of forces"[37].

Yet while repudiating attraction as something causally efficacious, Leibniz also strove to incorporate the inverse-square force insofar as it was mathematically demonstrated. Thus very shortly after the first edition of Newton's *Principia* appeared, Leibniz published his T*entamen de Motuum Celestium Causis* which, though open to multiple objections including its implications for the motions of comets, developed a vortex theory intended to accommodate Kepler's rules and Newton's inverse-square force.

Newton's approach to natural philosophy and his dispute with critics

Newton's goals for natural philosophy and the role of mathematical methods

While Newton was not expansive on questions about methodology for natural philosophy, he did discuss his goals and methods in various texts, most significantly in the *Principia*, but also in one of the *Opticks* queries and in letters and manuscripts[38]. In the final sentence of this passage from his Author's Preface, composed for the *Principia's* first edition and retained in subsequent editions, he specifies natural philosophy's goal as the discovery of impressed forces: "The basic problem [*lit.* whole difficulty] of philosophy seems to be to discover the forces of nature from the phenomena of motions and then to demonstrate the other phenomena from these forces", and presents mathematical principles of natural philosophy to facilitate that discovery[39]. For Newton, the forces that natural philosophy seeks are real causes, that is to say, the real movers in natural processes[40]. This stance can be seen by comparing the goal as formulated here in the Author's Preface to its analogue in one of the *Opticks* queries. In the query, just after having explained why vortices must be rejected, Newton reformulates natural philosophy's goal in explicitly causal terms: "the main Business of natural Philosophy is to argue from Phaenomena without feigning Hypotheses,

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as dated by Ariew and Garber); in Philosophical Essays, trans. Ariew & Garber; Hackett, 1989; pp. 312- 320; p. 312.

35. There is of course a substantial literature on Leibniz's reactions to Newton. Andrew Janiak's 2007 article is a recent, influential analysis, with particular attention to the question of how Newton can escape a dilemma about whether the force is real, which Leibniz sees him as facing.

36. Leibniz, Letter to Hartsoeker, published in *Memoirs of Literature*, 11 February, 1711; in Newton (2004, p. 109; on the translation, see p. xxxviii).

37. Leibniz to Clarke, his fifth letter, §113; 2000 [1717], pp. 62-62.

38. Needless to say, questions about Newton's methods for natural philosophy constitute a vast and difficult area of research. My limited aim here is to state some main features of his goals and methods, indicating their connection to his understanding of natural philosophy, especially the unity of that discipline with mechanics, as he explains in his Author's Preface. Newton's methodology has been investigated and explicated with great acuity by A. E. Shapiro, I.B. Cohen, William Harper, and George Smith, among others; Shapiro (1989); Harper and Smith (1995); and Smith (2002) in particular have been influential upon my discussion.

39. Newton, Author's Preface; *Principia* (1999; excerpts from pp. 381-383).

40. At various pounts, Newton indicates that he accepts real secondary causation. In one letter, for instance, he makes the point explicitly, writing, "Where natural causes are at hand God uses them as instruments in his works" (Newton to Burnet, 1680; in Newton, 1959-1971, vol. II, p. 334). In some other texts, he implicitly endorses real secondary causes, e.g. when remarking in Query 31 that God composes bodies "by the help of these [active] principles" (Newton, 1952, p. 401).

and to deduce Causes from Effects"[41]. Thus to "discover the forces of nature" is to "deduce causes from effects"[42]. Similarly, in the Principia's scholium on space and time, impressed forces are "the causes by which true and relative motions are distinguished"[43].

How exactly will the forces themselves, these real causes of natural phenomena, be found? Having cited his mathematical principles as the means of discovering them in the Preface, he subsequently distinguishes mathematical and physical perspectives on forces, and a related two-stage investigative procedure. In his explanatory remarks following the *Principia's* Definition 8, Newton explicitly distinguishes mathematical and physical points of view when considering forces, also warning his readers directly that he is restricting his attention to the former.

> I use interchangeably and indiscriminately words signifying attraction, impulse, or any sort of propensity toward a center, considering these forces not from a physical but only from a mathematical point of view. Therefore, let the reader beware of thinking that by words of this kind I am anywhere defining a species or mode of action or a physical cause or reason, or that I am attributing forces in a true and physical sense to centers (which are mathematical points) if I happen to say that centers attract or that centers have forces[44].

Motivating Newton's restricted focus is his two-stage procedure for investigating forces—a mathematical stage in which only the force's quantitative characteristics are sought, and then a subsequent, physical stage that aims to complete the description by finding its physical characteristics, including the physical means by which it is communicated[45]. In a scholium appearing later in the first book (that following proposition 69 in section 11), after having reiterated that the word 'attraction' will here denote any tendency to approach, whatever be the cause, he explains his two-stage procedure, which justifies ignoring physical questions for the time being.

> Mathematics requires an investigation of those quantities of forces and their proportions that follow from any conditions that may be supposed. Then, coming down to physics, these proportions must be compared with the phenomena, so that it may be found out which conditions [or laws] of forces apply to each kind of attracting bodies. And then, finally, it will be possible to argue more securely concerning the physical species, physical causes, and physical proportions of these forces. Let us see, therefore, what the forces are by which spherical bodies, consisting of particles that attract in the way already set forth, must act upon one another, and what sorts of motions result from these forces[46].

The first stage of investigation, then, is to discover an impressed force's mathematical law—the quantities figuring in it and and the functional relation among them—as Newton found the gravitational force to be directly proportional to mass, and proportional in the inverse square to distance. This investigative stage must precede the stage that seeks physical characteristics. For as the

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[41] Newton, Query 28 of the *Opticks*, pp. 343-344. The original Latin essay appeared as Query 20 in the 1706 Optice. The version of the essay that was published in the work's English translation, the *Opticks* of 1717/18, it appeared as Query 28. Query 28 of the *Opticks* is for the most part an English translation of the Latin Query 20, but does include some modifications. In particular, for the version published in 1717/18, as Query 28, Newton elaborates upon the nature of the very first cause by adding the phrase "which certainly is not mechanical". The passage in Query 28 is as follows:

> And for rejecting such a Medium, we have the Authority of those the oldest and most celebrated Philosophers of Greece and Phoenicia, who made a *Vacuum*, and Atoms, and the Gravity of Atoms, the first Principles of their Philosophy; tacitly attributing Gravity to some other Cause than dense Matter. Later Philosophers banish the Consideration of such a Cause out of natural Philosophy, feigning Hypotheses for explaining all things mechanically, and referring other Causes to Metaphysicks: Whereas the main Business of natural Philosophy is to argue from Phaenomena without feigning Hypotheses, and to deduce Causes from Effects, till we come to the very first Cause, which certainly is not mechanical; and not only to unfold the Mechanism of the World, but chiefly to resolve these and such like Questions.

[42] Interestingly, in the eighteenth century Émilie du Châtelet would substitute 'cause' for 'impressed force' when stating the second law; see Andrea Reichenberger (2016, pp. 185-188).

[43] Newton, *Principia*, 412.

[44] *Principia*, p. 408. Anti-realist intepretations of Newton's stance toward his forces, including that by Samuel Clarke, mentioned earlier (the fifth letter, §118-123 (2000 [1717], pp. 85-86; see also §110-116), looked to remarks such as this.

[45] George Smith has done a great deal to bring needed attention to this scholium and to explicate Newton's methodology in light of which is in fact the focus of his 2002 article; for his discussion of Newton's distinction between the physical and mathematical perspectives, see pp. 148-152. See also Janiak 2008, chp.3; the distinction figures centrally in his argument for a realist interpretation of Newton's force.

[46] *Principia*, Book 1, Section 11, Scholium following Prop.69; pp. 588-589.

strongest sort of necessity, mathematical necessity provides constraints upon other facts about the force. This first stage thus aims to discover what must be true, no matter how the force is physically realized; in that manner, Newton found that the elliptical orbit of a gravitating body requires an inverse-square force from a focus, regardless of how the force might be physically realized. The second stage of investigation, which occurs when "coming down to physics", tackles the question of how the force is physically realized, investigating the force's physical species, causes, and proportions. This second stage can begin for the gravitational force, since the first is complete. It would be premature, by contrast, to begin that stage for some other forces; with respect to magnetic force, for instance, though Newton's observations point toward an inverse-cube relation, he considers them too rough to yield any conclusion[47].

Until recently, Newton's distinction between the mathematical and physical perspectives on forces was often read in anti-realist terms, such interpretations first appearing during Newton's lifetime. In his fifth letter to Leibniz, for instance, Samuel Clarke asserted, "the effect itself, the phenomenon...is all that is meant by the words attraction and gravitation"[48]. And a few years later, Berkeley would assess the forces as being merely "useful for reasoning, and for calculations about motion and moving bodies", while opining that "in the case of attraction, it was used by Newton not as a genuine physical quality but merely as a mathematical hypothesis"[49].

Yet Newton repeatedly employs causal language that communicates a realist stance toward his forces, particularly the gravitational force[50]. The impressed forces discoverable by the *Principia's* mathematical methods will be incompletely described, to be sure, so long as the physical stage of investigation remains incomplete, but they can nevertheless be identified at the mathematical stage[51]. Each stage provides a different sort of information about a single, real force. This is perhaps most striking in the famous General Scholium passage (discussed further below), which pairs realism about the gravitational force, as identified by his treatise's law, with agnosticism about its physical basis. In nearly the same breath, then, Newton both declares his ignorance of gravity's cause,

> I have not yet assigned a cause to gravity.... I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses.

and yet presents the law of gravity found by his method as denoting the real mover of the planets and tides:

> The laws of motion and the law of gravity have been found by this method [of induction]. And it is enough that gravity really exists and acts according to the laws that we have set forth and suffices for all the motions of the heavenly bodies and of our sea"[52].

As will be seen later, Newton's reason for taking mathematical methods to be capable of and indeed necessary for identifying the forces of

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[47] Newton indicates these preliminary findings for the magetic force in *Principia*, Book 3, Prop 6, Corollary 5 (Newton 1999 [1726], p. 810).

[48] Clarke to Leibniz, the fifth letter, §118-123 (2000 [1717], pp. 85-86; see also §110-116. On Newton's multiple senses of the terms 'gravity' and 'gravitation', see McMullin (1978, pp. 59-61).

[49] Berkeley, §17 of "An Essay on Motion" (2008, p. 248 [*De Motu*, 1721]). Berkeley is writing some eight years prior to Newton's death. Earlier, in his correspondence with Leibniz, Samuel Clarke takes a similar stance, by treating Newton's gravitational force as referring only to gravitational effects. See Clarke's fifth letter in *Leibniz-Clarke*, 115. See also Andrew Janiak's discussion of Clarke's interpretation in *Newton as Philosopher*, 65-74.

[50] Although there is a long history of anti-realist interpretations of Newton's forces—including that of Samuel Clarke in his fifth letter to Leibniz (see 5.110-116, 5.118-123 of *The Leibniz-Clarke Correspondence*)—many commentators now defend a realist interpretation. Such commentators include: Stein (see for instance his observation that Newton takes forces to be identified by their laws; 1970, p. 270; 2002, pp. 288-289); Harper and Smith (1995; see in particular p. 121 about Newton's method for discovering causes, also pp. 128-129); Harper (2002; see in particular p. 72); Smith (2002, especially pp. 148-152; a realist interpretation is implicit in his illuminating explication of Newton's distinction between the mathematical and physical senses of force); and Janiak (2007; 2008, chapter 3, especially pp. 60-64; contesting those such as Clarke who took Newton to regard the law of gravity merely as a calculating device, Janiak defends a realist interpretation by arguing that for Newton, treating the gravitational force mathematically means showing that a physical quantity can be measured, via measurements of other physical quantities, namely, masses and accelerations.) As for my own view, I have consistently held a realist interpretation (see Kochiras, 2009, 2013).

[51] I have drawn the claim that a force is identified by its law from Stein (1970, p. 270; 2002, pp. 288-289) and Janiak (2008, p. 63). See also Smith (2002, p. 147-148).

[52] General Scholium, *Principia* p. 943; the translation is that of Cohen and Whitman, except that I have adopted Janiak's more literal translation (Janiak, 2007, p. 129) of *&ad corporum caelestium & mari nostri omnes sufficiat* (which Cohen and Whitman translate as "and is sufficient to explain all the motions of the heavenly bodies and of our sea").

nature themselves is directly connected to his own sense of a mechanical philosophy.

Hypotheses and Newton's dispute with his critics

In its broadest meaning, Newton's evidentiary sense of 'hypothesis' denotes any proposition not demonstrated from phenomena[53]; as he writes in the General Scholium, "Whatever is not deduced from the phenomena must be called a hypothesis"[54]. Certain complexities and diachronic developments aside[55], Newton's core ideas about hypotheses, remained quite stable over time, having first emerged publicly during the controversy that followed his 1672 paper on light and colors (communicated by letter to Oldenburg and published in *Philosophical Transactions*). In response to one of his critics—Ignatius Pardies, the Jesuit professor of mathematics in Paris—Newton explained, "hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments"[56]. One of the two core ideas here is that physical hypotheses have an important role to play in theory development, that of providing experiments for investigation[57]. The other is a prohibition, meant to secure a theory's status as knowledge: no (non-mathematical) proposition may properly be asserted within natural philosophy unless it has been derived from phenomena, thus no claim may be asserted so long as it retains the status of a hypothesis. Since metaphysical hypotheses are by nature incapable of being derived from phenomena, they should never be asserted within natural philosophy, and much later, Newton would cite this in response to one of Leibniz's attacks. "His arguments against me are founded upon metaphysical & precarious hypotheses & therefore do not affect me: for I meddle only with experimental Philosophy"[58].[59] As for physical claims, they may be asserted only once the evidence has elevated them from mere hypothesis to knowledge.

The paper prompting the aforementioned controversy and remarks presented the research that Newton had conducted during the previous decade, research that would overturn the consensus view in various formulations since ancient

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[53] While Newton usually intends the evidentiary sense, he does acknowledge a loose sense of the term 'hypothesis', one used to denote any philosophical proposition without reference to its evidentiary credentials, as may be seen in his reaction to Pardies' second letter: "A practice has arisen of calling by the name hypothesis whatever is explained in philosophy; and the reason of my making exception to the word, was to prevent the prevalence of a term, which might be prejudicial to true philosophy."(Newton, "Mr. Newton's Answer to the Foregoing Letter", Translated from the Latin; *Philosophical Transactions*, No. 85, p. 5014. 1672; reprinted in Newton 1978, Latin original on p. 109.)

[54] Newton, General Scholium, *Principia*, 1999 [1726], p. 943. The sentence belongs to an addition that Newton sent to Cotes on March 28, and his letter further clarifies hypotheses as follows: "And the word Hypothesis is here used by me to signify only such a Proposition as is not a Phaeomenon nor deduced from any Phaenomenon but assumed or supposed without any experimental proof." (Newton, letter to Cotes of 28 March, 1713, in in Newton 1850, ed. Edelston, pp. 154-155). William Harper (2002, p. 78) points to this remark when emphasizing that Newton understands hypotheses as including both propositions deduced directly from phenomena, and generalizations reached inductively from those propositions.

[55] Long ago, Cohen took to task those who "lump together what Newton wrote on any subject", and in particular those who take "Hypotheses non fingo" as describing Newton's position on hypotheses throughout his life (see Cohen, 1969, esp. pp. 306-308). Cohen called attention to Newton's 1672 remarks (discussed in this section) about hypotheses' role in furnishing experiments and also in explaining the properties of things, after those properties have been found via experiment (see also Cohen, 1958). Cohen was not alone in calling attention to such claims (see, e.g., Kuhn, 1958), and there has been much more attention to them since.

[56] Newton letter of 10 June, 1672 to Oldenburg, for Father Ignatius Pardies: "Mr. Newton's Answer to the foregoing Letter, No. 85, p. 5014. Translated from the Latin" (in P*hil. Trans.*, 85, p. 5014; reprinted in Newton, 1978, p. 106, Latin original, p. 99).

[57] Near twins of Newton's 1672 remark—that (physical) hypotheses may be used for furnishing experiments—continue to appear by the time of the General Scholium and beyond. He would reaffirm the role, for instance, in his 1715 Account of the *Commercium Epistolicum*: "In this Philosophy Hypotheses have no place, unless as Conjectures or Questions proposed to be examined by Experiments." Similarly, when explaining the purpose of his *Opticks*' queries in a 1719 letter to Fontenelle, he wrote that he does not treat hypotheses as *scientia*, but per *modum questionum*. (Newton's Letter to Fontenelle of Autumn 1719, explaining the Queries in the second edition of Optice, which appeared the same year. Isaac Newton, *Correspondence* vol. 7, p. 72. Although hypotheses figure in the queries, Newton opts for the term 'query'. (For discussion, see Cohen, 1969, p. 321; McMullin, p. 293).

[58] U.L.C. MS Add. 3968, a draft for his Account of the *Commercium epistolicum Collinii & aliorum, De analysi promota*, in Cohen (1980, p. 61, note p. 302).

[59] Kuhn sees metaphysical hypotheses as playing an important creative role in Newton's theory development; see Kuhn (1958, esp. pp. 43-44). It may also be noted that even the metaphysical principles that matter is passive and that causation is local could play an unofficial role of motivating a continued search for some physical cause of gravity (other than an internal power to attract from a distance), one consistent with Newton's method. For since inductive methods do not indicate any clear point at which to conclude that a sought after entity most probably does not exist, there is room for metaphysical principles to motivate the search (see Kochiras, 2011, the end of §4, p. 174).

times, the modification theory. According to that theory, white light was pure, while the colors discernible in a rainbow or upon passing light through a prism were modifications of that pure light; thus the view implied that each ray of white light would have the same index of refrangibility. Based upon his experiments, however, including his *experimentum crucis*, Newton concluded that in fact, "Colors are not *Qualifications of Light... but Original and connate properties*....To the same degree of Refrangibility ever belongs the same colour", and vice versa. Light, he explained, is "a *Heterogeneous mixture of differently refrangible Rays*", and rather than being pure, whiteness is generated from this "confused aggregate of Rays indued with all sorts of Colors"[60].

While Newton's paper provoked a range of objections, a significant part of the controversy concerned both the use of hypotheses, and the presumption that contributions to natural philosophy should concern a phenomenon's physical nature and basis[61]. That presumption is most evident with Huygens, who considered Newton's doctrine problematic because it not only failed to provide a mechanical explanation of color phenomena but even aggravated the difficulty of doing so: "For my part, I believe, that an Hypothesis, that should explain mechanically and by the nature of motion the Colours *Yellow* and *Blew*"[62]. Huygens furthermore held that while the discovery about diverse refrangibilities was important, Newton had failed to address the fundamental question: "Till he hath found this [mechanical] *Hypothesis*, he hath not taught us, what it is wherein consists the nature and difference of Colours, but only this accident (which certainly is very considerable,) of their different *Refrangibility*"[63]. Hooke, meanwhile, criticized Newton's suggestion that light is corpuscular; and Pardies claimed, among other things, that the modification theory could after all explain the elongated spectrum that Newton's experiments had produced[64].

In his responses, Newton explained the difficulty he saw in treating mere hypotheses as truths, and he concomitantly tried to broaden the domain of properties that natural philosophy might legitimately seek beyond physical ones. With respect to those critics demanding a mechanical explanation, he reasoned that anyone bent upon a particular physical explanation, mechanical or otherwise, for a given phenomenon, would be blind to its fundamental features—features identifiable in abstraction from all particular physical explanations. It was easy, he explained to his critics, to generate multiple mechanical hypotheses; and the fact that all were "in some measure capable" of explaining a given phenomenon only revealed their dubious status[65]. He made the point to Pardies both by implication (listing recent mechanical hypotheses by various theorists, to show how cheaply they could be had), and also directly: "If the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous

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[60] "A Letter of Mr. Isaac Newton, Professor of the Mathematicks in the University of Cambridge; containing his New Theory about Light and Colors; sent by the Author to the Publisher from Cambridge, Feb. 6, 1671/72, in order to be communicated to the R. Society" (*Philosophical Transactions*, Number 80, February 19, 1671/72, pp. 3075-3087 / reprinted in *Papers and Letters*, ed. Cohen, 1958; pp. 47-59); p. 3081/p.53; p.3079/p.51; and p.3083/p.55.

[61] These points have been much remarked and discussed by others. See in particular Harper and Smith (1995, p. 119) on Huygens' view that a mechanical explanation was the proper goal, and his consequent criticism of Newton's doctrine of light and colors.

[62] As mentioned earlier, with this remark Huygens demands an explanation that is not only mechanical in the sense of contact action but also grounded in the mathematical science of motion. (See Shapiro's discussion; 1989, p. 233; once again, p. 233 is also pertinent).

[63] Huygens: "An Extract of a Letter lately written by an ingenious person in Paris, containing some Considerations upon Mr. Newtons doctrine of Colors, as also upon the effects of the different Refractions of the Rays in Telescopical Glasses" (reprinted in Newton, 1978, p. 136-137.

[64] Unlike previous theorists, who placed the reflecting surface close to the prism and seen only a circle of white light that shaded toward red on one side and violet on the other, Newton placed the surface over twenty feet distant from the prism, producing "the vivid and intense colors... in an *oblong* form; which, according to the received laws of Refraction, I expected should have been *circular*" (A Letter of Mr. Isaac Newton, Professor of the Mathematicks in the University of Cambridge; containing his New Theory about Light and Colors; sent by the Author to the Publisher from Cambridge, Feb. 6, 1671/72, in order to be communicated to the R. Society" (*Philosophical Transactions*, Number 80, February 19, 1671/72, pp. 3075-3087 / reprinted in *Papers and Letters*, ed. Cohen, 1958; pp. 47-59); p. 3076 / p. 48.) In his letter of 9 April, 1672, Pardies held that the modification theory could explain the oblong spectrum, as the effect of multiple sunrays striking the prism at different angles of incidence (an effect Newton had already taken into account). (See Pardies, "Some Animadversions on the Theory of Light of Mr. Isaac Newton", Latin original and English translation published in *Philosophical Transactions* No. 4087; reprinted in Newton, 1978, p. 87.)

[65] The phrase appears in Newton's letter of 11 June 1672, responding to Hooke: "Mr. Isaac Newtons Answer to Some Considerations upon his Doctrine of Light and Colors; which Doctrine was printed in Numb. 80 of these Tracts" (in Newton, 1978, pp. 116-135. p. 118-119). Once again, all of these passages have been much discussed by others.

hypotheses may be devised, which shall seem to overcome new difficulties"[66]. *Pace* Huygens, then, finding a mechanical hypothesis was not at all what was fundamentally important; producing such hypotheses is "no difficult matter", Newton wrote, and "to examine how Colors may be explain'd *hypothetically*, is besides my purpose"[67].

With respect to his earlier suggestion that light is corporeal, Newton emphasized to Hooke that he had intended the suggestion tentatively; in other words, it was a mere hypothesis and had not meant to assert it. Much more importantly, he emphasized, that suggestion was fully separable from his doctrine[68]. In finding that each "prismatic color" had its unique index of refraction, he had demonstrated a property of light whose truth could be recognized independently of any particular physical explanation or hypothesis. How did Newton claim to have achieved this? As he explained to Hooke, it was by setting aside all physical hypotheses: "I chose to decline them all, and to speak of *Light* in *general* terms, considering it abstractly". What was it to consider light in these general, abstract terms? It was not to consider it as a body, or a power, or as having any particular constitution at all. To consider it abstractly was to think of it as stripped down to its structural properties: "as something or other propagated every way in streight lines from luminous bodies, without determining, what that Thing is"[69]. Or similarly, as Newton explained for Pardies, he understood light as "any being or power of being, (whether a substance or any power, action, or quality of it, which proceeding directly from a lucid body, is apt to excite vision". And he accordingly understood rays of light as "its least or indefinitely small parts, which are independent of each other"[70]. Once identified in terms of its source-type (luminous bodies) and effects (its tendency to excite vision), it could be considered in terms of its structure or configuration (its propagation in straight lines in all directions from the source).

In analyzing Newton's first papers on light and colors, some commentators have concluded that Newton's method was quite similar to Huygens' hypothetico-deductive method, except that Newton tried to formulate his final theory in abstract terms and without any hypotheses[71]. I would add an observation that follows upon a point I made earlier. To treat the mechanism of contact action as indefeasible, demanding a mechanical explanation before all else, is to employ the hypothetico-deductive method within a framework that is itself ungoverned by that method. For all his rigor elsewhere, Huygens too treated the contact action hypothesis as indefeasible. In doing so he, like others, treated the causal mechanism of contact action as physically necessary, and treated physical necessity as the strongest sort of necessity within the natural world. Newton, by contrast, consistently treated mathematical necessity

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[66] Newton's letter of 10 June, 1672 to Oldenburg, for Father Ignatius Pardies: "Mr. Newton's Answer to the foregoing Letter, No. 85, p. 5014. Translated from the Latin." (in Phil. Trans., 85, p. 5014; reprinted in Newton, 1978, p. 106, Latin original, p. 99).

[67] Newton's letter to Oldenburg of 3 April 1673, responding to Huygens: "Mr. Newtons Answer to the foregoing Letter further explaining his Theory of Light and Colors, and particularly that of Whiteness; together with his continued hopes of perfecting Telescopes by Reflections rather than Refractions"; in Newton, 1978, p. 143.

[68] In suggesting that light is corporeal, Newton invoked the concepts of substance and quality, and the relation of dependence between them, reasoning that (i) only substances are bearers of qualities; (ii) colors have been found to be qualities; (iii) so light must be a body (which is a substance) rather than a power or a wave (the latter being a quality of some medium), for to say it is a wave would imply colors to be qualities of waves, which is to say qualities of qualities, something incoherent given that only substances are bearers of qualities. See Newton, *ibid.* (1672, p. 3085; in Newton, 1978, p. 57:

> "These things being so, it can be no longer disputed, whether there be colors in the dark, nor whether they be the qualities of the objects we see, no nor perhaps, whether Light be a Body. For, since Colours are the *qualities* of Light, having its Rays for their intire and immediate subject, how can we think those Rays qualities also, unles one quality may be the subject of and sustant another; which in effect is to call it *substance*."

[69] Newton's letter of 11 June 1672, responding to Hooke: "Mr. Isaac Newtons Answer to Some Considerations upon his Doctrine of Light and Colors; which Doctrine was printed in Numb. 80 of these Tracts" (in Newton, 1978, pp. 116-135. p. 118-119. Noticing phrasings in a draft version that indicate Newton's mathematical way of conceiving his doctrine, Shapiro tends toward a strong interpretation: "In a draft of this letter Newton originally wrote 'theoremes' for 'Properties', and after 'in generall termes' he continued 'after the mode of Mathematicians'; UCL MS Add. 3970, f. 433v. These phrases clearly show Newton's tendency to identify his experimental theory with mathematics."(Shapiro, 1989, note 44, p. 245)

[70] Newton letter of 10 June, 1672 to Oldenburg, for Father Ignatius Pardies: "Mr. Newton's Answer to the foregoing Letter, No. 85, p. 5014. Translated from the Latin." (in *Phil. Trans.*, 85, p. 5014; reprinted in Newton, 1978, p. 106; Latin original, p. 99).

[71] See analyses by Shapiro (1989, esp. pp. 242-242); Snelders (1989, p. 219); it is Shapiro who emphasizes that Newton sought to formulate theory in abstract terms and who, as indicated in earlier notes, argues that he thereby compromised his theory's intelligiblity.

as being prior to physical necessity. He did not object to the hypothetico-deductive method in itself, and his practice as well as his remark about hypotheses' role of furnishing experiments point to his own use of something similar. But he objected to the manner in which his critics employed the method, for he expected physical hypotheses to be treated as such, and moreover in a way that acknowledged the priority of mathematical necessity. In connection with this, one can discern in Newton's remarks to Pardies a preliminary version of the two-stage procedure for investigating forces that he would much later describe in the *Principia*. In the letter for Pardies, the first stage is to investigate the properties of things, conceived abstractly, apart from any physical conception; in the case of light, it can be conceived as something that proceeds from a luminous body in all directions in straight lines, without determining whether it is a body, power, or wave. The second stage, as seen in that letter, is to proceed slowly to hypotheses, to explain those properties. Considered in connection with the surrounding remarks about hypotheses, that second stage might appear to be little more than an indulgence allotted to those seeking visualizable explanations. Considered apart from those other remarks about hypotheses, however, the second stage mentioned in the letter to Pardies looks much more like an investigation able to add substantive knowledge. And considered in connection with the *Principia*, it looks like a step toward the second stage of the two-stage procedure that Newton outlines for investigating forces. By the time of the *Principia*, what Newton mentioned in his letter to Pardies has been developed in connection with forces, the first stage having become that of determining a force's mathematical proportions and the second having been developed into a search for its physical aspects.

Reactions to the Principia's first edition and some events preceding the second edition

As Newton composed the *Principia*, the previous decade's controversy over his first paper had its effect—as did the chance that the Principia might face a similarly fractious reception. His treatise had found that the heavens are nearly empty of matter, and further that "the hypothesis of vortices can in no way be reconciled with astronomical phenomena and serves less to clarify the celestial motions than to obscure them"[72]. Those conclusions raised the possibility that unmediated distant action was the physical agent of his inverse-square force. Newton tried to forestall criticism, emphasizing in some of the earlier-quoted passages that his treatise considered forces only from a mathematical point of view, not a physical one.

He also responded to ongoing and anticipated disputes by suppressing some parts of his manuscript—including that part he had written "in popular form, so that it might be more widely read", which he replaced with a mathematical version designed to exclude readers unable to grasp his principles or to "lay aside the preconceptions to which they have become accustomed"[73]. Among the suppressed texts was the manuscript known as the Conclusio, a conclusion that Newton composed for the *Principia* in 1686 or early 1687; with his decision to omit it, his treatise was published without any conclusion[74]. The *Conclusio* had focused largely upon hypotheses, and with the explicit aim of encouraging their investigation. It set forth physical hypotheses about short-range, interparticulate forces that Newton thought might be responsible for a range of phenomena, including sensation; heat transfer; the cohesion of particles into aggregate bodies; and the varying arrangements that those component particles might take, with such variations underwriting the differing densities in aggregate bodies and thus the different phases (forms) that a uniform matter could take, as solid, liquid, or gas.

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[72] Principia, book 2, section 9 (1999 [1726], p. 790). Other summary remarks of the difficulties facing the vortical theory include that in his 1693 letter to Leibniz ("The heavens are to be stripped as far as may be of all matter, lest the motions of planets and comets be hindered or rendered irregular." Newton, *Philosophical Writings*, 2004; p. 108-109); those in Query 20/28 ("a Globe not solid (such as are the Planets) would be retarded....And therefore to make way for the regular and lasting Motions of the Planets and Comets, it's necessary to empty the Heavens of all Matter"; *Opticks*, 1730 edition, p. 343-345); and those in the General Scholium, to be discussed subsequently.

[73] Newton: *Principia*, remarks preceding book 3 (1999 [1726], p. 793). The suppressed third book was published posthumously, in 1728, in the original as De mundi systemate, and in translation as *A Treatise of the System of the World.*

[74] On the date of the "Conclusio", I am following the Halls, who conclude that Newton composed the "Conclusio" before finishing the final version of the Principia's third book; see *Unpublished Scientific Papers*, p. 320. On the date of the "Draft Conclusion to the *Principia*", I am relying upon Cohen, who as noted earlier concludes that the manuscript was written at some point during the period 1704-1712; see his "Guide", pp. 287-292.

Further investigation was needed, however, and in the *Conclusio* Newton writes that he wishes "to give an opportunity to others" to help ascertain whether such forces exist[75]. The *Principia* as published still retained some mention of the hypothesized forces; in the Author's Preface, Newton mentioned his "suspicion that all phenomena may depend on certain forces by which particles of bodies, by causes not yet known, either are impelled toward one another and cohere in regular figures, or are repelled from one another and recede"[76]. But the *Conclusio* had provided more detail, and by suppressing it, he effectively withdrew his invitation to others to investigate the hypothesized forces.

Newton was not mistaken in anticipating disapprobation. While not all found fault, the review appearing in the *Journal des Sçavans* denied that he had produced a work of natural philosophy; the review's author (anonymous but thought to be by Pierre Silvain Régis), assessed Newton's demonstrations as being "only mechanical", since "he has not considered their Principles as a Physicist, but as a mere Mathematician"[77]. Huygens too saw a failing in natural philosophy, but he diagnosed it differently. He read Newton as having provided a physical cause, but one that was unintelligible: an "absurd" principle of attraction that was "not explainable by any of the principles of mechanics"[78]. As for Leibniz, who learned of the Principia from the Acta Eruditorum's review, he reacted with his 1689 Tentamen; and he then wondered why Newton would invoke an "incorporeal and inexplicable power" of attraction, when Huygens had already explained gravity "very plausibly by the laws of mechanics"[79].

Toward the end of the century, there began some of the events that would eventually provoke the priority dispute and the associated public disagreements between Newton and Leibniz about natural philosophy. To review very briefly a few points about that dispute, Newton discovered his method of fluxions, that is, the calculus, during his *annis mirabilis* (1664-1666); but since he chose to guard rather than to announce his findings, they were not publicly known when Leibniz began his work in mathematics several years later. Although Leibniz had reached his discoveries a decade later than Newton, he made them public, facilitating their further development—carried out in large measure by the Bernoulli brothers during the 1690's—and ensuring their impact. It is now known that Leibniz did not obtain any substantive ideas from some letters concerning Newton's results (passed along to him by the Royal Society's secretary, Henry Oldenburg), yet circumstances were much less clear at the time. Although Leibniz began efforts to establish priority with some papers published soon after Newton's *Principia*, the incendiary events came much later; near the turn of the century, Nicolas Fatio de Duillier championed Newton as first discoverer, and in 1708 John Keill insinuated that Leibniz had plagiarized Newton's ideas. Afterward, differences between Newton and Leibniz could no longer be expressed temperately, including their longstanding differences about natural philosophy and method. Just prior to the time of the General Scholium's composition came Leibniz's aforementioned letter to Nicolas Hartsoeker; and then the Royal Society's notoriously partial report on the priority dispute, known as *Commercium Epistolicum*, which was followed by Leibniz's 1713 anonymous *Charta volans*, responding with both defense and counterattack.

Newton wrote the General Scholium in early 1713, drafting several versions. (On the General Scholium's history, including its drafts and the revision for the *Principia*'s third edition of 1726, see Stefan Ducheyne's chapter in this volume.) By this point, the *Principia*'s second edition was heading toward completion, under the editorship of Trinity College's young astronomy professor, Roger Cotes, and with the involvement of Richard Bentley. It was in a letter to Bentley that Newton first mentioned the General Scholium, indicating that he would send it along soon, and then adding, "some are perswading me to add an Appendix concerning the attraction of the small particles of bodies. It will take up about three quarters of a sheet, but I am not yet resolved about it"[80]. The decision he reached about interparticle attractions was a cautious one, and when

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[75] Newton, "Conclusio", 1687 (in Newton, 1962, p. 345, the Halls' translation).

[76] *Principia*, 1999 [1726]; p. 382.

[77] On the reviewer's identity, see Aiton (who indicates that he is "following Mouy's guess"; 1972, p. 114); Cohen, 1980, p. 96-97. The translation is Cohen's (*ibid.*)

[78] In Cohen (1980, pp. 81 and 80).

[79] Leibniz to Huygens, 1690 (in *Leibniz: Philosophical Essays* translated by Roger Ariew and Daniel Garber; 1989; p. 309).

[80] Newton to Bentley, 6 January 1712/13 (in Newton, 1959-1971, ed. Turnbull et al., vol. V, p. 384).

he sent the General Scholium to Cotes in early March, he had condensed the discussion to the famously enigmatic final paragraph[81]. He still had another modification to make, sending Cotes in mid-March the methodological statement that appears directly after *hypotheses non fingo*.

The General Scholium's response to the orthodox mechanical philosophers

The General Scholium's causal claim about gravity

With the publication of the second edition in 1713, the General Scholium took the place that the *Conclusio* might have held, serving to close the *Principia*. The Scholium begins with an attack on the orthodox mechanical philosophy's gravitational theory, a spare sentence of just five words that in hindsight seems all the more devastating for its dispassionate tone: *Hypothesis vorticum multis premitur difficultatibus*—"The hypothesis of vortices is beset with many difficulties"[82]. The opening paragraph proceeds by reviewing some of those difficulties, ending with the troubles Newton saw in its attempts to accommodate cometary phenomena. The secondary causes offered up by the orthodox mechanical philosophers not only fail to explain comets' regular motions, the paragraph asserts, they would actually impede them. Obeying the same laws as planetary motions, "comets go with very eccentric motions into all parts of the heavens, which cannot happen unless vortices are eliminated"[83].

Having dispensed with his critics' secondary causes, Newton turns to the ones that he himself had advanced, those designated by his laws of gravity and of motion. He begins by emphasizing something that a gravitational theory must acknowledge: void space. "The only resistance which projectiles encounter in our air is from the air. With the air removed…resistance ceases…. And the case is the same for the celestial spaces"[84]. Although on a realist construal of his gravitational force, the fact of void space ushers in the possibility of unmediated distant action, Newton nevertheless presents his gravitational force as causally efficacious. Its realist status is suggested in the following passage, which contrasts phenomena that can be accounted for via secondary, "mechanical" causes (to be discussed further in the next section) against those requiring primary causation. Whereas the advent of the orbits required divine action—a belief that retrograde comets must have made especially compelling—the gravitational force is sufficient to maintain them.

> "Planets and comets must revolve continually in orbits given in kind and in position, according to the laws set forth above. They will indeed persevere in their orbits by the laws of

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[81] Although Newton did consider the possibility of unmediated distant action, most notably in Query 21's hypothesis about an exceedingly rare aether whose particles repel one another, he was nevertheless inclined to doubt it. This is evident in a number of texts, including not only his fourth letter to Bentley, but drafts related to the *Opticks* queries, in which he refrains from locating in matter the active principles he suggests as gravity's cause, and wonders theyre they could be located; it is also evident from the fact that he continued to search for gravity's physical mode of action, rather than considering the question to have been settled. One might therefore wonder whether he pinned any hopes on the very subtle spirit that he briefly discusses in the General Scholium's closing paragraph. To be a means of avoiding unmediated distant action, by densely filling the heavens, while remaining consistent with his *Principia* findings against a dense material medium, such a spirit would have to be immaterial. Newton does speculate about such a medium, albeit briefly, in an unsent letter responding to Leibniz's charges in the *Memoirs of Literature* about unmediated distant action. (There, he mentions several possible causes of attraction, one being "a power seated in a substance in which bodies move and float without resistance and which has therefore no *vis inertiae* but acts by other laws than those that are mechanical"; in Newton (2004, pp. 116-117). As for the General Scholium's very subtle spirit—however, shown by the Halls' work on such manuscripts as the "Draft Conclusion to the *Principia*" to have been electric—its status as material or immaterial is rather unclear. And in any case, there are no indications that Newton ever considered that spirit as gravity's physical motor. (On this I disagree with Cohen, who suggests at a couple of points that Newton thought of "electricity as a possible agent in gravitation"; see Cohen's footnote to the passage bracketed by 'pp', *Principia*, p. 944, and also his "Guide", 1999, p. 25.) For a thorough investigation of the General Scholium's very subtle spirit, see the chapter by Cesar Pastorino in this volume.

[82] Newton, General Scholium, *Principia* (1999 [1726], p. 939).

[83] Newton, General Scholium, Principia (1999 [1726], p. 939). Aiton, a classic source on the vortex theory's resilience, considers some of the weaknesses in Newtonian arguments against the theory, along with an eighteenth century finding that invalidated one worry about the notion of comets moving in a fluid: "Even the supposed incompatibility of the motion of the comets with the idea of a fluid vortex dissolved before the vortex theory was finally abandoned. For Euler in 1745 made the startling discovery, now known as D'Alembert's paradox, that the steady flow of an inviscid fluid past a solid object exerted no force on it."(Aiton, 1972, pp. 112-113).

[84] Newton, General Scholium, *Principia* (1999 [1726], p. 939).

> gravity, but they certainly could not originally have acquired the regular position of the orbits by these laws....All these regular motions do not have their origin in mechanical causes, since comets go freely in very eccentric orbits and into all parts of the heavens....This most elegant system of the sun, planets, and comets could not have arisen without the design and dominion of an intelligent and powerful being"[85].

From here Newton embarks upon a comparatively extended discussion of the divine being, at the end of which he declares God to be part of natural philosophy if considered from phenomena. While that declaration admits divine causation into natural philosophy, he then returns to secondary causation, specifically to the gravitational force.

In the General Scholium's penultimate paragraph, Newton clarifies the content and the limit of his causal position on gravity, also asserting his own methodology over that of the orthodox mechanical philosophers. In terms of its compositional history, this otherwise continuous paragraph comprises two parts. This first part belongs to the General Scholium as Newton first sent it to Cotes (with his letter of 2 March, 1712/13):

> Thus far I have explained the phenomena of the heavens and of our sea by the force of gravity, but I have not yet assigned a cause to gravity. Indeed, this force arises from some cause that penetrates as far as the centers of the sun and planets without any diminution of its power to act, and that acts not in proportion to the quantity of the *surfaces* of the particles on which it acts (as mechanical causes are wont to do) but in proportion to the quantity of *solid* matter, and whose action is extended everywhere to immense distances, always decreasing as the squares of the distances. Gravity toward the sun is compounded of the gravities toward the individual particles of the sun, and at increasing distances from the sun decreases exactly as the squares of the distances as far out as the orbit of Saturn, as is manifest from the fact that the aphelia of the planets are at rest, and even as far as the farthest aphelia of the comets, provided that those aphelia are at rest. I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses.

Newton sent the following addition some weeks later (28 March), when responding to some matters that Cotes had raised:

> For whatever is not deduced from the phenomena must be called a hypothesis; and hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy. In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction. The impenetrability, mobility, and impetus of bodies, and the laws of motion and the law of gravity have been found by this method. And it is enough that gravity really exists and acts according to the laws that we have set forth and suffices for all the motions of the heavenly bodies and of our sea[86].

In this passage, Newton presents his failure to discover the physical process by which the gravitational force works as reason to refrain from assigning any physical cause to the force. That is to say, he presents that failure as reason to limit his causal claims—not as reason to abjure any causal claim about gravity whatsoever, nor as reason to classify his findings as falling outside the domain of natural philosophy. Indeed, he presents his discovery, despite its limitation, as constituting a very significant causal claim. The gravitational force expressed by his law is a force that is fundamentally relational rather than monadic; that holds universally of bodies rather than being peculiar to terrestrial ones; and that is proportional to the sheer quantity of the bodies' matter, instead of to their surface areas as predicted by vortex theories. It is this force that "really exists". It is the real cause of such phenomena as the tidal shifts and the planetary orbits. In Newton's view, then, his force fulfills natural philosophy's traditional goal of discovering the causes of natural

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[85] Newton, General Scholium, *Principia* (1999 [1726], p. 940).

[86] General Scholium, *Principia* p. 943; the translation is that of Cohen and Whitman, except that I have adopted Janiak's more literal translation (Janiak, 2007, p. 129) of &*ad corporum caelestium* &*mari nostri omnes sufficiat,* which Cohen and Whitman translate as "and is sufficient to explain all the motions of the heavenly bodies and of our sea". This passage has of course been much analyzed, and certain aspects of my discussion have been influenced by some aforementioned works by Cohen, Stein, Smith, Harper, and Janiak, among others.

phenemena. The quoted passage's final sentence might accordingly be understood as responding to the *Journal des Sçavans*, whose critic had opined long ago that the *Principia* constituted merely a mechanics. For despite the unanswered physical question, Newton's discovery is enough to realize, at least in part, natural philosophy's traditional goal of discovering causes of natural phenemena[87].

Newton's view that he has made a causal discovery, even though a question mark hangs over the physical side of the problem, is comprehensible only within the framework of his two-stage investigative procedure[88]. That procedure, which recognizes mathematical necessities as setting constraints upon physical possibilities, takes the mathematical stage to be capable of zeroing in on an impressed force as the cause of certain phenomena, even as the physical stage of investigation, which seeks that force's physical aspects, remains to be done. His view had never been comprehensible, however, to those critics who fully identified a causal discovery with the physical findings that for Newton constituted only the second stage, and who treated material contact action as a given. Having reminded such critics of the vexing facts that a truth-seeker must admit, including both the void and the irrelevance of surface area, Newton further responds by commending his own restraint in acknowledging the limits of his knowledge. To do so, he passes over the milder expressions found in his drafts, such as *hypotheses fugio*, "I flee from hypotheses". He instead settles upon the more mordant phrase *hypotheses non fingo*, implicitly accusing the vortex theorists of resorting to fictions, as his De gravitatione had accused Descartes of doing long ago[89]. (For a thorough discussion of Cartesianism and polemics in the General Scholium, see the chapter by Noa Shein in this volume).

The General Scholium on hypotheses

Newton's most famous words express a narrow claim about gravity, namely, his refusal to treat any mere hypothesis about the gravitational force's cause as if it had the status of knowledge. However, *hypotheses non fingo* are words usually understood as simultaneously expressing a broader, methodological principle about the role of hypotheses, namely, his prohibition against treating any mere hypothesis as knowledge. I concur with commentators who assign that dual meaning; for regardless of debates about whether or to what extent Newton might have feigned hypotheses, the principle expresses an ideal of his experimental philosophy[90].

How should we understand the sentence that follows, however? *Prima facie*, its second conjunct—"hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place in experimental philosophy"—looks like a polemic. It looks like a repudiation of all uses of all hypotheses—even the crucial role that Newton continuously assigned to physical hypotheses, namely, to serve as conjectures whose truth value could be investigated

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[87] Might this have been Newton's meaning with the words *satis est*? It is difficult to know for sure, as Cohen notes in his discussion of the phrase (1999, §9.2, pp. 277-278; see also Cohen, 1980, pp. 32-33).

[88] As noted earlier, George Smith has done a great deal to bring attention to the importance of this scholium, which follows proposition 69 in book 1, section 11.

[89] *Hypotheses fugio* is the version contained in Drafts A and C (the relevant sentence reading, *Nam hypotheses seu metaphysicas seu physicas seu mechanicas seu qualitatum occultarum fugio*, and followed by the claim that hypotheses are prejudices that do not beget science); see Newton, 1962, p. 350). Once again, the chapter by Stefan Ducheyne in this volume contains a detailed discussion of these and other variants in the drafts. With respect to Newton's ultimate choice of the verb fingere, and Cohen's choice of 'feign' (instead of, e.g., "I frame no hypotheses", as in Motte) when translating Newton's famous words, see Cohen (1962, esp. pp. 380-382; 1999, §9.1, esp. pp. 275-277). Cohen cites a precedent for his translation in Koyré, who pointed to Newton's use of the term confingere in the 1704 Optice as grounds for thinking that in the General Scholium, he used 'fingo' to refer to invented fictions (see Cohen, ibid., p. 275). In further support for that reading, Stephen Snobelen (2001, p. 201, note 133) calls attention to this remark, which follows Rule 3: "Certainly idle fancies ought not to be fabricated recklessly against the evidence of experiments" (*Principia*, 1999 [1726], p. 795).

[90] Gabbey (2003, p. 188, and elaborated in discussion) denies that hypotheses non fingo expresses any methodological claim, as assumed by the standard translation, and in connection with this he holds that *hypotheses non fingo* should be translated using the present progressive—thus, "I am not feigning hypotheses" or "I am not now feigning hypotheses". In his view, it is the subsequent sentence that expresses a methodological claim, while *hypotheses non fingo* is an instantiation of that subsequent methodological claim. While I see Gabbey's translation as feasible, I have come to favor the standard translation. As noted, I see no difficulty in supposing that Newton intended h*ypotheses non fingo* to convey both his narrower absention about gravity's cause and a broader, methodological principle. Furthermore, if Newton had intended any sentence to convey a methodological principle in the version of the General Scholium that he first sent to Cotes, that principle would had to have been conveyed by the words *hypotheses non fingo*, since the sentence that follows in the published version did not yet exist.

experimentally. He had articulated that role in connection with his 1672 paper, as noted previously, and throughout his career he reaffirmed it by his practice and in print, explaining the aim of his *Opticks* queries, for instance, by indicating that he did not treat hypotheses as *scientia*, but rather *per modum questionum*[91]. In fact, however, what might look like a polemical repudiation of hypotheses' experimental role is simply due to the elision of a word; Newton's intended meaning is that *feigned* or *assumed* hypotheses have no place in experimental philosophy. This becomes evident when one reviews the sentence's history, as part of a passage that he added in response to an objection raised by Cotes; their exchange illuminates that sentence along with Newton's other remarks about hypotheses[92].

When Cotes received the General Scholium that Newton sent on March 2, he had already been asked to write the Editor's Preface to the *Principia*, and in his reply he outlined the accessibly-styled synopsis of book 3's argument for gravitation that he was planning for that preface. In the course of his outline, however, Cotes reports himself stymied when trying to reason toward the 7th proposition, which asserts universal gravitation. Bearing in mind the *Principia*'s liberal definition of attraction as "any sort of propensity toward a center", Cotes casts an orthodox mechanical philosopher's eye upon its extension of the third law to gravitational attractions. In what has become known as the Invisible Hand Objection, due to the thought device he uses to represent an unseen aether, he asks Newton to imagine "two Globes A & B placed at a distance from each other upon a Table, & that whilst ye Globe A remaines at rest the Globe B is moved towards it by an invisible Hand"[93]. A bystander would see neither the hand nor any movement of A toward B, but only the movement of B towards A. So what could be the basis for believing that A also moves toward B, and is its third law partner? And given that we do not observe the sun moving toward a planet, what is the basis for believing it to be the planet's third law partner? Cotes suggests that despite Newton's professed neutrality in using the term 'attraction', his third-law pairing of sun and planet is actually based upon a tacit assumption: that the sun possesses a true, physical power of attraction. Here we see that shining a spotlight upon his critics' method and their uses of hypotheses, Newton was also drawing attention to his own; for how, Cotes then asks, would Newton answer "anyone who should assert that You do *Hypothesim fingere*"?[94] Leaving aside a number of issues, one can see Cotes as pressing Newton to resolve the following dilemma. To reaffirm the third-law pairing of sun and planet, or more generally, the mutuality of gravitational attraction, would be to have feigned a hypothesis (and not just any hypothesis, but that of true attraction and thus unmediated distant action). Yet to avoid such hypothesis-feigning, by adhering firmly to Definition 8's neutral definition of 'attraction', would amount to conceding that the planet's third-law partner might be something other than the sun—and in so doing to compromise the assertion of mutual attraction.

In his letters of response, Newton rejects Cotes' dilemma as inapposite. He sees his two-stage investigative procedure as keeping him from being pushed into the dilemma's first horn. While his critics would see this as a sleight of hand, from Newton's perspective the term 'attraction' is neutral with respect to the physical causes,

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[91] Newton's Letter to Fontenelle of Autumn 1719, explaining the Queries in the second edition of Optice, which appeared the same year. Isaac Newton, *Correspondence* vol. 7, p. 72. As noted, in 1672, Newton wrote that hypotheses "may furnish experiments" ("Mr. Newton's Answer to the foregoing Letter., No. 85, p. 5014. Translated from the Latin", (in Newton, 1978, p. 106)). It is Roger Cotes who explicitly states the role for the second edition, writing in his Editor's Preface that those basing their natural philosophy on experiment "do not contrive hypotheses, nor do they admit them into natural science otherwise than as questions whose truth may be discussed".(Cotes, Editor's Preface to the Second Edition; Principia, 1999 [1726], p. 386.) Newton did not in the end review Cotes' preface, however it represented his view accurately on this point, and the following year, in his 1715 Account of the *Commercium Epistolicum*, Newton assigned hypotheses the role of "Conjectures or Questions proposed to be examined by Experiments."(Newton, 2004; p. 123).

[92] The exchange between Cotes and Newton concerning this objection is far richer and more complex than my cursory discussion could do justice to. An especially illuminating exploration of Newton's methodology in connection with the exchange is William Harper's 2002 article, developed largely in response to Howard Stein (1967 and 1991 (PSA 1990; referenced by Harper as 1991)).

[93] Cotes, from his letter of 18 March (erroneously dated '18 February'), 1712/13 (Letter LXXX in Newton, 1850, ed. Edelston, pp. 151-154). A recent, in-depth analysis of Cotes' objection may be found in Biener and Smeenk (2011); see also discussions in Koyré (1965); Harper (2002, esp. pp. 76-78).

[94] Cotes' letter to Newton of 18 March, 1713 (Cotes had erroneously dated it '18 February'); in Newton 1850, ed. Edelston, pp. 152-153. In case Newton needed any reminding that those critics might well raise the same objection, Cotes informs Newton of Leibniz's latest attack, in the letter to Hartsoeker published in the *Memoirs of Literature*.

since the physical stage of investigation has not been achieved. Yet since the mathematical stage has discovered the gravitational force itself, as expressed in the law of gravitation, the masses to which the third law applies are those figuring in that law, which is to say the sun and planet in the simplified case at hand. As for the dilemma’s second horn, Newton sees the ideas expressed later in Rule 4 as protecting him from that. In remarks to Cotes that very much resemble Rule 4, as various commentators have noted, he emphasizes that though even a proposition with the highest evidence will remain defeasible in an experimental philosophy, that defeasibility does not erase the line between legitimate challenges and illegitimate ones: “He that in experimental Philosophy would except against any of these must draw his objection from some experiment or phaenomenon & not from a mere Hypothesis, if the Induction be of any force”[95].

It is in the course of so defending Law 3’s epistemic status that Newton clarifies what a hypothesis is: “The word Hypothesis is here used by me to signify only such a Proposition as is not a Phaenomenon nor deduced from any Phaenomena but assumed or supposed wthout any experimental proof”[96]. The first part of that sentence (i.e., “a Proposition as is not a Phaenomenon nor deduced from any Phaenomena”) defines a hypothesis in the broadest sense. The remainder of the sentence (i.e., “assumed or supposed wthout any experimental proof”) then restricts the definition to something narrower, to wit, a feigned hypothesis. That meaning is evident even though the word ‘feigned’ is elided; and this allows us to replace a polemical reading of the first sentence of the General Scholium’s added passage with the charitable one noted above.

Never having been intended to fulfill the *Conclusio’s* function, the General Scholium considers hypotheses mainly at the metalevel, as Newton responds to critics by asserting the primacy of his own methodology. If we ask why he responded this way, an initial answer may be simply that his critics had themselves focused upon method, by insisting that theories provide physical causes and, in particular, causes meeting their intelligibility requirement. However, we may grant that point while still asking why Newton did not deflect their criticism by changing the subject[97]. He could have changed the subject by endorsing the main criticism made in a review that the *Principia*’s first edition had received. When the anonymous critic in J*ournal des Sçavans* complained in 1688 that his demonstrations were “only mechanical” since he had “not considered their Principles as a Physicist, but as a mere Mathematician”,[98] Newton could have replied that this was no criticism, because the discovery of predictive mathematical devices was all he had ever intended; in other words, he could have repudiated realism. That he did not change the subject suggests that he considered the question about method pertinent, and thus that he saw himself and the orthodox mechanical philosophers as sharing a common goal—natural philosophy’s traditional goal of discovering the real causes of natural phenomena. For if Newton were not interested in the discovery of real causes, he would see questions about whether matter has powers of unmediated distant attraction as irrelevant to his enterprise. As I interpret Newton, his realism explains why he responded to the orthodox mechanical philosophers as he did, and it is an important feature of his own mechanical philosophy, as indicated below.

The machine of celestial bodies and Newton’s own mechanical philosophy

Machines and natural processes

During the early modern period, many theorists looked to machines when trying to understand natural processes, and yet which features characterized machines was often in the eye of the beholder. To be sure, certain features caught everyone’s attention; since final causes had no place in any of the steps of the operation by which a wedge split a beam or a pulley hoisted a chunk of marble, machines suggested a way of

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[95] Newton to Cotes, Letter of 31 March, 1713 (Letter LXXXII in Newton, 1850, ed. Edelston, pp. 156-157). This sentence is very similar to one appearing in the passage Newton just sent Cotes, to append to his General Scholium. Harper and Smith (1995, pp. 140-141) and Harper (2002, pp. 94-95) in particular have drawn the connection between the General Scholium and related exchanges and Rule 4(which Newton drafted in this period but which did not arrive in time for inclusion in the Principia’s second edition).

[96] Newton to Cotes, Letter of 28 March, 1713 (Letter LXXXI in Newton, 1850, ed. Edelston, pp. 154-156).

[97] I thank Andrew Janiak for encouraging me to address this question

[98] Review of the *Principia in Journal des Sçavans*, 2 August 1688, probably by Pierre Silvain Régis, Cohen notes (1980, p. 96, 97, his trans.).

explaining natural processes by efficient causes alone. And further, there was enough hope of modeling natural processes upon the shapes and sizes of a machine’s parts, along with the possibility of transferring motion among them by contact action, that one can speak of an orthodox mechanical philosophy and of its dominant sense of mechanism. Nevertheless, not everyone’s eyes were equally drawn in that direction. Those focusing upon the effects of changing the direction of the input force with a pulley, or of shifting the fulcrum of a balance, might be more struck by everything that made a science of machines and motion possible—by the proportions, regularities, and causal constraints imposed by an interdependence of parts, such that a change at one point in the system necessitates predictable and mathematically-expressible changes elsewhere. There were, then, many alternatives to the orthodox mechanical philosophy’s senses of ‘mechanism’ and ‘mechanical’, and those alternatives included a constellation of meanings associated with mechanics as a discipline: treating natural systems as machines; understanding natural phenomena as law-governed; and representing such phenomena mathematically[99]. Indeed, Newton’s acolyte John Keill mocked the orthodox mechanical philosophers for having developed theories that ignored mechanics:

> Although now-a-days the Mechanical Philosophy is in great Repute, and in this Age has met with many who cultivate it; yet in most of the Writings of the Philosophers, there is scarce anything Mechanical to be found besides the Name. Instead whereof, the Philosophers substitute the Figures, Ways, Pores, and Interstices of Corpuscles, which they never saw[100].

As for Newton himself, he used terms such as ‘mechanical’ in a variety of ways, according to his purpose. He was of course conversant with their prevailing senses, as he had to be when evaluating the orthodox mechanical philosophy. In this passage from the General Scholium, for instance, he uses the term ‘mechanical causes’ to refer to surface impacts: “This force arises from some cause....that acts not in proportion to the *surfaces* of the particles on which it acts (as mechanical causes are wont to do) but in proportion to the quantity of *solid* matter”[101]. Similarly, in Query 28, when he refers to philosophers who explain everything “mechanically”, he means those who insist upon “dense matter”[102]. However, Newton did not restrict his uses of the terms ‘mechanical cause’ and ‘mechanical philosophy’ to their prevailing senses. When writing of mechanical causes in his *Conclusio*, for instance, it was not the vortex theorists’ sense of the term that he had in mind, but instead a sense associated with his distance forces. Anyone discovering the local motions of minute particles and the “lesser forces” responsible for them, he declared, “will have laid bare the whole nature of bodies so far as the mechanical causes of things are concerned”[103]. These hypothesized “lesser forces”, he suggests at various points, might behave similiarly to gravity; thus he is here associating the term ‘mechanical cause’ with impressed forces speculated to operate between bodies that are spatially separated yet unconnected by any dense material medium. Along similar lines, in a manuscript summarizing some of the *Principia*’s content, the section devoted to the celestial system and gravitational force is entitled “The Mechanical Frame of the World”[104]. And in the General Scholium, he appears to be referring to his own laws when asserting that although the planetary orbits will persevere by those laws, “all these regular motions do not have their origin in mechanical causes”[105].

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[99] Scholars have documented a range of meanings for terms such as ‘mechanism’ and ‘mechanical’. Alan Gabbey’s list, for instance, includes the following: theories explaining phenomena non-qualitatively, in terms of the motions and configurations of the parts of a uniform matter; those treating the universe and its component systems as machines; those aiming to mathematize representations of phenomena; those postulating necessary laws of nature and motion; and those theories excluding everything spiritual or immaterial from the investigative domain (see Gabbey, 2002; pp. 337-38; also 2004, p. 15). Gabbey does not take these meanings to be unified by any shared necessary condition, notably contact action. By contrast, in an article from 1972 (see p. 523, n. 2), J.E. McGuire identifies a variety of notions of mechanism and accordingly of mechanical philosophies, but takes contact action to be a necessary condition uniting those mechanical philosophies.

[100] John Keill, Preface to his *Introduction to the True Physics*, 1702; in A.R. Hall (1980, p. 159).

[101] General Scholium, *Principia* (1999 [1726], p. 943).

[102] *Opticks*, Query 28, ([1730] 1952, pp. 368-69).

[103] “Conclusio” (in Newton, 1962, p. 333; the Halls’ translation).

[104] Newton, “The Elements of Mechanicks” (MS. Add. 4005, fols. 23-5 (ff. 5 23r - 5 24v according to the new folliation system of the Cambridge Digital Library)); in Newton (1962, p. 167); the Halls date the manuscript as post-*Principia*. Verelst, however, disagrees; see the end of the section entitled “A cluster of structurally related pre-*Principia* manuscripts” in her manuscript, “When Everything hinges on Gravitation”.

[105] General Scholium, *Principia*, (1999 [1726], p. 940).

At one point, in fact, Newton even uses the term 'mechanical philosophy' to refer to his own theory. In a draft variant of what would eventually become Query 31, he refers to his theory and laws of motion as "the Mechanical Philosophy", and urges inquiry into the active principles that he takes to be fundamental to it: "We ought to enquire diligently into the general Rules or Laws observed by nature in the preservation or production of motion by these principles as the Laws of motion on which the frame of Nature depends & the genuine Principles of the Mechanical Philosophy"[106]. Although this remark belongs to a draft passage he cancelled, it nevertheless indicates that the term 'mechanical philosophy' was not bound in his mind to the prevailing usage of contact action. It also had a sense applicable to his own theory[107].

Newton's mechanical philosophy in "Draft C" and other texts

Given how strongly the term 'mechanical philosophy' has been linked to contact action, hearing it associated with Newton's theory might initially seem to strike a discordant note. Nevertheless, since Newton approached the celestial orbits as a problem of mechanics, there must have been some sense in which he considered his theory a mechanical philosophy. A glimpse of that sense may be found in "Draft C" for the General Scholium.

> Now truly, if God rendered the System of the Sun and Planets in a most beautiful order, if he gave the Planets motion in such directions and velocities that they be carried in concentric orbits around the sun, in the same order and in the same plane; if he gave motions to the four Moons of Jupiter, utterly concentric with Jupiter, in the same order [and] in the same plane, and [gave] similar motions to the Moons of Saturn and to the Moon of the Earth; and to establish so accurately a machine of such great bodies at such distances is supreme skill and supreme power: If, moreover, Comets are moved in such eccentric orbits that they swiftly cross the orbits of the Planets, and in crossing scarcely perturb the motions of the Planets, & no irregularities in the motions of the planets can arise except by the attraction of Comets....[and] moreover, the distances of the fixed [stars] and the sun be [so] very great, [that] the whole System not fall in upon itself, certainly final causes have a place in natural Philosophy, and one may certainly inquire to what end this world was made[108].

Here, in the midst of the passage, Newton refers to the celestial system as "a machine of such great bodies", established by God. Taken literally, these words do not compare the world to a machine, they refer to it as an actual machine. This literal sense seems to be Newton's intended meaning. The idea had precedents, not all talk of the *machina mundi* being metaphorical, and in any case the literal meaning agrees with his Author's Preface and related passages from the Geometry, written several years later.

In those texts, it is not the orthodox mechanical philosophy that Newton is concerned to oppose; he mentions the moderns in the Preface only to praise their attempts to reduce natural phenemonena to mathematical laws. He is instead opposing some old disciplinary divisions. One was the ancients' partition of mechanics into a practical branch, devoted to the production of motion in physical machines, and a rational branch, which treated machines and the principles of motion abstractly, employing rigorous demonstrations and being, in Newton's words, "no less exact than geometry itself"[109]. Another division was that between natural philosophy and mechanics, particularly rational mechanics. The search for natural causes had traditionally belonged to natural philosophy, while mechanics, as a discipline applying mathematical methods to physical objects, had traditionally been classified within mathematics as a mixed science. In opposing those divisions, Newton is articulating, as well as furthering, developments that had been underway for some time; throughout the sixteenth and seventeenth centuries, mechanics and distinctions related to it had been changing. Rational

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[106] Draft variant of what would become Query 31, dated to c. 1705 by McGuire. University Library, Cambridge (ULC) Add. 3970, fols. 255r-256r. The text, written in English, is quoted in McGuire (1968, 170-71), with his discussion found in those same pages. McGuire dates the text, which he identifies the text as a draft variant of what appeared as Query 23 in the 1706 Optice and as Query 31 in the 1717/18 Opticks, to c. 1705.

[107] I have also discussed this and some of the following points elsewhere (see Kochiras, 2013).

[108] Newton, "Draft C" for the General Scholium, MS. Add. 3965, fols. 361-62; in Newton, 1962, p. 358 (my translation). (This draft is called "Manuscript C" by the Halls, in Newton, 1962, chapter 8.)

[109] Newton, *Geometry* (in Newton, 1976, ed. Whiteside, p. 289).

mechanics, which had re-emerged with the availability of its ancient texts, was less clearly distinct from practical mechanics, the former having ties to engineering and other practical endeavors, and practitioners of the latter delving into theoretical matters that involved geometric methods[110]. Additionally, while mechanics had historically been tied to problems about equilibrium, there was increasing attention to motion; and there were greater similarities between that discipline and natural philosophy.

Newton explains his vision of natural philosophy, against the old divisions, by contesting two assumptions about mechanics and the natural world. He contests the belief that anything mechanical is by its nature imperfect and inexact, and also the complementary assumption that exactness characterizes only geometry—an error implying that the the causal principles of natural motions should not be mathematically precise[111].

Yet the idealized cases and machines of rational mechanics frame the possibility of perfectly realized physical machines, functioning by mathematically precise principles of motion. And Newton makes just that point in the Geometry: a perfect mechanician, working with perfect accuracy, would "*work everything to the pattern of the rational*"[112]. His better known remarks from the Preface similarly deny that physical things are by their nature imprecise: "Anyone who works with less exactness is a more imperfect mechanic, and if anyone could work with the greatest exactness, he would be the most perfect mechanic of all"[113]. The Preface then redefines mechanics as a single science of motion, such that a mechanics investigating real machines and the principles of their motions will properly include not only devices such as the pulley and balance, but also the world and its natural powers. With those remarks, and most especially that from the *Geometry*, Newton indicates exactly what it is to be mechanical: to be created according to the patterns of rational mechanics, with the exactness of its machines and principles of motion. Thus, a physical thing will be mechanical *simpliciter* if it conforms in some degree to the possibilities offered by rational mechanics. It will be more mechanical if it conforms more accurately; "the more mechanical—that is, skilfully wrought—a thing is, the more exact it is"[114].And a physical system will be perfectly mechanical if it realizes perfectly the patterns and principles of rational mechanics. "Draft C" points to the celestial bodies as such a system.

For Newton, the system of celestial bodies is mechanical in that the force driving it manifests geometric proportions, functionally relating quantities in that system (to allow again the anachronism), such that a change in any one necessitates changes in others. And that force, so much at odds with the orthodox mechanical philosophy, is the best exemplar of his own mechanical natural philosophy. His view that the physical can be worked according to the pattern of the rational recasts apparent imperfections, most notably deviations from a perfectly elliptical orbit, as problems to be investigated under the presumption that there is exactness in the world. Limitations exist of course; due to the interactions of so many bodies, Newton concludes in a manuscript, a planet "traces a fresh orbit" with each revolution, and "it would exceed the force of human wit to consider so many causes of motion at the same time"[115]. Yet these are human limitations, not to be mistaken for any real imprecision in the world, and no reason to seek less than a mathematical physics.

The limit of causal claims and some implications

Newton's refusal to assert any complete or universal causal principle was an instance of a much-remarked feature of his method, one that would characterize the modern science that would succeed natural philosophy: while some orthodox mechanical philosophers, most notably Descartes, sought to construct complete systems and explanations, Newton isolated limited and

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[110] See, respectively, Bertoloni Meli (2006, p. 638), and Bennett (2006, p. 673-674).

[111] The Preface along with related passages of the *Geometry* raise interpretive questions about the relationship that Newton sees between geometry and mechanics. A recent and compelling interpretation of that relationship and of Newton's philosophy of geometry has been given by Mary Domski; focusing in particular upon Newton's claim that "geometry is founded upon mechanical practice", Domski rejects the constructivist interpretation associated with Descartes' *Géométrie*, developing an alternative, historical interpretation.

[112] Newton, *Geometry* (in Newton, 1976, ed. Whiteside, p. 289, my emphasis).

[113] Newton, Author's Preface; *Principia*, (1999 [1726], p. 381).

[114] Newton, *Geometry* (in Newton, 1976, ed. Whiteside, p. 291).

[115] Newton, "On the Motion of Bodies in Non-Resisting Mediums" (in Newton, 1962, p. 281; the Halls' translation).

tractable questions for empirical investigation. His position carried the consequence indicated by the General Scholium's most famous words, namely, that he gave no answer to the vexing question about gravity's physical means of operation. This left the door open to the vitalist strand of his thought mentioned earlier. For in the absence of any complete causal explanation, the possibility remained that such causal questions might eventually be answered by findings about some vegetative spirit or active principles[116].

Such vitalist speculations, informed by Newton's study of alchemical texts and motivated by observations that seemed to contravene the Cartesian notion that the universe contained a fixed quantity of motion, transferrable among bodies but never lost or replenished, were by no means extinguished by his mechanics and gravitational theory. Even the *Principia* itself, in its first edition, alludes to the connection he drew between decreasing particle size and increasing activity, by mentioning his suspicion that comet vapors supply the subtlest part of the air, "which is required for the life of all things"[117]. His vitalist ideas had not abated by the period of the 1706 Optice; in draft material he classifies life and will as active principles, and speculates that "the laws of motion arising from life or will may be of universal extent"[118]. And as we saw earlier, in the cancelled passage that describes his own theory as a mechanical philosophy, he suggests active principles as its more fundamental causal principles.

The divine mechanic

A final point concerns Newton's notion of a celestial machine as illuminated by his idea of the divine mechanic. While the Author's Preface and the *Geometry* simply describe one who could work with the greatest exactness as being "the most perfect mechanic of all", those circumspect references to the divine mechanic have explicit analogues in other texts. In one of his letters to Bentley, Newton refers to the deity as a "cause ... not blind & fortuitous, but very well skilled in Mechanicks & Geometry"[119], and the above-quoted passage from "Draft C" is similarly direct. Using the sort of phrase he reserves for the deity, Newton describes the planetary "machine of such great bodies" as a work of "supreme power"; and in any case that remark belongs to a longer description of God's creation, one reasoning toward a place for final causes in natural philosophy.

The notion of the world as a machine has frequently been associated with deism and even atheism, especially when the machine at issue is a clock. This is in fact the way that Samuel Clarke presented it in one his replies to Leibniz. Leibniz had seized upon one of Newton's remarks in Query 31 of the *Opticks*, a voluntaristic suggestion that God sometimes acts directly to correct irregularities in the orbits; interactions among the planets and with comets caused such irregularities to increase, Query 31 suggested, "till the System wants a Reformation"[120]. When Leibniz derided Newton's God as an imperfect clockmaker, Clarke parried by linking the clock metaphor to a deism he took the Hannoverian's philosophy to imply. Leibniz's "Notion of the world's being a great Machine, going on without the Interposition of God, as a Clock continues to go without the Assistance of a Clockmaker, is the Notion of Materialism and Fate", Clarke wrote; and it "tends, (under pretence of making God a Supra-mundane Intelligence) to exclude Providence and God's Government in reality out of the World"[121]. But though Clarke here charges that a Leibnizian clockwork universe would be a godless one, the notion that the world is a machine does not actually entail deism. In fact, the clockwork metaphor had initially been a pious one. Medieval thinkers introducing clockwork analogies had emphasized not only God's creation of the machine, but also his providential attention to it, as the clockmaker and caretaker[122].

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[116] Here I am sympathetic to the "two-tier" ontology that McGuire favors and McMullin opposes (at least if attributed to Newton consistently), in which forces are causally efficacious and yet are themselves caused by active principles. See McGuire (1968, p. 160); McMullin (1978, p. 82).

[117] *Principia*, Book 3, Proposition 41 (1999 [1726], p. 926).

[118] Draft material for the *Optice*, ULC Ad. 3970.9 f.619, in Westfall (1971, p. 397). Very similar words recur elsewhere in the drafts: "We find in orselves a power of moving our bodies by or thought. Life & Will (thinking) are active Principles by wch we move our bodies, & hence arise other laws of motion unknown to us." (ULC Add. 3970, fol. 620r; in McGuire, 1968, p. 171.) These remarks and that quoted in section 2 all belong to the same set of drafts. A note in section 2 mentions a number of works discussing Newton's vitalist and alchemical thought.

[119] Newton's first letter to Bentley, in Newton (1959–1971, Vol. III, p. 235).

[120] *Opticks*, Query 31, p. 402; but Query 23 in the edition on which Leibniz was commenting.

[121] Clarke's first reply, his translation of 1717; p. 15 (italics in the original have been eliminated here).

[122] On this and related points, see Snobelen (2012, pp. 152-153).

Newton's divine mechanic is very much in line with that original metaphor, as Leibniz realized; a deity who might intervene to adjust the planetary orbits or any other processes functioning by secondary causes is very much a providential ruler. Communicating that aspect of God's nature is one of Newton's central concerns in the General Scholium; whereas "a god without dominion, providence, and final causes is nothing other than fate and nature", Newton's God rules "as the lord of all" (a dominion that is, as agreed by all but Descartes, yet constrained by mathematical truths and the law of non-contradiction)[123].

Conclusion

To review briefly in closing, one of Newton's chief aims in his exceedingly rich General Scholium was to assert his own method and conception of natural philosophy over the vortex theorists and their orthodox mechanical philosophy. In my view, his dispute with some earlier thinkers and their associated disciplinary divisions was a dispute about whether mathematical necessity obtains in the physical world; and I interpreted his dispute with the vortex theorists as a dispute about its relationship there to physical necessity. The vortex theorists assumed their preferred physical cause, material contact action, as a given, employing their method of hypotheses (hypothetico-deductive method) within the confines of that assumption. By treating a particular causal mechanism as indefeasible, they implied it to be physically necessary, and furthermore treated physical necessity as the strongest sort of necessity within the physical world.

Newton, by contrast, took mathematical necessity to obtain in the physical world, and he took mathematical necessity to be the strongest sort of necessity there, setting constraints upon how forces could be physically realized and upon physical possibilities generally. While he employed something similar to the method of hypotheses, he set the bounds not with any assumed physical cause but instead with his prohibition against elevating mere hypotheses to the status of known truths, most famously declared in the General Scholium's penultimate paragraph. That penultimate paragraph is quite consistent his other core idea about hypotheses, i.e., that have the crucial role of furnishing experiments; as I indicated, although one remark initially appears polemically to deny that role, attention to the paragraph's compositional history shows otherwise.

In connection with the 1672 letter first articulating that role in theory development, I understood the procedure that Newton outlines there as a preliminary version of the two-stage investigative procedure for forces that Newton explains in the *Principia*. In the 1672 letter, properties of things should first conceived abstractly, apart from any physical conception, but afterward one may explain them by proceeding to hypotheses; by 1687, those steps have crystallized with respect to forces into the mathematical and physical stages of investigation.

Concerning the first, mathematical stage of the *Principia*'s investigative procedure, I sided with those who have interpreted Newton's stance toward his forces in realist terms. I then raised the question of why Newton answered his critics as he did. He could have deflected the charge of having allowed unmediated distant action had he embraced a different charge—namely, that he had considered the celestial motions not as a cause-seeking physicist, but merely as a mathematician. Instead, he responded by waging a methodological counteroffensive. His realism explains why. Although he specified it in a new way, as the discovery of impressed forces, Newton retained natural philosophy's traditional goal, the discovery of real causes of natural phenomena. He shared that traditional goal with the orthodox mechanical philosophers, and he responded to them as he did because he saw his method, rather than theirs, as the means of achieving their common goal.

In the interpretation that I have advanced, Newton too has a mechanical philosophy, though one founded upon a very different conception of the mechanical—that which is created according to the patterns and possibilities established by the mathematically precise, demonstrative discipline of rational mechanics. A physical system that perfectly realizes such possibilities will be perfectly mechanical, and "Draft C" for the General Scholium accordingly cites the system of celestial bodies as a machine. In Newton's conception, mathematical methods are essential to fulfilling natural philosophy's traditional goal of discovering natural causes; since the natural powers driving physical systems are fundamentally

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[123] General Scholium, *Principia* (1999 [1726], pp. 940-941).

characterized by geometric proportions, mathematical methods are essential for finding them. And once again, his investigative procedures recognize mathematical necessity not only as obtaining in the physical world but also as constraining physical possibilities there, which is hardly surprising, given that it constrains even divine decisions there.

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