Cosmology and Physical Astronomy in Newton's General Scholium

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1. INTRODUCTION

The General Scholium opens with Newton's trenchant criticism of the idea that the planets are carried in a vortex of subtle matter, like leaves swirling in the eddies of a stream. With the demise of Ptolemaic astronomy, natural philosophers in the seventeenth century faced the challenge of developing an entirely new understanding of the nature and causes of celestial motions. Vortex theories held that planetary motions result from the mechanical impulse imparted to planets by a swirling aetherial vortex. Many leading natural philosophers, before and after Newton, agreed that vortex theories provide the only plausible account of celestial dynamics, despite substantial differences in implementation. Newton's idiosyncratic case against this dominant line of thought had a profound impact on later work, even as vortex theories were avidly pursued for several more decades.

Newton transformed the assessment of vortex theories through four related lines of argument, each summarized laconically in the General Scholium's opening paragraphs. First, Newton recognized the dynamical significance of Keplerain regularities, shifting attention away from gross features of celestial motions that were the main explanatory targets for Descartes' seminal vortex theory. Newton provided the first quantitative study of vortex motion, based on his treatment of fluid motion and resistance, to show that vortices cannot account for Keplerian motions. Second, one of Newton's most spectacular successes in the *Principia* was to treat comets as subject to the same force as planets and their satellites. Newton pressed the stark challenges comets pose for vortex theorists more strongly with each subsequent edition of the *Principia*, as further observations substantiated his treatment. Newton could find, third, no evidence that celestial spaces offer resistance to the motion of planets or comets, and likewise failed to detect aether resistance in terrestrial experiments. Finally, Newton regarded many features of the solar system Descartes explained with vortices as evidence, instead, of providential design.

Taken together, these contributions exemplify Newton's effort to discourage his contemporaries from continuing to make "trial of nature in vain," by pursuing, like Descartes, unifying hypotheses of grand scope, and to promote an alternative, "more secure" approach to reasoning in natural philosophy. Many responses to the *Principia*'s first edition evaluated it negatively on broadly Cartesian terms. Régis's review in the *Journal de Savants* criticized Newton for merely offering a mechanics of celestial motions — rather than a physics — given his reliance on the problematic idea of attraction. The General Scholium and various other revisions Newton made to the second edition reflect Newton's efforts to parry this line of attack. He reframed the debate along two lines. First, he defended the claim to have established the reality of the gravitational force as the cause of celestial motions, without needing to "feign hypotheses" about its underlying physical source. His argument focused in part on ruling out other post-Copernican approaches to physical astronomy, with Leibniz replacing Descartes as the most significant rival by the second edition, and in part on making the case for a different approach to using evidence in natural philosophy. Newton transformed the debate in a second sense by defending an anti-deist conception of the relationship between

God and the natural world, as revealed in his discussions of design in relation to the solar system, and in his willingness to consider a "material or immaterial" agent as the cause of gravity.

The case summarized in the General Scholium reflects Newton's own reasons for abandoning vortices in the early 1680s. Section 2 recounts why Newton abandoned Cartesian natural philosophy in light of his new understanding of the dynamics governing celestial motions. The seed that would grow into the *Principia Mathematica*, the first *De Motu* manuscript (1684), established that Keplerian regularities hold very nearly for planets attracted to the sun by an inverse-square centripetal force. Any force of resistance arising from an aetherial fluid filling celestial spaces would provide an impedimet to, rather than an explanation of, regular planetary motions. A similar transition occurred in Newton's manuscript discussions of comets: he initially regarded comets as moving in a fluid aether, but soon recognized that their motion could be treated instead in terms of an inverse-square force directed towards the sun. These lines of thought lead to a sophisticated criticism of vortex theories in the *Principia*'s first edition. Section 3 considers Newton's arguments in Books 2 and 3 of the *Principia* in detail, and traces the substantial changes in subsequent editions. Section 4 contrasts Newton's new approach to evidential reasoning in the *Principia* with Leibniz's *Tentamen.* Readers of the first edition of the *Principia* would have found few clues regarding Newton's views on several of the questions regarding the origin and structure of the solar system Descartes had addressed. Section 5 considers Newton's famous correspondence with Bentley in relation to the design argument presented in the General Scholium.

2. DISCOVERING THE DYNAMICS OF KEPLERIAN MOTION

A spectacular comet appeared in November 1680, moving toward the sun and remaining visible until December 8th. Nearly all astronomers held that the appearance of a second comet, visible in the evening starting on December 10th, moving away from the sun, was entirely coincidental. But England's royal astronomer, John Flamsteed, proposed that these were in fact observations of a single comet, which had passed within Mercury's orbit, only to execute a sharp turn before reaching the sun. Newton learned of Flamsteed's proposal (and his observations) in February of 1681, and he began to explore intensely the nature of comets, their trajectories, and the causes of their motion. Here I will briefly explore why Newton was uniquely situated to recognize the dynamical significance of Kepler's laws, as they came to be known, and to develop a new understanding of comets, and how these discoveries led him to take the radical step of abandoning Cartesian vortices.

Newton's first foray into astronomy was inspired by an earlier comet. Newton's observations of the comet of 1664, just before his twenty-second birthday, appear in his Trinity College notebook along with brief notes from reading Streete's *Astronomia Carolina*.[1] From Streete Newton learned what are now called Kepler's first and third laws. Following Jeremiah Horrocks, Streete took Kepler's third law, which holds that the period of the planetary orbits T is related to their mean distances from the sun r as $T^2 \propto r^3$, to be exact, allowing one to determine mean distances from the more readily measured periods.[2] This result of Kepler's was widely accepted among astronomers by this time, justified by observations like those reported in *Harmonia Mundi*.Streete agreed with the widely held view that Kepler's so-called first law, that the planets move along elliptical orbits with the sun at one focus, was not as well established empirically.

1. J. E. McGuire and M. Tamny, *Certain Philosophical Questions: Newton's Trinity Notebook* (Cambridge: cup, 1983).

2. In a later manuscript, Newton found an expression for what he called the *conatus recedendi* for a body in circular motion (or centrifugal force), namely $\frac{v^2}{r}$, and noted that the *conatus* of the planets to recede from the sun then varies as $\frac{1}{r^2}$ as a consequence of Kepler's third law. Manuscript from 1665 / 1666 (J. Herivel, *The Background to Newton's* Principia [Oxford: Oxford University Press, 1965]).

Despite the success of Kepler's *Rudolphine Tables*, as vividly illustrated by his accurate prediction of the transit of Mercury, observed by Gassendi in 1631, the level of accuracy in positional measurement was not sufficient to rule out alternative oval-shaped orbits, such as those proposed later by Cassini and Huygens. Kepler's reasons for prefering an ellipse, based on his causal account of planetary motions, were not widely accepted.

Two decades later, a proper appreciation of the dynamical significance of Kepler's so-called second law would be Newton's first step in the *De Motu.* The second law holds that the radius vector from the sun to a planet sweeps out equal areas in equal times. In principle this law could be used to calculate the velocities of planets along their trajectories, but it was both mathematically intractable and lacked sufficient physical motivation, in the eyes of Streete and his contemporaries. The area rule implies "Kepler's equation" relating the mean anomaly M and the eccentric anomaly E, for an ellipse with eccentricity e: $M = E - e \sin E$.[3] As Newton would later prove (in the *Principia*'s stunning Lemma 28),[4] there are no algebraic solutions of this equation with a finite number of terms, so it cannot be solved directly for E given a value of M. Kepler proposed an iterative technique and challenged geometers to do better. Later astronomers developed less cumbersome methods for calculating planetary motions, but were not constrained by fidelity to the area rule. Streete used a geometric construction due to Ismaël Boulliau; although Nicolaus Mercator later showed that this construction works because it approximates the area rule, Newton would not have gleaned the area rule or Kepler's physical motivation for it from Streete's text.[5]

Newton was certainly aware of the mathematical challenge raised by the area rule, and found analytical and geometric solutions similar to those discovered by his contemporaries. Wren's geometric solution to what he called "Kepler's problem," based on the cycloid and published as an addendum to Wallis's *De Cycloide* (1659), spurred James Gregory to find an analytical solution using a Taylor series expansion.[6] Newton later remarked that he could solve Kepler's problem using a series expansion, without providing details, in a letter prepared for Leibniz in 1676.[7] He also obtained, in a 1679 manuscript, a new geometric solution to determine planetary motion that approximates Kepler's area rule.[8]

Like his contemporaries, Newton's approach to these mathematical problems of predictive astronomy was only loosely related to physics. Streete and the other texts Newton turned to later, such as Wing's *Astronomica Brittanica* (1669), described the physics of celestial motions with appeals to magnetic forces as well as aetherial vortices. Neither Wing nor Streete followed Kepler in attempting to base calculations of planetary motions on physical principles. Streete, for example, justified the main elements of his predictive astronomy empirically, and the discussions of vortices and magnetism provided plausibility arguments rather than grounding the mathematics.[9]

Throughout the 1660 - 70s, Newton appealed to the aether to explain terrestrial gravitation and the motion of celestial bodies in a variety of manuscripts, reflecting the deep influence of Descartes' *Principia Philosophiae.* Descartes' account of vortices emphasized their broad explanatory power, with little dis-

3. The mean and eccentric anomalies are angular parameters used to characterize a planet's motion along an elliptical orbit; the equation reflects, in modern terms, Kepler's treatment of elliptical motion in *Astronomia Nova.*

4. See Guicciardini (*Isaac Newton on mathematical certainty and method*), chapter 13, for further discussion of this lemma.

5. See Wilson ("From Kepler's laws, so-called, to universal gravitation: empirical factors"; "Predictive astronomy in the century after Kepler") for thorough discussions of post-Keplerian astronomy, in particular the competing views regarding the area rule and related questions.

6. Wilson, "From Kepler's laws, so-called, to universal gravitation: empirical factors," p.128.

7. H. W. Turnbull et al., eds., *The Correspondence of Sir Isaac Newton*, vol. 1–7 (Cambridge: Cambridge University Press, 1959–1977), II, p. 144.

8. Derek T Whiteside, "Newton's early thoughts on planetary motion: a fresh look," *The British Journal for the History of Science* 2, no. 02 (1964): 117–137.

9. Streete appealed vaguely to a magnetic attraction that keeps the parts of the Earth together, and maintains satellites in their orbits, but ascribes the motions of the planets around the sun to a vortex.

cussion of predictive astronomy. Descartes had no reason to expect the details of planetary trajectories to be stable over long periods of time, since the motion of our vortex depends on interactions with other vortices. Providing a quantitatively precise underpinning for regularities such as those observed by Kepler had no justificatory force, since these regularities may be only transient. Instead Descartes justified his account based on the clarity of its basic principles and their ability to provide unifying explanations of a wide array of celestial phenomena, from sunspots to the formation of planets to novae (stars of variable brightness).[10] Despite his debt to Descartes, Newton often sought to link vortices to predictive astronomy. For example, in annotations to Wing's *Astronomica Brittanica*, Newton briefly considered the possibility of explaining lunar inequalities based on the interaction between the Earth's vortex, which carries the moon, and the solar vortex.[11]

A well-known exchange with Hooke in 1679-80 led Newton to re-conceptualize motion along a curvilinear trajectory.[12] In his initial letter, Hooke sought Newton's opinion regarding his own hypothesis (initially proposed in 1666) that the motion of the planets should be regarded as composed of motion along the tangent with an attractive motion directed towards a central body. In response, Newton proposed an experiment to illustrate the effect of the earth's rotation on the motion of a falling body, and sketched the trajectory of a body allowed to fall through a hollowed-out region into the interior of the earth. Hooke objected that the body should not spiral down toward the center of the Earth, as Newton had drawn, and the ensuing debate focused on how to calculate the trajectory of such a freely falling body. In the final letter of the exchange, Newton treated the trajectory in terms of a tangential motion compounded with "all the innumerable converging motions successively generated by the impresses of gravity.".[13]

Hooke was probably mystified by how Newton arrived at the sketched trajectory. Newton noted that the motions generated by gravity are "proportional to the time they are generated in," and his argument turns on comparing the total time elapsed (and hence the "impressions" due to gravity along the way) along different parts of the trajectory. This is a subtle argument, since the total time elapsed along a trajectory depends upon its length as well as the velocity of the body traversing it, but the trajectory itself is not given. Newton may have found the trajectory based on his earlier fluxional treatment of the "crookedness" (curvature) of a curve. In earlier work (ca. 1671), Newton had generalized the treatment of uniform circular motion to motion along an arbitrary curvilinear trajectory based on this concept.[14] Nauenberg[15] finds a trajectory for a body falling under a given force law based on a plausible reconstruction of the reasoning behind the letter to Hooke employing the curvature concept. The calculation nowhere relies on Kepler's second law, which is obscured by the approximations used in this approach.

Kepler's area law is easier to see if the action of gravity is treated as a deflection from inertial motion. Newton later credited Hooke for suggesting this approach, which he first developed in detail in *De Motu* (1684). There Newton built on a generalization of Galileo's treatment of uniformly accelerated motion. Newton stated as a hypothesis that the deviation from an inertial trajectory produced by *any* centripetal force is proportional to the square of the elapsed time, "at the very beginning of its motion". Galileo's result holds for *finite* elapsed times in the special case of uniform acceleration, but Newton recognized that it is valid *instantaneously* for arbitrary centripetal forces. The first theorem of the *De Motu* established Kepler's area law, and it follows directly from Newton's conception of force and inertial motion, provided that the

10. The discussion of planetary motion is limited to III.141-145, in which Descartes considers deviations.

11. Whiteside, "Newton's early thoughts on planetary motion: a fresh look."

12. The original letters appear in Turnbull et al. (*The Correspondence of Sir Isaac Newton*, II); cf. Gandt (*Force and Geometry in Newton's* Principia, pp. 136-155) for an overview of the correspondence, which I draw on here.

13. Turnbull et al., *The Correspondence of Sir Isaac Newton*, II, 307.

14. The centrifugal force required to maintain an object of unit mass in uniform circular motion is given by $\frac{v^2}{r}$; Newton generalized this to the requirement that the component of the instantaneous force orthogonal to the velocity is given by $\frac{v^2}{\rho}$, where ρ is the radius of curvature of the curve at that point.

15. Michael Nauenberg, "Newton's early computational method for dynamics," *Archive for History of Exact Sciences* 46, no. 3 (1994): 221–252.

magnitude of the force varies only with distance from a force center. Combining these two results leads to an expression relating the magnitude of the force to geometrical properties of the trajectory. In *De Motu* Newton applied the result to several cases, including the case raised by Hooke (an ellpitical trajectory, with an inverse-square force directed at once focus).

Returning to the comet of 1680, it is not entirely clear what method Newton would have employed in considering a problem in orbital dynamics when he and Flamsteed began their exchange. At the outset of the exchange, Newton apparently accepted what was then conventional wisdom, going back to Kepler, that comets move along straight lines. He criticized Flamsteed's proposal that the observations were of a single comet, due in part to mistakes regarding the observational data.[16] In response to Flamsteed's implausible explanation of the comet's sharp button-hook in front of the sun, Newton considered how to treat a comet's motion in terms of an attractive force towards the sun. In the correspondence, Newton argued that the only plausible one-comet view must have the comet pass around the sun. In draft letters to Flamsteed, Newton claimed to have a method to determine the comet's motion "almost to as great exactness as the orbits of the planets," without elaboration. He may have employed whatever methods he had used in answering Hooke's letters (what Nauenberg calls the "curvature method"), or he may have already developed the ideas recorded in the *De Motu*. The lack of evidence to settle this question should not obscure the more fundamental point: Newton had (partially) developed the mathematics necessary to move back and forth from proposed features of a force law to trajectories. The comet of 1680 posed a timely challenge: could these same techniques provide a basis for calculating a cometary trajectory? And if so, how does the force responsible for cometary motion compare to that responsible for planetary motion? Newton had identified these as the most important questions regarding comets by the time of the *De Motu*, but his correspondence with Flamsteed and manuscripts from around that time reveal that these questions were still entangled with open questions regarding the aether and the constitution of comets.

Newton recorded a number of propositions regarding comets in a manuscript (U.L.C. manuscript Add 3965.14 fol. 613r-v), partially translated by Ruffner.[17] It was clearly written sometime in the years 1681-84, based on references to the 1680 comet and comparison to views in the *De Motu* and *System of the World*. At the outset Newton states basic commitments of a vortex theory (Ruffner, "Newton's Propositions on Comets: Steps in Transition, 1681–84," p. 263):

2. The matter of the heavens is fluid.
3. The matter of the heavens revolves around the center of the cosmic system in the direction of the courses of the planets.

Yet the propositions providing a more detailed description of the comet's trajectory refer to gravity regarded as an attractive force, without reference to the aether:

5. There is gravitation toward the centers of the sun and each of the planets, and that toward the center of the sun is far greater.
6. That gravitation in things diminishes in duplicate ratio to the distance from the center of the sun or a planet as they recede from the surface of the sun or planet.
7. The motion of a comet is accelerated until it is in perhelion and retarded afterwards.
8. A comet does not travel in a straight line but in some curve the maximum curvature of which is at the minimum distance from the sun, the concave part faces the sun, and the plane passes through the sun, and the sun is in its near focus.

16. See Wilson ("The Newtonian achievement in astronomy.") for further discussion of the correspondence.

17. JA Ruffner, "Newton's Propositions on Comets: Steps in Transition, 1681–84," *Archive for history of exact sciences* 54, no. 4 (2000): 259–277.

9. The angular motion of a comet around the sun is very nearly reciprocal to the distance from the sun. Whence the motion would be uniform only if performed in a straight line.

(10.) That curve is an oval if the comet returns in an orbit, if not [the curve] is nearly a hyperbola.[18] [...]

Newton clearly brings comets within the realm of gravity, characterized as an inverse-square force. The conception of "force" employed in these propositions adumbrates the *Principia*'s mature formulation. Proposition 5 hints at an what he would later call the absolute measure of gravity (proportional to mass), but it does not reveal how to measure the magnitude of the force towards the sun compared to that towards the planets. Proposition 9 is a step towards formulating Kepler's area law. Suppose that "angular motion" is measured by an angle multiplied times a radial distance, θr. Then the first part of Proposition 9 states that θr^2 is very nearly a constant – or, in modern terminology, that angular momentum is very nearly conserved, which is equivalent to Kepler's area law.[19] Newton does not, however, explicitly define "angular motion" in this manuscript, and it is unclear why he takes the result to hold only "very nearly" rather than exactly. Proposition (10) suggests the possibility of highly eccenctric elliptical orbits, but at the time of composition Newton presumably could not yet prove (as in the *Principia*) that conic sections exhaust the possible trajectories under inverse-square gravity.

The final proposition noted that the comet's trajectory passed within Mercury's orbit. The nature of the trajectories posed obvious problems for an aether theory. Although Newton does not raise the issue in this manuscript, he must have wondered how to reconcile the motion of the comet, passing through a range of distances from the sun, with an aetherial vortex. This was an even more acute problem for comets, like the one Newton observed in 1664, that move around the sun with an orientation opposite to that of the planets.

By the time of the initial *De Motu* manuscript, Newton had completed the transition to treating celestial dynamics in terms of an inverse-square force. The early propositions specify the connections between Keplerian regularities and the force of gravity. In the case of planets, Newton stated in a scholium in the first *De Motu* manuscript that the planets move in their orbits "exactly as Kepler supposed." Insofar as an inverse-square centripetal force sufficed to explicate the phenomena, there was no need to introduce anything further, such as an aether. Newton took a substantial further step towards *universal* gravitation in a revised manuscript (called Version III by Herivel[20]). Given that there is an inverse-square force pulling the planets towards the sun, and pulling satellites towards their respective planets, it was natural to consider the combined effect of these forces. Newton did so and discovered a striking consequence: the sun and planets orbit a common center of mass, with the planets moving along enormously complex trajectories rather than closed elliptical orbits. The sophisticated methodology of the *Principia* reflects Newton's response to the challenge of inferring the force in such a case, demonstrating the fruits of basing a treatment of celestial dynamics on his concept of centripetal force despite the complexity of real motions.

Perhaps a centripetal force was not *necessary* to recover the phenomena, however, even if it was sufficient. Newton's brief argument against the possibility of vortices in Version III is worth quoting at length:[21]

> Thus far I have considered the motion of bodies in non-resisting media: so that I may determine the motion of celestial bodies in aether. But as far as I can judge the resistance of pure aether is either nothing or excessively small. Quicksilver resists strongly, water much less, air certainly far less again. These media resist according to their density which is almost proportional to their weight and so they resist (or rather almost resist) according to the quantity of their

18. This proposition is not numbered in the manuscript, unlike the others, but it occurs after Proposition 9. These are followed by 5 propositions regarding cometary tails.
19. As noted by Ruffner, "Newton's Propositions on Comets: Steps in Transition, 1681–84," pp. 266-67.
20. Herivel, *The Background to Newton's* Principia.
21. Ibid., pp. 301-02.

> solid matter. … [A]ether penetrates freely but does not offer sensible resistance. That comets descend below the orbit of Saturn is the opinion of all those sounder astronomers …: these comets are therefore carried with immense speed indifferently in all parts of our heavens yet do not lose their tail nor the vapour surrounding their heads [by having them] impeded or torn away by the resistance of the aether. And the planets actually have now persisted in their motion for thousands of years, so far are they from experiencing any resistance.
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> Motions in the heavens are ruled therefore by the laws demonstrated.

Newton explored similar questions in other earlier manuscripts. Aether resistance would differ from other kinds of resistance; a gravitational aether must penetrate bodies, so that its effects depend on mass rather than merely surface area. In the *De Grav*, Newton briefly comments that the motion of projectiles, pendulums, and comets fail to reveal an appreciable resistance due to the aether. The *Principia* reports the results of a pendulum experiment to test whether there is any measurable *internal* resistance attributable to motion through the aether. As Ruffner[22] points out, the estimate of resistance in *De Grav* differs by several orders of magnitude different from that in the *Principia*, challenging Dobbs's claim that these two different sources report the same experiment.[23] The case against the aether based on celestial motions requires would have been difficult to make prior to the understanding of the dynamics developed in the *De Motu*, needed to assess the consequences of adding a resistance force.

The *De Motu* transformed the most pressing cosmological questions faced by Newton and his contemporaries. Rather than treating the details of predictive astronomy as largely irrelevant to understanding the solar system, following Descartes, Newton showed how observed celestial motions could reveal underlying dynamical principles. This transformation was not restricted to technical issues in astronomy; Newton's criticisms of Descartes extended beyond the vortex theory to his doctrines regarding the nature of body, space, and motion. Although the *De Motu* only hints at these broader issues, the earlier *De Grav* is partly devoted to "disposing Descartes's fictions." By the time of the *Principia* Newton had developed a novel natural philosophy to take the place of discarded Cartesian doctrines.[24] But it is also the case, as I will emphasize in the next section, that the transformation in understanding the interplay between astronomy and natural philosophy was not complete in the *De Motu*. Newton's realization of the complexity of planetary motions in Version III led him to develop a sophisticated new understanding of how to make progress in natural philosophy. In the next section, I will briefly describe Newton's program for further work in celestial mechanics alongside his criticisms of vortex theories.

3. AGAINST VORTICES

In the *Principia*, Newton extended and strengthened the three lines of thought that initially led him to abandon vortices. First, the argument that various observations put an upper bound on the aether resistance was not based on a full account of fluid resistance, which Newton aimed to provide in Book 2. Second,

22. JA Ruffner, "Newton's *De gravitatione*: a review and reassessment," *Archive for History of exact Sciences* 66, no. 3 (2012): 241–264.

23. Betty Jo Teeter Dobbs, "Newton's Rejection of the Mechanical Aether: Empirical Difficulties and Grounding Assumptions," in *Scrutinizing Science: Empirical Studies of Scientific Change*, ed. Arthur Donovan, Larry Laudan, and Rachel Laudan, vol. 193 (Dodrecht: Kluwer, 1988), 69–83.

24. On my view, the *De Grav* itself is a transitional document, and represents a step along the way to Newton's mature view in the *Principia*; several aspects of the *Principia*'s novel dynamics are not taken into account in the *De Grav* (see also Zvi Biener, "*De Gravitatione* Reconsidered: The Changing Role of Geometrical Definitions in Newton's Metaphysics of Space," *Forthcoming in the* The Journal of History of Philosophy).

Newton constructed a quantitatively detailed vortex theory to show that it did not, like his account of centripetal force, provide fertile ground for celestial mechanics. Third, the *Principia* included a fold-out figure depicting Newton's calculated trajectory for the comet of 1680/81. Although Newton had anticipated this calculation in his exchange with Flamsteed, and suggested a method in the first *De Motu*, finding a usable method was, as he put it "an exceedingly difficult problem" (3.41) — the solution of which was one of the *Principia*'s major achievements. I will consider the presentation of these three lines of argument in the various editions of the *Principia* in the following, evaluating their strength and impact, before returning to a contrast between vortex theories and the Newtonian program.

3.1. MOTION IN RESISTING MEDIA

The final two problems in the *De Motu* regard projectile motion in resisting media. Newton explored fluid resistance more thoroughly in the *Principia*.[25] Book 2 made several steps towards a solution like that he had achieved for centripetal forces in Book 1: namely, to find the trajctory (up to quadrature) of a body subject to specified resistance forces, given its initial position and velocity.[26] But, more generally, Newton considered a wide variety of problems that he apparently chose as potential sources of empirical insight into fluid resistance, such as the efflux problem and the effects of frictional damping on pendulums. Drawing on this theoretical work, he undertook thorough experiments, using pendulums in the first edition and augmented with experiments regarding freely falling bodies in the later editions. If it had been successful, this line of work would have allowed Newton to infer the properties of resistance forces, in much the same fashion as he used celestial motions to infer universal gravity. However, by contrast with his work in celestial mechanics, Newton's treatment was based on flawed initial assumptions. Some of the problematic assumptions regarding the nature of fluid resistance came to light in criticisms of Newton's solution to the efflux problem, prior to publication of the second edition, but an understanding of resistance sufficient to carry out something like Newton's program would not be attained until the twentieth century.

In the first edition, Newton reported a series of ingenious pendulum experiments. He could not use projectile motion to study resistance, as he had initially suggested in the *De Motu*, because he lacked a fully general solution for projectile motion in resisting media. But a pendulum bob moves along a fixed trajectory, and resistance damps the bob's oscillations. Newton established systematic relationships that hold between this damping and the resistance force, which he assumed to depend upon the relative velocity v of the pendulum with respect to the medium (up to second order in v). Despite their ingenious design, these experiments did not allow Newton to determine the different contributions to resistance, and there were persistent discrepancies in the experimental results. Newton substantially revised Book 2 for the second edition, including a completely different set of experiments.[27] Newton measured the duration of free fall of globes dropped in air (from the top of St. Paul's Cathedral) and in water. Although these experiments were designed with the aim of determining the properties of resistance forces, they were based on a theoretical framework different than that in the first edition. Newton had classified different

25. See, in particular, George E. Smith, "The Newtonian Style in Book II of the *Principia*," in *Isaac Newton's Natural Philosophy*, ed. Jed Z. Buchwald and I. Bernard Cohen, Dibner Institute Studies in the History of Science and Technology (Cambridge, MA: MIT Press, 2001), for a masterful discussion of Book 2, which I draw on here.

26. Newton's treatment of the problem for centripetal forces is in Book 1, §8, where he determines trajectories for an arbitrary central force (up to quadrature). Throughout Book 2, Newton assumes that bodies move under Galilean gravity (uniform gravity directed along parallel lines) through a medium with resistance proportional to v, v^2, or a linear combination of both terms.

27. Errors in the initial treatment of the efflux problem, noted by Johann Bernoulli and communicated to Newton while the second edition was underway, triggered some revisions; but Newton had independent reasons for dissatisfaction with pendulum experiments before that.

types of resistance as arising from different properties of a fluid; in the second edition, he argued that the dominant contribution to resistance, namely "inertial resistance," was due to impacts between the particles of the fluid and the immersed body. The results he had obtained earlier in Book 2 were sufficient to determine the duration of free fall for globes if this were the only form of fluid resistance. Differences between the predicted result and experimental outcomes would potentially reveal other contributions to resistance, due to the "elasticity, tenacity, and friction" of the parts of the fluid.

Proposition 3.10 states the implications of these experimental studies of resistance for celestial motions.[28] Air resistance comparable to that at the Earth's surface would rapidly slow Jupiter down; based on an earlier result regarding the variation of density with altitude (assuming Boyle's Law), Newton finds that at 200 miles (or greater) above the Earth's surface, however, the air would offer negligible resistance to Jupiter's motion. Resistance in terrestrial experiments is ascribed to "air, exhalations, and vapors," but in celestial spaces or in a vacuum (such as that created by Boyle) there is no cause for resistance. Newton remarks that comets encounter no resistance in 3.L4.C3. In the absence of resistance, Newton concludes, the planets will continue to move "for a very long time" without requiring any external force or source of motion. Newton succinctly presents this line of argument in the General Scholium's second paragraph.

This line of thought did not provide a conclusive argument against the existence of the aether for two reasons, both (at least partially) recognized by the time of the second edition. Newton's argument concerned the effects of an aether regarded as a continuous fluid, offering resistance due to the inertia of parts of the fluid impacting an immersed object. First, many aether theorists did not treat the aether as a continuous fluid in this sense, as illustrated by Newton's own proposed aether theory in the second English edition of the *Opticks* (1717). At best Newton's argument posed a challenge to the aether theorists, to propose an alternative account of the aether's physical properties.

The second problem goes to the core of Newton's physical understanding of fluid resistance.[29] Newton regarded resistance as arising primarily from the inertial properties of the fluid. Leibniz suggested an alternative conception, distinguishing between what he called absolute and respective resistance, where the former appears to be closer to the modern conception of viscosity.[30] Huygens suggested that a subtle, highly agitated fluid may have great "penetrability."[31] Later work, in part stimulated by the flaws in Newton's treatment of the efflux problem, led to the realization that a body moving through a fluid with properties like those assumed by Newton (in particular, with no viscosity) would experience no resistance whatsoever, regardless of the body's shape. This result, known as d'Alembert's paradox, establishes that the study of fluids without viscosity has, to put it mildly, limited applicability to real fluids. Although the significance of viscosity for resistance was not fully understood until much later, alert readers of Newton's Book 2 would have noticed that he had to include an explicit hypothesis regarding viscosity in constructing a vortex theory, to which we now turn.

3.2. VORTEX MOTION

In the closing section of Book 2, Newton constructed a quantitative vortex theory and explored its ramifications for predictive astronomy. Descartes had treated the swirling motion of the aether as generated by the rotation of the central star. Newton considered fluid motion generated by a rotating central body

28. This refers to Proposition 10 of Book 3, and I will use similar abbreviations (including L for lemma, and C for corollary) below.

29. See Smith, "The Newtonian Style in Book II of the *Principia*"; George E. Smith, "Was Wrong Newton Bad Newton?," in *Wrong for the Right Reasons*, ed. Jed Z. Buchwald and Alan Franklin (Amsterdam: Springer, 2005), for further discussion.

30. Marginalia in the *Principia*, also mentioned in correspondence with Clarke.

31. Christian Huygens, *Discours de la cause de la pensanteur* (1690), p. 162.

(a cylinder, in 2.51, or a sphere, in 2.52-53), and assumed that it would eventually reach a steady state of rotation. He derived results for the variation of velocity in the moving medium with distances from the central object, based on treating the forces on a given shell of fluid as balanced when the vortex reached its stable, equilibrium state. Newton introduced viscosity in order to account for the transmission of motion from the central body through the surrounding aether. The earlier results of Book 2 provided little insight into such viscous forces, forcing Newton to state as an explicit hypothesis that the resistance (due to viscosity) is proportional to the velocity gradient.

Newton posed a number of challenges based on his results for a vortex induced by a spinning sphere. He proved that the periodic times of objects immersed in the vortex satisfy $T \propto R^2$ rather than Kepler's third law, and argued that vortex motion is also not compatible with Kepler's area rule. Treating the vortex as flowing along streamlines, one would expect the flow to speed up in narrower spaces. But that would correspond to speeding up at aphelion, where the orbits are more compressed, contrary to the area law and observations. A further challenge related to the consequences of transmission of motion from the central body through the aether. Maintaining the rotation of the aether required that the central body had some "active principle," to continue to drive the vortex, contrasting with Newton's conclusion (in 3.10) that on his account planetary motions persist without any external active principle.[32] Several other corollaries showed that Descartes's speculations regarding the interaction among vortices, and the behavior of objects moving among vortices, cannot be reconciled with this account. For example, Newton argued that it would not be possible for a second sphere (representing Jupiter, say) to maintain a vortex while moving through a larger vortex in a stable orbit around the central body.

By the time of the second edition, Huygens and Leibniz had both responded Newton's discussion of vortices, and neither fully abandoned the aether. Newton's arguments show, at best, that a particular version of a vortex theory fails to be compatible with Keplerian motion, but his arguments do not establish the general claim that all vortex theories must similarly fail. Leibniz's goal in the *Tentamen* (1689) was to recover Keplerian motion within a vortex theory. He treated planetary motion as resulting from "harmonic circulation" and "paracentric motion," corresponding to the tangential and radial components of the planet's velocity (respectively). Leibniz regarded the harmonic motion as due to the uniform rotation of the vortex, whereas the paracentric motion, resulting from a combination of centrifugal force and attraction towards the sun, carried the planets to different distances from the central body, moving across layers of the vortex. The physical underpinnings of this account were not entirely clear, and Leibniz's position shifted between the *Tentamen*, a later unpublished revision, and correspondence. Yet Leibniz's aim is quite clear: he hoped to provide a physical account, compatible with Cartesian restrictions to action by contact, in place of Newton's centripetal attractive force acting through void spaces, while preserving as much of Newton's mathematical description as possible.

Huygens shared Leibniz's concern with providing an understanding of the cause of gravity based on action by contact. His "Discourse on the Cause of Gravity" (published in 1690) includes a 1669 essay on the cause of gravity, and an addendum added in response to the *Principia.* The early essay defends a sophisticated aether model designed to explain terrestrial gravity as a consequence of impacts from an aetherial fluid, swirling at great speed in all directions. In the later addendum, Huygens accepted Newton's case in favor of an inverse-square centripetal force governing the motion of planets while rejecting fully universal gravitation, and briefly described an empirical test to evaluate the contrasting predictions of their competing views for the shape of the Earth. He later proposed extending an account like the one he gave for terrestrial gravity to the attraction of the planets towards the sun in the posthumously published *Cosmotheoros* (1698), emphasizing the contrast between his view and Cartesian vortices.

These two early responses illustrate two related aims that would continue to guide research on vortices

32. Newton also pressed this point in the *Opticks* (1706), illustrated by considering the persistence of vortices in fluids with varying "tenacity" (tar, oil, and water).

well into the eighteenth century: first, to provide a mechanical account of gravity, and second, to emulate Newton's success in accounting for aspects of celestial motions such as Kepler's laws. There were a variety of other considerations that supported continued interest in aether theories; for example, Huygens (and, later, Euler) held that the transmission of light through celestial spaces required an aether. Later aether theories drew on more sophisticated treatments of fluid motion and resistance. Some of this work was inspired by efforts to correct flaws in Newton's treatment of vortex motion. Johann Bernoulli forcefully pressed a fundamental objection in 1730: Newton handled the balancing of forces among shells in the vortex incorrectly, as he did not have a full understanding of torque and angular momentum. This mistake undermines Newton's argument entirely; much later studies in fluid mechanics would show that there is no stable vortex induced by a spherical body in the type of fluid Newton considered.

Newton's arguments initially had little impact among Cartesian natural philosophers, who did not regard the connections with predictive astronomy as particularly important. For example, Malebranche did not revise post-*Principia* editions of *The Search After Truth* to respond to Newton in his elaboration and defense of Cartesian vortices.[33] By the time of the second edition, the assimilation and transformation of Newtonian ideas within mechanics was well underway, as illustrated by Varignon's treatment of motion under central forces in Leibniz's mathematical style. This led to a new phase of research in vortex theories, in which one of the main aims was to recover Keplerian motion from a detailed analysis of fluid motion. The evaluation of aether theories was partly based on their ramifications for predictive astronomy. Euler, a staunch proponent of aether theories, considered the implications of aether theories for a variety of astronomical anomalies, such as the secular acceleration of the moon.

3.3. COMETS

The lines of argument Newton developed in Book 2 are not decisive, since they were based on contentious assumptions regarding fluid resistance and vortical motion. At best these arguments pose a challenge: to construct an aether theory based on plausible dynamical principles that would provide an understanding of the quantitative details of celestial motions. If this includes comets, moving on highly eccentric, oblique, and sometimes retrograde elliptical orbits, the challenge becomes nearly insurmountable.

Any aether theory proposed to account for diverse celestial motions faces a natural question: how do the different vortices or aetherial flows postulated to explain distinct motions relate to one another? For example, how does the vortex carrying the moons of Jupiter relate to the solar vortex in which Jupiter and its moons are immersed? Descartes proposed that Jupiter maintains a stable vortex embedded in the solar vortex, which Newton criticized implicitly in the corollaries to 2.52. Whatever the prospects for an "eddies within eddies" picture for Jupiter and its moons, comets clearly demand a quite different account if they follow the elliptical trajectories described by Newton. How could a cometary vortex or aetherial flow cut across the solar vortex, especially for retrograde comets or those moving obliquely compared to the plane of the planets, without disrupting planetary motions? These would not be transient incursions; the vortex carrying the comet would presumably flow along its highly eccentric elliptical path. The solar vortex would thus be thoroughly threaded and penetrated by cometary vortices, with little hope of recovering the regularities of celestial motions.

David Gregory raised these problems as objections to Leibniz's vortex theory in his discussion of Newtonian ideas applied to astronomy, *Astronomiae physicae et geometricae elementa* (1702). Leibniz's weak responses in the manuscript *Illustratio Tentaminis* (1705) show that these objections run deep. Leibniz

33. See E. J. Aiton, *The Vortex Theory of Planetary Motion* (New York: American Elsevier, 1972), for further details regarding the development of aether theories in this period.

suggested that vortices do overlap without disruption in cases like water waves from stones thrown in a pond, or sound waves in air. Yet further considerations of such cases actually undermines Leibniz's case: the vortices in such cases interfere with one another, and provide no defense for the implausible idea that an entangled mess of cometary and planetary vortices could produce stable motions. Leibniz further questioned whether comets obey the same regularities as planets, in particular Kepler's second law. This was potentially a better objection, as Newton's success with the comet of 1680/81 in the first edition might have been a lucky accident.

By the time of the second edition, Newton had made progress toward achieving, as he had claimed in the draft letter to Flamsteed, precision in description of cometary trajectories comparable to that of planetary astronomy, applied to many comets. Halley's *Synopsis astronomiae cometicae* (1705) included the orbital elements determined, based on Newtonian methods, for 24 sets of cometary observations. This study provided the first evidence that the cometary orbits were in fact elliptical, with distinct sets of observations reflecting a single comet's periodic return. In 1696 Halley announced to the Royal Society that a single comet had been observed in 1531, 1607, and 1682; there was barely enough time before its next return in 1759 for astronomers to develop the methods needed to calculate the time it would reach perihelion more precisely. The second and third editions added several more cases of successful applications of Newton's method for determining cometary trajectories.

Newton could mention the stark challenges comets pose for vortex theorists briefly in the General Scholium, since Roger Cotes presented this critique with apparent relish in his editorial preface. It would be hard to improve on Cotes's trenchant summary:[34]

> It all finally comes down to this: the number of comets is huge; their motions are highly regular and observe the same laws as the motion of the planets. They move in conic orbits; these orbits are very, very eccentric. Comets go everywhere into all parts of the heavens and pass very freely through the regions of th eplanets, often contrary to the order of the signs. These phenomena are confirmed with the greatest certainty by astronomical observations and cannot be explained by vortices. Further, these phenomena are even inconsistent with planetary vortices. There will be no room at all for the motions of the comets unless that imaginary matter is completely removed from the heavens.

Cotes's main target was Leibniz's *Tentamen*, which he did not identify in the text but singled out as "worthy of censure" in correspondence with Bentley.[35]

4. Newton's Program

The discussion above has emphasized Newton's critique of vortices at the risk of downplaying the significance of Newton's new mode of inquiry. The *Principia*'s Book 3 treats a variety of terrestrial and celestial phenomena in light of universal gravitation, including the tides, the shape of the earth, lunar theory, precession of equinoxes, and comets. This set a new agenda for work in celestial mechanics, that would eventually realize the benefits of treating celestial motions – and many terrestrial phenomena as well – as consequences of universal gravity. This work reflected a new approach to physics that responds to the challenge of effectively using phenomena to guide inquiry despite their enormous complexity. Although

34. Isaac Newton, *The Principia, Mathematical Principles of Natural Philosophy: A New Translation*, trans. I. Bernard Cohen and Anne Whitman (Berkeley, California: University of California Press, 1999 [1726]), p. 395.

35. Worthy of censure due to Leibniz's claim to have written the *Tentamen* without reading the *Principia*; Cotes was right to doubt this claim.

here is not the place for a thorough discussion of Newton's methodology, I will briefly contrast it with Leibniz's *Tentamen*.[36]

Leibniz claimed in the *Tentamen* to have constructed an aether theory compatible with Kepler's descriptions of planetary motions. Yet the status of Kepler's laws is not entirely clear: in his detailed reconstruction of Leibniz's developing ideas, Bertoloni Meli[37] documents Leibniz's shifting view regarding the status of the third law (among other things). Contemporary critics, notably David Gregory (1702), raised this question as well. I will not go into detail, since the contrast with Newton is clear even if we grant that Leibniz had successfully recovered Kepler's laws. The first contrast with Newton regards the scope of the vortex theory compared to universal gravitation. Terrestrial gravity has to be explained by something other than the solar vortex, to account for accelerations towards the center of the earth, and Leibniz would subsequently endorse Huygens's proposal. Terrestrial motions, the tides, and the motion of the moon are then accounted for by a distinct vortex. As emphasized above (and by David Gregory), comets seem to require another vortex distinct from that governing planetary motions. Overall, then, various phenomena treated as the consequences of universal gravity are regarded instead as resulting from distinct vortices. Second, there is no indication in the *Tentamen* regarding how to respond to the discrepancies between actually observed motions and those predicted by vortices. The empirical assessment of the proposal is limited to a comparison of the general features of motions in a vortex with the regularities observed by astronomers. Perhaps, as with Galileo's treatment of air resistance and friction, Leibniz would have treated the departures from Keplerian motion as resulting from complications that would forever elude systematic theorizing.[38]

By contrast, the mathematical results in Books 1 and 2 enable Newton to draw conclusions about the causes of motion, in spite of the complexity of phenomena. Newton's inferences from phenomena to forces do not depend on an exact description of motion. Many of the results in Book 1 do not require the antecedent of a conditional to hold exactly in order to draw a conclusion. For example, 1.45 establishes a relationship between the apsidal angle (characterizing how much the apsides shift in successive revolutions) and the exponent of the force law governing the motion. Even if astronomical observations only fix an approximate value for the apsidal angle, this proposition implies an approximate value for the exponent. Obtaining results with this level of generality is crucial for the project of inferring features of the underlying dynamics from observed motions.

In addition, Newton carefully identified the idealizations and simplifying assumptions that he required to find mathematically tractable problems. He then took the further step, in the closing sections of Book 1, of considering the consequences of removing various idealizations; proving theorems related to multiple interacting bodies, and bodies of real, finite extent in place of point masses. These further results made it possible to assess the implications of discrepancies between observations and a given idealized model; for example, whether specific departures from Keplerian motion could plausibly be attributed to pertur-

36. Recent literature on Newton, which I draw on here, has led to a much more sophisticated understanding of his methodology; see William L. Harper, *Isaac Newton's Scientific Method: Turning Data into Evidence about Gravity and Cosmology* (New York and Oxford: Oxford University Press, 2011); George E. Smith, "From the Phenomenon of the Ellipse to an Inverse-Square Force: Why Not?," in *Reading Natural Philosophy: Essays in the History and Philosophy of Science and Mathematics to Honor Howard Stein on his 70th Birthday*, ed. David B. Malament (Chicago: Open Court, 2002), 31–70; George E. Smith, "The Methodology of the *Principia*," in *Cambridge Companion to Newton*, ed. I. Bernard Cohen and George E. Smith (Cambridge: Cambridge University Press, 2002), 138–173; Howard Stein, "From the Phenomena of Motion to the Forces of Nature: Hypothesis or Deduction?," *PSA 1990* 2 (1991): 209–22; Howard Stein, "On Metaphysics and Method in Newton," *Unpublished manuscript*, ms. Chris Smeenk and Eric Schliesser, "Newton's *Principia*," in *The Oxford handbook of the history of physics*, ed. Jed Buchwald and Robert Fox (Oxford University Press, 2013), 109–165 provides a more thorough overview of the *Principia*.

37. Domenico Bertoloni Meli, *Equivalence and Priority: Newton versus Leibniz, Including Leibniz's Unpublished Manuscripts on the* Principia (Oxford: Oxford University Press, 1997).

38. See William Harper and George E. Smith, "Newton's New Way of Inquiry," in *The Creation of Ideas in Physics: Studies for a Methodology of Theory Construction*, ed. J. Leplin (Dordrecht, The Netherlands: Kluwer Academic Publishers, 1995), for a thorough discussion of the contrast between Galileo and Newton.

bations from a third body. Newton could then approach observed motions through a series of controlled idealizations, in what Cohen called the "Newtonian style."

Before the second edition, few recognized the fertility of Newton's approach. Newton surely overstated the case in writing to Leibniz that "all phenomena of the heavens and of the sea follow precisely, so far as I am aware, from nothing but gravity acting in accordance with the laws described by me."[39] Newton's treatment of a variety of topics in Book 3 made a plausible case that universal gravity could account for the tides, the shape of the Earth, and a variety of other phenomena, although the details of his treatments did not survive critical scrutiny. Astronomers would not harvest the fruits of treating celestial motions in terms of gravity for several decades, following the development of perturbation methods and other mathematical tools. Within Newton's lifetime, the *Principia*'s positive contribution to predictive astronomy was limited to comets; substantial progress in planetary astronomy based on universal gravity would require several more decades of further work.

5. COSMOGONY

By contrast with the grand sweep of Cartesian natural philosophy, the *Principia*'s first edition says very little about the origins of the world. What was to take the place of Descartes's imaginative, comprehensive cosmogony?[40] In the General Scholium's third paragraph, Newton briefly attributes the regularity of solar system motions to providential design rather than mechanical causes.[41] As is so often the case with Newton, there is a lot behind this brief passage. Newton had developed a speculative cosmogony by the time of the second edition, but revealed very little of this in the *Principia* itself or other publications. My discussion will be limited to two aspects of the context needed to understand Newton's position: first, the further contrast with Descartes it indicates, regarding God's relation to the natural world, and second, Newton's concern with stability of the solar system.[42]

Descartes asserted in his *Principles* that the laws of nature suffice for creating the solar system, with all its structure, from an intial "chaos," as confused as the poets can imagine (III.46-47). Given such an arbitrary starting state, after a sufficient period of time the laws will lead to the structures we observe: stars, planets, comets, and their characteristic arrangement and motions. This account leaves no room for teleology or divine intervention. Descartes was careful to qualify his account: his cosmogony accounted for a world just like ours, and had the benefits of providing a mechanical understanding of how these features could come about. But he regarded it as false, nonetheless, since it apparently conflicted with the cosmogony sanctioned by Scripture (III.45). This attempt at maintaining orthodoxy did not conceal the substantial overlap between Descartes's cosmogony and Epicurean views.

Many of Newton's English contemporaries, including Henry More and Robert Boyle, rejected this Cartesian position, since it left God with no role after Creation. Boyle endowed an annual lecture regarding theology and natural philosophy upon his death in 1691, and the first Boyle lecturer, Richard Bentley, developed a design argument directed against "Epicureans and Hobbists" based on Newton's natural philosophy. In the ensuing correspondence, Bentley pressed Newton to consider a cosmogony that, as in Lucretius' *De Rerum Natura* and Descartes' *Principles*, attempted to account for the solar system without

39. Turnbull et al., *The Correspondence of Sir Isaac Newton*, III, p. 258.

40. I use the term in the same sense as in Cudworth's *True Intellectual System of the Universe*, as regarding the creation or generation of the universe or structures within it such as the solar system; cosmology, by contrast, refers to study, or a theory, of the universe as a whole, typically referring to structures or properties of the universe as a whole rather than their origin.

41. This passage is set apart as a fourth paragraph in the Cohen and Whitman translation, but, as Zvi Biener pointed out to me, there is no paragraph break in the Latin.

42. See David Kubrin, "Newton and the cyclical cosmos: Providence and the mechanical philosophy," *Journal of the History of Ideas* 28, no. 3 (1967): for an extended discussion of these issues.

providential design. Specifically, Bentley apparently asked Newton whether a uniform distribution of matter, of either finite or infinite extent, could lead by "mechanical causes" alone to the formation of the sun and planets, and account for the orbits of the planets and their satellites.[43] Newton argued that both the creation of the sun and planets, and imparting appropriate motions to the planets, required "the counsel and contrivance of a voluntary agent."[44] In Query 23 (later 31) of the *Opticks* (1706) Newton dismissed a Cartesian cosmogony as "unphilosophical," because it seeks an account of the origin of the solar system "out of a Chaos by the mere Laws of Nature."[45]

In addition to marking a theological contrast with Descartes, this line of argument provided Newtonians with a reply to objections such as that raised by Leibniz, namely that the vortex theory naturally explained why the planets, and their satellites, revolve in nearly the same plane and with the same orientation. Leibniz anticipated this reply (writing to Huygens in 1690), and rightly objected that "to have recourse to the decision of the author of nature is not sufficiently philosophical when there is a way of assigning proximate causes."[46] Newton did not explore the prospects for an account of the formation of the solar system based on his gravitational theory, instead attributing qualitative features of the solar system to design. But the passage from the Queries cited earlier goes on to indicate a more pressing concern: that mutual interactions among planets and comets may lead to "irregularities," such that the system will require a periodic "reformation."

The central cosmological role Newton assigns to comets reflects a dramatic over-estimate of their mass. Newton had no way of determining the mass of comets via their gravitational interactions, and estimated that their mass is comparable to that of the Earth indirectly, based on the amount of heat absorbed from the sun and a correlation between density and absorbed heat inferred from the planets (3.41). (The first reliable estimate of a comet's mass took advantage of the closest recorded approach of a comet to the Earth: Lexell's comet (1767) passed within .0151 AU of the Earth, close enough to perturb the Earth's motion. Laplace was then able to place an upper bound on its mass, $1/5000$ the mass of the Earth.)[47] Such massive comets would have appreciable perturbative effects on the planets as they pass through the solar system. It was thus possible that by judiciously deploying comets, the Creator could maintain the regular motions of the planets; on the other hand, comets moving through the solar system on arbitrary trajectories would disrupt the planets. Newton also considered the interactions among comets, and takes the fact that they are widely separated at their aphelia (where they could perturb each other) as further evidence of design. In addition to their role in insuring dynamical stability of the solar system, Newton suggested other restorative roles for comets: cometary tails would replenish the earth and other planets, and comets themselves would replenish the sun via periodic, cataclysmic impacts.[48] Newton regarded this cosmogony as quite speculative, and even as he succeeded in determining cometary trajectories based on gravity, his successors recognized the problems with his account of the nature of comets and the generation of cometary tails.[49]

43. The nature of the four queries Bentley posed seems fairly clear from Newton's response, although we do not have Bentley's original letters. Newton briefly considered how to determine the consequences of gravity in an infinite, uniform distribution of mass, and pursuing this question further leads to foundational questions regarding the nature of acceleration in Newton's theory (related to Corollary VI of the Laws).

44. Turnbull et al., *The Correspondence of Sir Isaac Newton*, III, pp. 233-235.

45. Isaac Newton, *Opticks* ([New York]: Dover Publications, 1952 (1730)), p. 402.

46. Gottfried Wilhelm Leibniz, ed., *Philosophical Essays* (Indianapolis: Hackett Pub. Co., 1989), p. 310.

47. See Tofigh Heidarzadeh, *A history of physical theories of comets, from Aristotle to Whipple*, vol. 19, Archimedes: New Studies in the History and Philosophy of Science and Technology (Springer, 2008), pp. 196-198.

48. Kubrin, "Newton and the cyclical cosmos: Providence and the mechanical philosophy."

49. Heidarzadeh, *A history of physical theories of comets, from Aristotle to Whipple*.

6. CONCLUSIONS

By the mid-eighteenth century, the main Cartesian motivations for a vortex theory no longer held sway. D'Alembert's article on "attraction" in the *Encyclopédie* (1751) treats a restriction to action by contact as completely unfounded:[50]

> When we see that two separated bodies approach one another, we should not be pressed to conclude that they are pushed toward another by the action of an invisible fluid or other body, until experience has demonstrated it [...] How wrong are those modern philosophers who proudly declare themselves opposed to the principle of attraction without giving any other reason than that they cannot conceive how one body can act on another which is distant from it. [...] Nothing is wiser and more in agreement with the true philosophy than to suspend our judgment on the nature of the force that produces these effects.

Accounting for how this Newtonian view came to be widely accepted on the continent is beyond the scope of this essay. The arguments Newton higlights at the outset of the General Scholium surely played an important role in this transformation. Newton abandoned the aether — and the rest of the Cartesian account of body, space, and motion along with it — upon recognizing that his conception of inertial motion combined with an inverse-square centripetal force was sufficient to capture Keplerian regularities, and failing to find any positive evidence of resistance due to the aether's presence. He was characteristically thorough in developing the first quantitative vortex theory, as well as a framework for the study of resistance forces, in order to put these negative arguments against the aether on solid footing. These arguments at best put the burden of proof on vortex theorists to develop alternative accounts of the aether, a challenge that Leibniz, Huygens, and many of their successors took up. The more powerful argument was ultimately the success of Newton's proposal to base celestial mechanics on the law of gravity. At the time of the second edition, the strongest case for this success was Newton's treatment of cometary trajectories; but by D'Alembert's time, the Newtonian approach had led to advances in planetary astronomy, based on new methods for treating perturbations. This progress followed not just from Newton's proposed force law, but also stemmed from a new conception of how to use evidence to guide inquiry.

50. Translated in Curtis Wilson, "Euler on action at a distance and fundamental equations in continuum mechanics," *The investigation of difficult things* (Cambridge), 1992, p. 402.

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