Working Hypotheses, Mathematical Representation, and the Logic of Theory-Mediation

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1. Introduction

In the General Scholium, Newton famously remarks that in the program of "experimental philosophy" that he pursues in the *Principia mathematica* (1687), "hypotheses, whether metaphysical or physical, or based on occult qualities, or mechanical, have no place" (Newton 1999, 943).[1] Newton targets Descartes's vortical explanation of planetary motion as one such hypothesis, and in Section 9 of Book 2, he presents a series of arguments intended to show that the Cartesian hypothesis is incompatible with observed Keplerian planetary motion and cometary motion. Based on these arguments, Newton asserts in the scholium that concludes Section 9 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" (*ibid*, 790, emphasis added).[2]

While Newton was eager to distance his program of natural philosophy from Descartes's, significant questions arise about just how different their methods were when Descartes's "hypothetical" natural philosophy is put into conversation with the portions of Book 2 that bear directly on the argument against the vortex hypothesis, specifically the portions dedicated to the problem of fluid resistance. These have been central to George E. Smith's path-breaking work on the *Principia* and our understanding of Descartes's and Newton's differing methodologies.

The goal of this paper is to articulate Smith's insights and to explore what we believe are some of their more radical implications. First, we examine a few of the apparently 'Cartesian' moves that Newton makes in Book 2 and highlight how Smith's notion of 'working hypotheses' can be used to

[1] The General Scholium was added to the second edition *Principia* (1713) and retained in the third (1726). A similar though less forceful dismissal of "hypotheses" can be found in Newton's correspondence. See the passage appended to Note 33 below. See Shapiro 2004 for discussion of Newton's use of the term "experimental philosophy" to separate his program of natural philosophy from the "hypothetical" natural philosophy that he associates with Descartes and Leibniz.

[2] The calculations that Newton uses to establish the incompatibility between Descartes's Vortex Hypothesis and Kepler's Area Rule rest on incorrect assumption, as first pointed out by George G. Stokes in a paper of 1845. See Cohen 1999, Chapter 7 and Smith 2005, Notes 4 and 5 for further discussion of Newton's errors.

draw a clear line between Newton's method and Descartes's (§2, §3). The notion of 'working hypotheses' gives us further occasion to elaborate on the more general logic of theory-mediation that Smith attributes to Newton (§4). We focus specifically on the constitutive role of theory-mediated working hypotheses in individuating new phenomena. We locate in Smith's framework two types of evidence to which working hypotheses can appeal: what we call 'independent and 'conditional' evidence. Viewing Newton's methodology in light of this distinction allows us to extend Smith's account of theory-mediation (in §5). By considering his proposal (in Smith 2001b) that the laws of motion are also best considered working hypotheses, and by connecting the laws of motion with the working hypotheses of Book 2, we show that Smith's nuanced portrait of Newton's non-Cartesian methodology opens up a way of appreciating the foundational role of mathematics for physical reasoning. On our reading, Newton's method rests on the condition that, to serve as evidence at all, natural forces and natural motions must be described in mathematical terms.

Ours is a more Kantian image of Newton's method than Smith's. But, as should be evident from what follows, it is an image that we could not have drawn if it weren't for Smith's distinctive insights into Newton's way of bringing together theory and evidence in the *Principia*.

2. Fluid Resistance and Working Hypotheses in the First Edition of the *Principia*

In all editions of Book 2 of the *Principia* (1687, 1713, 1726), Newton supposes that the overall resistance encountered by a body moving in a fluid arises from different features of the fluid, such as the fluid's inertia and what today we call viscosity. He considers each of these to be due to *independent* physical mechanisms and represents the contribution of each mechanism to the overall resistance encountered by a body as a function of the body's velocity relative to the fluid.[3] Put differently, in all editions of Book 2, Newton represents the resistance due to each physical mechanism as one of several additive components ($a_n v^n$) that contribute to the total resistance encountered by a body ($F_{Resistance}$). We call this Newton's 'fundamental assumption' of fluid

[3] In the third edition *Principia*, for instance, Newton holds a three-mechanism view such that the overall resistance acting on a body moving through a fluid is produced by the fluid's density, internal friction, and tenacity (or absence of lubricity, or slipperiness), and in this case, the tenacity is taken to be independent of the moving body's velocity, whereas the internal friction is taken to be proportional to the body's velocity. See the Scholium to Book 2, Section 3 (Newton 1999, 678-678) and Smith's contribution to Cohen 1999 (pp. 188-194).

resistance.[4] In its most general form, it can be represented as follows, allowing that n may be fractional:

$$F_{Resistance} = a_0 + a_1 v + a_2 v^2 + \cdots + a_n v^n \qquad (1)$$

In the first edition of the *Principia*, v^2 is presented as the dominant term and taken as the effect of the moving body pushing the fluid medium out of its way (i.e., the effect of the body overcoming the inertia of the medium). The other two additive components – the a_0 and $a_1 v$ terms of the fundamental assumption – are both taken to arise from what Newton refers to as a "defect of lubricity," or slipperiness, in the fluid medium (Smith 2005, 157).[5] In other words, with these considered to be independent physical factors, Newton had to establish "separate force laws for [these] different mechanisms of resistance" (Smith 2005, 134). This meant he had to disaggregate the contribution of the fluid's inertia from the fluid's lubricity in such a way that he could identify the appropriate coefficients for each of the additive components above.

In the first edition, Newton tried to accomplish this disaggregation by means of pendulum experiments.[6] Assuming air resistance is the primary cause of pendulum decay, he expected the arc loss per swing to be a function of the various powers of v, according to the fundamental assumption. The arc loss could then be expressed as follows, with A_i being constants for a given pendulum:

$$\delta_{arc} = A_0 + A_1 V_{max}^1 + A_2 V_{max}^2 + \cdots A_n V_{max}^n \qquad (2)$$

[4] Although Smith's account of Newton's reasoning in Book 2 isn't cast in exactly the same terms, our rendering of Newton's 'fundamental assumption' of fluid resistance captures the additive view of fluid resistance that Smith attributes to all editions of the *Principia* (See Smith 2000, 2001a, 2005). Our rendering is also sufficiently general to accommodate the fact that in different editions of Book 2 Newton treated the overall resistance encountered by a body as arising from different physical mechanisms (Smith 2005, 157).

[5] In other texts, including the later editions of the *Principia*, Newton used to term "tenacity" (*tenacitas*) to refer to the absence of slipperiness, or lubricity, in the fluid. See Smith's contribution to Cohen 1999 (pp. 188-194).

[6] Pendulum experiments are useful for studying projectile motion since their properties are easier to measure than the properties of free fall (although Newton ended up using free-fall measurements in the second edition of the *Principia*). Moreover, Newton lacked a fully general solution for projectile motion in resisting media, so constrained motion proved important.

By starting the pendulum at different heights, Newton was able to generate different sets of results, which allowed him to infer the values of A_i by solving simultaneous equations.[7]

However, the overall data that he collected presented Newton with some problems. He tried several linear combinations:[8]

$$A_0 + A_1 V + A_2 V^2$$

$$A_1 V + A_2 V^2$$

$$A_{1/2} + A_1 V + A_2 V^2$$

$$A_1 V + A_{\frac{3}{2}} V^{\frac{3}{2}} + A_2 V^2$$

But the result was always the same: No matter which combination he chose, he could not establish stable values for all of the A_i, or even establish a stable range of values that accommodated all of the measurements that he collected from the pendulum experiments. Ultimately, in the published edition of the first edition of the *Principia*, Newton used the final linear combination listed above, and he presented values for the A_i based only on a subset of the experimental data that he had collected. He offered no explanation for either of these choices.

The arbitrary, even speculative, character of Newton's reasoning in this case may well seem "hypothetical". As smith notes, the failure to fix the A_i and determine the relevant powers of v (i.e., the failure to disaggregate the various contributions to resistance) indicates that Newton's pendulum experiments provided no real test of his fundamental assumption. In other words, the experiments did not entail—although they were compatible with—Newton's physical understanding of the causes of resistance.[9] This is why the charge of Cartesianism arises. Descartes's physical explanations often

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[7] To calculate a single swing arc loss, Newton actually measured the loss of 1/8 or ¼ of the overall arc, and divided by the number of swings. He worked out how to express arc loss per swing in terms of pendulum length, and total resistive force as a ratio to bob weight. He used a cycloidal pendulum, where maximum velocity is proportional to overall arc length. For additional details see Propositions 30 and 31 of Book 2 of the first edition *Principia*, and Smith 2001a, 259ff.

[8] The v^2 term is common because at high velocities the total resistance varied as nearly v^2. See Smith (2000, 130, footnote 19).

[9] Smith notes that "[r]egardless of why Newton presented the findings in the way he did… [the] experiments do not begin to allow reliable conclusion to be drawn about contributions to resistance involving an exponent

posited microphysical mechanisms that were compatible with the phenomena they were meant to explain, but were by no means uniquely entailed by them. Moreover, those microphysical mechanisms lacked independent evidence. They seemed plausible from a mechanical point of view, but there was no reason to believe in them except for their (purported) success in saving the very phenomena they were meant to save. This is what we mean when we call them "hypothetical." For example, in Part III of the *Principles*, Descartes asks us to imagine that the insensible particles that make up fire have both a rectilinear and a circular motion. When they collide with other bodies, we are also to imagine that these particles adapt their shape in such a way that they can fill the narrow spaces between larger pieces of matter (Descartes 1984, 110ff; III.52ff). Descartes supplies this imagery to explain, among other things, why light is emitted from every part of the Sun and why it travels in straight lines (Descartes 1984, 115-118; III.60–64).[10] Newton's fundamental assumption, as articulated above, seems methodologically similar. His treatment of resistance and Descartes's treatment of fire both rely on claims about the behavior of unobservable micro-mechanisms that are supposedly responsible for the phenomena they are trying to explain, but which are not entailed by these phenomena, and for which there is no other evidence. It is in this respect that it becomes fair to ask: Was Newton guilty of pursuing the Cartesian brand of natural philosophy that he publicly rejected?[11]

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of v less than 2." Moreover, despite Newton's efforts to focus on this term in subsequent experiments (some in water), here too "his results were disappointing" (2001a, 262).

[10] For a critical discussion of the "hypothetical" explanations that Descartes presents in Part III of the *Principles of Philosophy*, see McMullin 2008.

[11] Other examples from Book 2 could also be used to motivate the same question. Perhaps the most famous among them is Newton's treatment of the speed of sound in the second edition *Principia*. In Book 2, Section 8, Newton compares his theoretical value for the speed of sound with the experimental data that had been compiled by William Derham, and he reports a perfect fit between the two (Newton 1999, 778). But to establish this fit, Newton relies on the alleged existence of "vapors lying hidden in the air," which have a "crassitude" that increases the speed of sound. Newton has no direct evidence of these hidden vapors, just as Descartes has no direct evidence that the insensible parts of fire adapt their shapes to fill the spaces between the parts of matter. Both are posited as generally intelligible but empirically untestable ways to make sense of what has been empirically verified. For the debate over whether Newton's introduction of the "crassitude" of air is best viewed

Smith has given us good reasons to reject this reading. He has long argued that Newton's model of theorizing departed substantially from the Cartesian hypothetico-deductive model (CHD), even in the context of fluid resistance.[15] A full discussion of Smith's rich account of the "Newtonian Style" is beyond our scope, but a quick contrast with CHD will bring out the features with which we will be primarily concerned in this essay. On the CHD, the theoretician's role is to construct theories whose predictions agree as nearly as possible with observed phenomena.[16] When predictions and phenomena agree, the theoretician has accomplished her task. When they don't, it's back to the drawing board—the theoretician must modify her existing theories, perhaps even replace them. *How* she modifies or replaces them, however, is entirely up to her. Because the only empirical evaluative criterion for the success of a theory is that the theory, *taken as a whole,* saves the phenomena, a mismatch between theory and observation provides no information about which theoretical changes are necessary. The phenomena only indicate whether a theory fits better or worse than its competitors. This is why competing but empirically equivalent theories, are a real prospect for the CHD. In the CHD, at least generally, information flow is unidirectional. Theories tell us what to think about the world, but the world doesn't tell us what to think about the content of our theories.[17]

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as a "fudge factor" or as a good faith attempt to account for Durham's results, compare Truesdell 1970 and Westfall 1973 with Cohen 1999, 361-362.

[15] See Smith 2001a, 2002a, 2005, 2016. In Smith 2002a and 2016, Smith specifically contrasts Newton's methodology with the Cartesian hypothetico-deductive method as it is described by Christiaan Huygens in the Preface to his *Treatise on Light* (1690) (Smith 2002a, 139-140; Smith 2016, 189). For discussion of how Newton's method for establishing universal gravitation departs from a more generally construed hypothetico-deductive model of scientific reasoning, see Ducheyne 2012 and Harper 2011.

[16] We are not using "prediction" in a technical sense. The point is simply that however Cartesian theories (or models, qualitative descriptions, etc.) make claims about the world, the Cartesian theoretician seeks agreement between those claims and the world.

[17] Not only can the world not tell the Cartesian how to revise her theory, arguably, the world cannot even tell her if the theory is generally true. This is because, at least as Descartes presents it in Part III of the *Principles*, the hypotheses that are posited to explain observed phenomena are not to be accepted as true, no matter how much empirical evidence might be amassed in their favor. Hypotheses are more or less acceptable depending on their consistency with Descartes's metaphysically derived laws of nature, and depending on their

In contrast, information flow in the Newtonian framework is bi-directional.[18] Smith stresses that Newton tried to construct theories by means of "if-and-only-if" propositions that mutually bind some theoretical feature and some feature of the phenomena.[19] These propositions allow both for seeing what observations would follow given certain values of relevant theoretical parameters, and for seeing what values these parameters would take given certain observations. The latter allows observations, in effect, to measure theoretical parameters in light of previously established "if-and-only-if" propositions. As Smith puts it, it allows observations to measure theoretical parameters because some relevant theory has mediated the measurement process.[20]

The indispensable role of theory is worth stressing here. Not only would phenomena have no theoretical significance without the mediation of some theory, they would not even be constituted *as phenomena* without that theory. For example, it is possible to measure the acceleration of a falling projectile without a sophisticated theory (albeit not without great ingenuity), but there is nothing in the phenomena of free-fall that corresponds to the direction of the gravitational force outside a theory of force and motion. Because such 'theory-mediated' measurements presuppose a theoretical background, when measurements go wrong—i.e., when they cannot be made to agree with one another or with other relevant measurements—they implicate the parts of the theory that made them possible and thus point to possible revision. As Smith puts the contrast with CHD, the aim of Newtonian theory is to "find ways in which the world [c]ould provide conclusive answers to theoretical questions," not just ways to "conjectur[e] answers and then [test] the implications of these conjectures" (Smith 2002a, 147; 2016, 197-8). In other words, a primary aim of the Newtonian theoretician is to construct a theory sufficiently powerful to allow the world to force her

consistency with the phenomena. But in general, every theory that Descartes posits to explain the visible world, including his vortex theory of the heavens, is "to be taken only as an hypothesis {which is perhaps very far from the truth}" (Descartes 1984, 105, III.44). For further discussion, see Domski 2019.

[18] This line of thought is further developed in Biener 2016.

[19] Smith also notes that when "[Newton] is unable to establish a strict converse [of an if-then statement], he typically looks for a result that falls as little short of it as he can find" (Smith 2002a, 146; Smith 2016, 197).

[20] Ideally, for these inferences to count as good measurements, additional constraints are necessary. For example, they should submit to *quam proxime* reasoning, as emphasized in Smith 2002a and Smith 2016.

hand in future theory development. This theoretical coercion, if you will, is the centerpiece of the "Newtonian style." Newton's claim that that the law of gravity was "deduced" from the phenomena takes on new significance in this light (Newton 1999, 790). The claim is not merely that there is some empirical support for the law. It is that within the theoretical framework of the laws of motion, and in light of the then-current best measurements of planetary and terrestrial motions, the world constrains further theory choice to such an extent that accepting the law of gravity becomes the most reasonable course of action.[21]

Smith has also argued that Newton's treatment of motion in resisting media is methodologically consonant with his deduction of gravity (cf. Smith 2001a). In particular, Smith has provided a means of re-describing Newton's seemingly arbitrary hypotheses in a way that sidesteps the Cartesian paradigm. For Smith, Newton's hypotheses—like the fundamental assumption—are best viewed as "working hypotheses" (cf. Smith 2000 and 2005). Although these are not directly entailed by the phenomena, they are not final pronouncements either, as Descartes's hypotheses seem to be.[22] Rather, they are theoretical posits—perhaps first approximations—that play a critical role in theory development. Specifically, they facilitate theory-mediated measurements that otherwise would have been impossible. By so doing, they let new evidence be constructed and deployed for/against the theory in question. In essence, they enable new questions to be put to the

[21] We say "the *most* reasonable" because theoretical choice is not *entirely* constrained. Questions regarding the inductive scope of accepted claims—how they are "rendered general by induction"—remain open. A more guarded reading of Newton's claim that gravity is "deduced from phenomena" is that within the theoretical framework of the laws of motion, given the then-current best measurements of planetary and terrestrial motions, and *within the range of then-current measurements*, the world constrains further theoretical choice to such an extent that accepting the law of gravitation *within that range* is the most reasonable choice. As Newton notes in the General Scholium, his evidence shows that gravity extends "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" (589).

[22] We say "*seem* to be" because we believe the traditional reading of Descartes is not entirely correct. As we read Part III of the *Principles*, and as noted in Note 15 above, Descartes is not offering his hypotheses as true and final pronouncements about the workings of nature. They are explanatory devices that, he says, could be "very far from the truth," and consequently, they are revisable and essentially different in kind from the three laws of nature of Part II, which are final and firm pronouncements about the true workings of nature. For further discussion of the "truth" that Descartes associates with his laws of nature, see Domski 2018.

world and empower the world to answer. And because these new measurements implicate the parts of the theory that made them possible, the evidence they generate is not for/against the theory *simpliciter*, but for/against those very working hypotheses. When things go wrong—when negative evidence accrues—they point the way to theoretical revision.

Smith grants that the fundamental assumption that guides Newton's treatment of motion in resistive media in Book 2 is not as robust a working hypothesis as we find in other parts of the *Principia*. In accepting it, the theoretician's hand is not forced to the extent that it is forced in regards, the law of gravitation, for example. Nonetheless, the fundamental assumption, Smith argues, retains the primary virtue of Newton's other working hypotheses. It serves as an enabling posit that opens a fruitful and well-defined pathway for further inquiry, and as such, it gives us a fruitful way of distinguishing Newton's method from Descartes's.

Smith applies a similar framework to the revised sections of Book 2 that were published in the later editions of the *Principia*. In these sections, Newton pursues a different strategy for establishing overall fluid resistance than he did in the first edition. But again, the fundamental assumption plays a central role in his reasoning. And again, he seems to employ the "Cartesian" style hypotheses we described above.

3. Fluid Resistance and Working Hypotheses in the Second and Third Editions of the *Principia*

In Book 2, Section 7 of the second and third editions of the *Principia*, Newton examines the resistance forces on bodies that move through two general types of fluid: rare and continuous.[23] A rare fluid consists of particles that do not interact. Specifically, on Newton's account, a rare fluid consists of non-interacting particles "that are equal and arranged freely at equal distances from one another"

[23] For more general discussions of the problematic and sometimes mistaken reasoning that can be found in these editions of Book 2, Section 7, see Truesdell (1970), Westfall (1973) and the Smith essays cited herein, especially Smith 2005. Rouse Ball's *An Essay on Newton's "Principia"* (1893) is also a noteworthy commentary on Book 2. Ball makes no attempt to hide the mistakes of Section 7, but also finds there "much that is interesting in studying the way in which Newton attacked questions which seemed to be beyond the analysis at his command" (Ball 1893, 99; cited in Cohen 1999, 181).

(Newton 1999, 728).[24] When a body moves through this type of fluid, the fluid's particles impact the body and thus impede the body's motion. However, since the particles do not interact, there is no change in the fluid constitution ahead, behind, or around the moving body. Consequently, in this type of fluid, resistance is treated by Newton as a function only of the front surface area of the body, the density of the fluid, and their relative velocity. Determining the resistance offered by a continuous fluid, in contrast, is more complicated. Real-world examples of this type of fluid include water, hot oil, and quicksilver, and they are continuous insofar as they consist of "solid particles effectively in contact with their immediate neighbors" (Smith 2005, 129). When a body moves through this sort of fluid, it "does not impinge directly upon all the particles of fluid which generate resistance" – as is the case in a rare fluid – "but presses only the nearest particles, and these press others and these still others" (Newton 1999, 735).

To get some purchase on how much resistance the fluid offers in this scenario, Newton first limits his treatment to continuous fluids that are inelastic and lack (in contemporary terms) internal friction and viscosity. He also initially considers only the resistance encountered by spherical bodies that move through the idealized continuous fluid he has defined.[25] He reasons that the inertial resistance encountered by a body moving in this type of fluid is due mainly to the body's needing to push forward the column of fluid ahead of it. By symmetry considerations, this force is equivalent to the force exerted in a gravitational field on a stationary body by the column of fluid above it. The contiguous parts of the fluid press downward on one another to exert some total force (their weight), which, in the non-gravitational case, is the total (inertial) force a moving body needs to overcome when it "presses… the nearest particles, and these press others and these still others". To determine this total weight and thereby generate a model of resistance for spherical bodies, Newton expands on his treatment of the efflux problem – the problem of determining the velocity of water escaping from a hole in a closed vessel.

[24] Newton also briefly discusses motion in an "elastic" fluid, which is a rare fluid in which there are repulsive forces between the particles.

[25] Newton discusses other shapes in the scholia to Propositions 37 and 38. In what follows, we limit our discussion to the case of moving spheres since it is most relevant to Newton's purported refutation of Descartes's Vortex Hypothesis – a hypothesis that concerns the motion of (roughly) spherical bodies through the heavens.

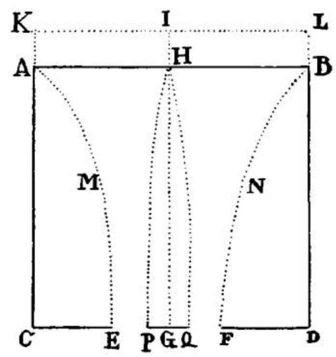

Figure 1: The Efflux Problem and Fluid Resistance in the Second Edition *Principia*

The efflux problem is treated in Book 2, Proposition 36, and its extension to the case of resistance begins in Corollary 7 (see Figure 1; Newton 1999, 740).[26] The specific task is to determine the weight of the column of water supported by the circular disk PGQ, which is at rest with respect to the cylinder ABCD but in relative motion with respect to the descending water effluxing through EP and QF. Earlier in the proposition, Newton asks us to imagine that "the interior of the vessel around the falling water ABNFEM is filled with ice, so that the water passes through the ice as if through a funnel" (*ibid*, 734). In Corollary 7, he further adds that PHQ – the column of water above the resting disk PGQ – should be considered frozen. This is because the fluidity of these regions "is not required for the very ready and very swift descent of the water" (*ibid*, 740). To determine the weight of PHQ – which, as noted above, is equivalent to the force PHQ exerts on the disk PGQ – Newton compares its weight to the weight of the "cylinder of water whose base is that little circle and whose height is GH." He determines that the weight of the column PHQ will be greater than 1/3 of the weight of the cylinder (Corollary 7) and less than 2/3 of the weight of the cylinder (Corollary 8). Then, in Corollary 9, he provides a precise value: "The weight of the water sustained by the little circle PQ, when it is extremely small, is very nearly equal to the weight of a cylinder of water whose base is that little circle and whose height is $^1/_2$GH" (*ibid*, 741). In other words, Newton establishes that the weight of the column of water supported by the disk PGQ is very nearly half the weight of the cylinder of water that has PGQ as its base and a height of GH.[27]

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[26] For discussion of Newton's general (and sometimes mistaken) approach to the efflux problem in the second edition, see Maffioli 1994.

[27] In the first edition of the *Principia*, Newton claimed that a stationary body suspended in the flowing efflux stream of water would support the full weight of the cylinder of water above it. He also claimed in the first

How exactly does Newton justify this value? In the first instance, he reasons that although the specific shape of the column PHQ is not well defined, it's shape (whatever it is) can be set in proportion to the weight of the cylinder of water that has PGQ as its base and a height of GH. Specifically, he asks us to imagine that the cataract of water falling around PHQ "falls with its whole weight and does not rest or press on PHQ but slides past freely and without friction, except perhaps at the very vertex of ice, where at the beginning of falling the cataract begins to be concave" (*ibid*, 740). In this scenario, the angles PH and HQ, which are formed at the vertex of the column, will be convex towards the cataract just as AME and BNF are convex. Consequently, PHQ "will be greater than [that is, it will inscribe] a cone whose base is the little circle PQ and whose height is GH, that is, greater than $^1/_3$ of a cylinder described with the same base and height." (*ibid*, 740). This provides the lower bound. To establish the upper bound, Newton compares the frozen column with a half spheroid "whose base is the little circle and whose semiaxis or height is HG," which meets P and Q at right angles (*ibid*, 741). Because the angles of PHQ are all acute, the entire frozen column above PGQ will "lie within the half-spheroid," and is therefore less than "$^2/_3$ of a cylinder whose base is that little circle and whose height is GH" (*ibid*, 741). Immediately thereafter, in Corollary 9, Newton concludes:

> The weight of the water sustained by the little circle PQ, when it is extremely small, is very nearly equal to the weight of a cylinder of water whose base is that little circle and whose height is ½ GH. For this weight is an arithmetical mean between the weights of the cone and the said half-spheroid. (*ibid*, 741)

Newton's use of the arithmetical mean is elegant. However, Newton does not explain why it is *physically* compelling. Instead, he provides some experimental results concerning the general relation between the velocity of the efflux stream and the width of the efflux hole (*ibid*, 735-736). But none of these results provide any support for his use of an arithmetical mean, which raises questions

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edition that the water escaping the vessel in the efflux problem would have a velocity equal to a body falling from the full height of the tank. In the second edition, both of these values are cut in half, and as a result, in the second edition, Newton lowered the value for the inertial resistance on the front face of the body by a factor of four (Smith 2000, 119-121). See Smith 2001a for a detailed account of Newton's first edition treatment of the weight supported by a resting body in a moving fluid, and also for an English translation of the relevant portions of Book 2, Section 7 from the first edition.

about his claim that the weight supported by the disk PQ is equal to the weight of the cylinder that has a height of ½ GH.

Indeed, looking ahead in the text to the experimental results that Newton presents both to support his general theory of resistance and to disconfirm Descartes's Vortex Hypothesis, one might think that Newton's choice of ½ was reverse-engineered.[28] What's at stake in particular is Newton's theoretical assumption that the force acting on the rear of a body moving through a fluid is negligible. To see the significance of the assumption, consider that when a body moves through a fluid, the volume of fluid that it displaces comes rushing in behind it. It's not unreasonable to think that the force exerted by the rushing fluid on the rear of the body could offset, to some degree, the resistance force on the front. It is also not in principle unreasonable to think that the force acting on the rear of the body could be large enough to entirely counteract the force on the front. On this line of thinking, it would be possible for, say, planets to move in an aetherial fluid without encountering a net resistance, and thus without any observable retardation of their motion.[29]

Newton attempts to block this line of reasoning. He argues on qualitative grounds that, in fact, the force acting on the rear of the body is negligible. For example, he reasoned that in a continuous fluid pressure is propagated instantaneously, and consequently "generates no motion…, [and] thus neither increases nor decreases the resistance" (*ibid*, 744).[30] He offered no direct experimental evidence to support these particular claims; instead, after he's established that the weight of the water sustained by the disk in the flowing efflux stream is one half the weight of the cylinder that is above the disk, what we get, starting in Proposition 40, are the results of a series of free-fall experiments that are based on this theoretical model. The charge of reverse-engineering could arise at this juncture because Newton's choice of ½ is uniquely suited to allow these experiments to confirm the idea that the force on the rear of the moving body is negligible and thus to preclude the possibility of a non-resistive Cartesian aether. In other words, Newton chose a value that left unchallenged his account of the physical mechanisms responsible for resistance. This account includes not only his speculations about the propagation of pressure, but the division of fluids into

[28] Smith explicitly considers this possibility in Smith 2005.

[29] In fact, in 1752, Jean le Rond d'Alembert showed that the drag force on a body moving with constant velocity in an incompressible and inviscid fluid is zero.

[30] The quoted inference may seem unbelievably terse, but Newton's justification of it and its premises is, in fact, unbelievably terse, no more than a few sentences.

physical kinds and the use of weight as a proxy for inertial resistance. Most importantly, it includes the additive, fundamental assumption that was discussed earlier. Without that assumption, Newton could not subtract away internal friction and viscosity from real fluids and thereby isolate inertial resistance.

Taking a closer look at the experimental results that Newton presented in Proposition 40 of Section 7 raises further questions about the testability of the principles that are foundational to Newton's treatment of resistance, and to his purported challenge to Descartes's Vortex Hypothesis. Prior to presenting these results, Newton had used his solution to the efflux problem and its extension to calculate a normalized value for the inertial resistance acting on a spherical body moving through an idealized continuous fluid. That is, in Proposition 38, he presented a way of quantifying the "resistance that arises from the inertia of matter of the fluid" in terms of the fluid's density, the sphere's weight and diameter, and their relative velocity. In Proposition 40, he compares this theoretical value with real-world measurements of resistance, which, he says, "can be investigated" by measuring a body's velocity and time of descent through a specified distance of fall. And thus, the announced task of the experiments that he presents is "[t]o find from phenomena the resistance of a sphere moving forward in a compressed, very fluid medium" (*ibid*, 749).

Newton reports that "I got a square wooden vessel, with an internal length and width of 9 inches (of a London foot), and a depth of 9 ½ feet, and I filled it with rainwater; and making balls of wax with lead inside, I noted the times of descent of the balls, the space of the descent being 112 inches" (*ibid*, 750-751). He provides the measurements he collected when conducting twelve separate experiments using the setup he describes, each of which involved balls of varying weight falling through water. The final two experiments that he details, Experiments 13 and 14, are different than the first twelve; in these cases, Newton reports what was observed when he dropped balls from the top of St. Paul's Cathedral in London and measured the rate of their fall through the air.

In many of these fourteen cases, Newton provides experimental data of the times of fall that very nearly matches the times that are predicted by his theory of resistance. Measuring the time of descent by the oscillations of a pendulum, he reports in Experiment 5, for instance, that the times of fall he observed for four balls were 28 ½, 29, 29 ½, and 30, and "By the theory they ought to have fallen in the time of very nearly 29 oscillations" (*ibid*, 754). However, in other cases there is a notable discrepancy between Newton's theoretical prediction and experimental results. For instance, when completing Experiment 3, "Three equal balls, each of which weighed 121 grains in air and 1 grain in water, were dropped successively in water" and allowed to fall 112 inches (*ibid*, 752). Newton notes that, "[a]ccording to the theory, these balls should have fallen in a time of roughly 40 seconds" (*ibid*, 752). But they didn't. They instead fell "in times of 46 seconds, 47 seconds, and 50 seconds" (*ibid*, 752). Newton lists several possible reasons for this discrepancy:

> I am uncertain whether their falling more slowly is to be attributed to the smaller proportion of the resistance that arises from the force of inertia in slow motions to the resistance that arises from other causes, or rather to some little bubbles adhering to the ball, or to the rarefaction of the wax from the heat either of the weather or of the hand dropping the ball, or even to imperceptible errors in weighing the balls in water. (*ibid*, 752)

In light of these possibilities, the conclusion that Newton reaches is not that his theory should be adjusted but that the experimental setup should be refined to account for the interfering factors that he's listed. He claims, in particular, that "the weight of the ball in water ought to be more than 1 grain, so that the experiment may be made certain and trustworthy" (*ibid*, 752).

There are other cases in the Scholium to Section 7 where Newton reports significant discrepancies between theory and data but about which he makes no comment at all. And in some of these cases, the reported discrepancies appear to challenge the reliability of his theoretical account of inertial resistance. Notice, in particular, that the vertical-fall experiments were conducted using the real continuous fluids of water and air. In principle, the balls falling through these fluids will always encounter greater resistance, and move more slowly, than bodies moving through ideal continuous fluids, which have no internal or surface friction. Consequently, since Newton's theory of inertial resistance is a theory of motion in *ideal* continuous fluids, it should always be the case that the time of a ball's fall through water and air is greater than what the theory predicts. And yet, Newton provides experimental results indicating that the times of fall through real continuous fluids is *lower* than a body's fall through an ideal continuous fluid. In his report of Experiment 11, for instance, Newton states that three balls of equal weight "fell in the times of 43 ½, 44, 44 ½, 45, and 46 oscillations," and he simply ends the description of this experiment by remarking that "[b]y the theory they ought to have fallen in the time of roughly 46 5/9 oscillations" (*ibid*, 755). The lack of an explanation for the discrepancy here is noteworthy, because this is a case where the observed times of fall contradict the theoretical model. It should never be the case that the observed fall of bodies through real continuous fluids is swifter than the theoretical prediction for fall through ideal continuous fluids. Yet this is exactly what Newton reports – and he reports it without any attempt to account for the discrepancy by locating possible problems with his experimental setup, and thus, without any reason why this data should not count as an exception to his theory, and thus, should not count as adequate grounds for modifying or even rejecting it.

So, while Newton concludes the presentation of his experiments with the qualification that "*almost all* the resistance encountered by balls moving in air as well as in water is correctly shown by our theory" (*ibid*, 759; emphasis added), there are particular cases where the failure to establish a fit between theory and experiment raises questions about Newton's methodology. He presents experimental evidence that seems to undermine his theoretical approach to inertial resistance, and with no explanation for why the data in these cases might be unreliable, his theory is left vulnerable to being challenged by his own experimental results. Smith puts it this way:

> While most of the vertical-fall data lie close to his theory, some of the experimentally determined resistances were non-trivially larger than the theory implied, and some were *less*. Newton noted the cases where the experimental resistances were significantly larger, calling attention to the high velocities in these cases and arguing that when the velocity becomes high enough, there is a loss of fluid pressure on the rear face of the moving body, contrary to the assumption made in deriving his theory. He says nothing, however, about the cases in which the experimental resistances fell non-trivially below his theoretical value…In putting forward his theory, Newton remarked that it is supposed to give the least resistance that can occur insofar as the viscous and surface-friction effects will augment the resistance from fluid inertia. Unless the experimental resistances falling below his theory are attributed to experimental error, therefore, they cannot help but raise questions about whether the theory is correct. (Smith 2005, 144-145; emphasis in the original)

The experimental results also cannot help but raise questions about the Cartesian character of Newton's methodology. It is the fundamental, additive assumption with which we began that entails that real resistance will always be greater than ideal, merely inertial resistance. Therefore, it is this assumption that is directly challenged by the experimental results that Newton presents. That Newton does not reconsider its merits, even after producing legitimate empirical grounds for doing so, appears to leave the fundamental assumption on the same footing as a vortex model of the heavens, and leaves Newton appearing as guilty as Descartes of postulating a general explanatory device that is (somewhat) consonant with the phenomena but is by no means tested by them.[31]

4. Working Hypotheses and Newton's Logic of Theory-Mediation

Smith applies the same strategy for defusing the charge of Cartesianism made about the first edition of Book 2 to the revisions found in the later editions of the *Principia*. In brief, he urges a reading according to which the questionable proposals that Newton makes in Book 2, Section 7 are not proposals that are meant to be directly verified by experimental evidence, and also are not proposals that are forwarded as final pronouncements that gain support from the evidence available. According to Smith, these proposals should be understood as serving a different function. Broadly speaking, they convince us of the acceptability of the theory insofar as they demonstrate the fruitfulness of the theory. Furthermore, they reveal how the theory can be extended and they serve as the basis for further evaluation of the theory itself.

To better understand the nuance of Smith's suggestion, we can return to Newton's choice of the precise ½ value in the case of the stationary disk of Corollaries 7 through 10 of Problem 36. On Smith's account, Newton did not choose this specific value simply because his theoretical assumption about the action on the rear of a body dictated it. Relatedly, he did not choose ½ in order to block a Cartesian counter-argument to his claims about the detectable effects of inertial resistance on the motion of spheres through continuous fluids.[32] Rather, there are other, interrelated considerations at play here. Newton is showing that there is a value for the weight of the column of water above the stationary disk that: [1] is consistent with his theoretical considerations (it is a value that falls between the upper and lower bounds he's calculated); [2] is consistent with the general theoretical assumption that the action on the rear of the body is negligible; but moreover, [3] allows him to extend his theory in such a way that he can isolate and measure the inertial resistance acting on the front face of the body. And in addition, the value he proposes [4] gives him the means of investigating why there might be discrepancies between what the theory predicts and what obtains in the real

[31] Or, as Smith puts the point: "Newton's vertical-fall data for water and air provide no real evidence for his theory of resistance in inviscid fluids. To whatever extent Newton's presentation gives an impression that these data do constitute evidence for the theory, that presentation is misleading. The only element of good science here appears to be the vertical-fall data themselves" (Smith 2005, 145).

[32] Smith explicitly considers these options in Smith 2000, pp. 122-123.

world. These factors, taken together, make Newton's chosen value of ½ a "working hypothesis," a proposal radically different in kind from a Cartesian hypothesis. As Smith puts it:

> Newton's choice of ½ is best regarded as a *working hypothesis*, a first approximation on which further research can be predicated. The choice of ½ need not then have been mere wishful thinking. Evidence would have accrued it to the extent that the further research predicated on it would have succeeded in yielding stable results on the magnitude and variation of non-inertial mechanisms of resistance. Failure of such research to yield stable results would have given grounds for modifying or abandoning the ½ number. This strategy might well have seemed the best hope not only for investigating the non-inertial mechanisms, but also for providing a basis for refining the ½ number. (Smith 2000, 124)

The ½ choice is thus not "deduced from the phenomena" insofar as there was insufficient empirical evidence to infer that the weight of the column of water is half the weight of the cylinder. Newton's hand is in no way forced into endorsing the ½ value. But the choice is justified nevertheless for two types of reasons. First, it is consistent in various ways with Newton's other theoretical commitments. Second, it allows Newton to obtain a precise value for the theoretically expected inertial resistance of a fluid, and thus provides a baseline against which to compare experimental results. In other words, specifying a precise value for the column of water allows for the construction of further 'down-stream' evidence. The theoretical backing of Newton's theory of inertial resistance does not stand as the sole reason for accepting it as a possibility worth testing. It is *a* reason, of course. But on Smith's reading, working hypotheses are possibilities worth accepting because they have both theoretical backing and also because of the fruits they bear for further, more sustained research.

The choice of ½ also show that the vertical-fall experiments cannot be fully understood apart from this promise of further research. The experiments are not offered to confirm a theoretical model in a straightforward hypothetico-deductive way. That is, the results Newton reports aren't meant to convince us that the theory should be accepted because what the theory predicts actually obtains in real-world situations. Instead, the results are presented in the context of the theory – or better, in conversation with the theory – such that they create resources for further research. More precisely, Newton is showing us that when the theory is applied to the real-world situations he describes, the theory can give rise to the sort of evidence that allows for further extension of the theory. It does so by making deviations from the baseline the subject of further research. On this point, Smith emphasizes Newton's claim (made after concluding the presentation of the theory and turning to the vertical-fall data) that "This is the resistance that arises from the inertia of matter of the fluid. And that which arises from the elasticity, tenacity, and friction of its parts can be investigated as follows" (Newton 199, 749). On Smith's reading, this remark "does not imply that the data are to be taken as evidence for the theory" (Smith 2005, 145). It implies that the data have provided Newton adequate support to begin the project of extending his theory of resistance.

Two intertwined features of this account are particularly important to us. First is the constitutive role of working hypotheses in individuating new phenomena and, by so doing, rendering them theoretically meaningful. In fact, we locate the ability of working hypotheses to initiate novel research in this power to individuate. This is a point worth putting in its full generality: Departures from baseline theoretical values cannot be discriminated in theoretically-unmediated experience; or, equally, they cannot be understood as distinct phenomena outside the context of a relevant theory. For example, nothing in the unmediated experience of falling objects directly corresponds to the effects of the inertial component of resistance. There are no "joints" in nature that tell us how to decompose observable motion into theoretically significant phenomena. Decomposition is only available when we make some enabling theoretical assumptions. These allow us to construct counterfactual, idealized phenomena (like the motion of a body in a friction-free fluid or the motion of the moon absent the Sun's pull), compare real-world observations to them, and thus decompose real-world observations into evidentially significant components.

In the case discussed above, it is the fundamental additive assumption and a few lower-level parameters (like the ½ value) that allow us to calculate a theoretical value for inertial resistance. This value entails an expected time for free-fall in an ideal fluid, which serves as the primary phenomenon. Taking this phenomenon to be our baseline, we can identify cases that depart from it by comparing observed values with the theoretical value. Any such departures are secondary phenomena, which can be investigated in their own right. They may lead to tertiary phenomena, forth-order phenomena, and so on. But there are no distinct phenomena to investigate without a working hypothesis to constitute and individuate them. One way of articulating the importance of this iterated approach to theory-mediated measurement is to say that because each application of a working hypothesis also tests all previous applications, a successful application provides very strong evidence that the hypothesis in question is true (perhaps approximately). In other words, theory-mediation can give rise to exceptionally compelling evidence. However, the foregoing discussion also allow us to articulate a somewhat different methodological point: because each iterated application of a working hypothesis tests the manner in which phenomena were constituted both by itself and all the applications that came before it, a successful application provides extremely strong evidence that we've decomposed phenomena in the (approximately) right way. It shows, in other words, that we've carved nature at more-or-less the right joints. We will return to this issue shortly.

The second feature is the central role in Smith's account of what we'll call 'conditional evidence.' Conditional evidence is evidence for some hypothesis that is based on phenomena that would not be constituted as phenomena without assuming the hypothesis which the evidence is meant to support. But the idea of conditional evidence raises the prospect of a complement that Smith only briefly considers. We'll call it *independent* (or simply 'non-conditional') evidence. This sort of evidence is independent in the following sense: it serves as evidence for some hypothesis, but on the basis of phenomena that can be constituted and individuated as phenomena independently of the hypothesis in question. Smith notes that evidential reasoning before Newton was based almost entirely on evidence of this kind. He writes that "the quantities central to the mathematical theories of motion under uniform gravity laid out by Galileo and Huygens were all open to measurement without having to presuppose any propositions of the theories themselves (Smith 2016, 193). Evidence of this sort plays no role in the portions of Book 2 we've been considering. However, things look different in the case of the laws of motion, a set of claims that Smith also identifies as working hypotheses (Smith 2001b, 335). Considering these will allow us to discuss independent evidence in more detail and by so doing extend Smith's account of Newton's logic of theory mediation.

5. Mathematical Representation in Newton's Logic of Theory-Mediation

Support for the laws of motion exemplifies the two notions of evidence we have sketched above. The case for conditional evidence is familiar to students of Smith's work. The laws allow deviations from straight-line motion to be identified *as phenomena*, and thus to become evidentially meaningful. The deviations provide evidence concerning forces and their sources, and this evidence is used in conjunction with the laws to generate further, higher order deviations and higher order evidence. When the evidence yields a coherent account of forces and sources, the laws gain empirical support and the process can continue. Yet the laws are merely "working" hypotheses in the sense that the world can be such that this process ultimately fails: the process can yield incoherent information, or we may be unable to find appropriate forces and sources to explain some phenomena. When this happens, the laws themselves need to be reconsidered. For our purposes, what's important in this picture is that the laws themselves allow us to constitute the phenomena that serve as evidence for the them.

As a matter of historical development, the laws gained empirical support of this sort through a process that extended far beyond the *Principia* (Smith 2014). However, Newton also drew attention to the conditional nature of the evidence for the laws (specifically the first and second laws) as the initial, prima facie reason for accepting them. In the scholium that follows the laws, he writes that "By means of the first two laws and the first two corollaries Galileo found that the descent of heavy bodies is in the squared ratio of the time" (*ibid*, 424). That is, he suggests that by *assuming* the laws, Galileo was able to generate results that were recognized in Newton's time to be true. Newton suggests the same for the laws of collision of the famous Royal Society competition of 1666-8: "*From the same laws* and corollaries and law 3, Sir Christopher Wren, Dr. John Wallis, and Mr. Christiaan Huygens… found the rules of the collision and reflections of hard bodies" (*ibid*, 424; emphasis added). According to Newton, the three mathematicians did not reason *to* the laws of motion. Rather, they started with the laws of motion and derived from them generally accepted results. In other words, they generated conditional evidence in the laws' favor. Newton presents this evidence as the presumptive reason we should accept the laws.[37]

But Newton also suggests there was independent (non-conditional) evidence for the third law. He writes that by means of a 10-foot pendulum he found "within an error of less than three inches in the measurements—that when the bodies met each other directly, the changes of motions made in the bodies in opposite directions were equal, and consequently that the actions and reaction were always equal" (*ibid*, 426). He provides a calculational method for conducting these experiments with bodies of varying elasticity (*ibid*, 425) and reports studying the collisions of bodies of wool, cork and steel (*ibid*, 427). What's notable here is that Newton doesn't assume the law in order to describe the phenomena under study, and then use that phenomena to generate evidence in favor of the law. That is, the phenomena he uses as evidence are not made possible by the third law itself. Of course,

[37] Newton offers a reconstructed history to suit his needs, not one we should take as fact. First, he was in no position to comment on Galileo processes of discovery. He knew little of Galileo (Cohen 1992). Second, Newton would have known that the laws of Wren, Wallis, and Huygens were based on a variety of principles. Those principles can be recovered within the framework of Newtonian laws, but they are not identical to them. Why Newton didn't make this explicit is a complicated question (see Biener and Schliesser 2016). For our purposes, it is only important to notice that he portrayed the evidence generated by Galileo, Wren, Wallis, and Huygens as conditional. For more on the Royal Society competition, see Jalobeanu 2011. For further discussion of mathematical certainty in the reasoning that Newton attributes to Galileo, Wren, Wallis, and Huygens, see Domski 2018.

there are other assumptions needed to make pendular collisions serve as evidence for the third law (assumption about elasticity, the center of mass of the pendulum bobs, etc.). But the third law is not required. The situation is the same in Newton's defense of the third law for attractions. Newton argues that in a body whose parts do not attract one another equally and oppositely (i.e., where one part exerts an unbalanced force on the other), motion would be generated, and thus the system would move "indefinitely with a motion that is always accelerated" (*ibid*, 427-428). This, he claims, "is absurd." The absurdity is derived from unstated notions about perpetual or infinitely accelerated motion, but it does not derive from a provisional acceptance of the third law itself.[38] We do not need the third law to constitute the (hypothetical) phenomena that Newton judges is "absurd". Newton further adds that indefinitely accelerated motion is "contrary to the first law." This is also not conditional on the third law. Both arguments provide *non-conditional* evidence for the law, even if the latter argument provides evidence that is conditional on the first law. Newton provides other examples from the study of simple machines, much to the same effect.

As things stand, it might seem that the non-conditional evidence for the third law might us – at least in this highly delimited context – to a Huygensian/Galilean evidential predicament. Just to reiterate, in this predicate empirical evidence is collected without assuming any of the theoretical propositions under study and thus does not provide a check on whether we've constituted the relevant phenomena in more-or-less the right way. But we believe this is not the case. In order to see this, however, we must look beyond Newton's laws to more fundamental mediating theoretical assumptions. To this end, Newton's defense of the third law – specifically, his comparison of the rebound motions in two-pendulum collisions – is instructive. The core of Newton's claim is that, in pendular collisions, "when the bodies [being tested] met each other directly, the changes of motions made in the bodies in opposite directions were equal, and consequently that the actions and reaction were always equal" (*ibid*, 426). Newton establishes this by showing that "the quantity of motion—determined by adding the motions in the same direction and subtracting the motions in the opposite directions—was never changed." The process requires a host of assumptions. Some assumptions are quite specific: e.g., that the distance between a bob's center of oscillation and its center of mass is negligible. Other assumptions are more general: e.g., that air resistance retards motion but small corrections can be introduced. We'd like to draw attention to further assumptions that are more general still: for example, that velocities, directions, and some measure of 'bulk' are the relevant

[38] Newton's thought experiment is actually more complicated, but the gist is the same. See Newton 1999, 427-428.

mathematical properties for the study of collision, that they can be assigned coherently to bodies, and that quantities of motion calculated by means of them can be combined vectorially according to the parallelogram rule (the latter in order to combine and compare motions along one direction with motions along another).

Of course, these assumptions were broadly accepted by Newton's time. Yet this does not make them trivial. Identifying the relevant mathematical properties for the study of motion was a hard-fought achievement of pre-Newtonian mechanics. Similarly, measuring magnitude and direction raises a host of conceptual issues that natural philosophers grappled with. For instance, scholars have long recognized that Newton, as part of his argument against the Cartesian definition of motion, explicitly confronted the problem of identifying locations in space and durations in time in a mathematically consistent way.[40] Debates also surrounded the parallelogram rule. First and foremost was the issue of which motive quantities the rule governs. Using the rule requires that the relevant quantities are represented as proportionately sized line segments in a diagrammatic space. Whether the choice of representation makes physical sense – i.e., whether quantities and motions combine in real space as they do in this space – is an empirical matter. But investigating the matter requires some choice of mathematical representation. Newton's choice, as David Miller points out (2016, 188), is to treat "magnitude and direction [as] jointly constitutive of a 'motion'", a conception which is implicit in the Laws (of which more shortly) and explicitly represented in Newton's enunciation of the parallelogram rule (in the first corollary to the Laws). This choice dictates what "motion" means in the *Principia*. Our point is that such choices about mathematical representation should also be understood as working hypotheses. They enable further inquiry into motion by providing fundamental inferential posits and by constituting and individuating the phenomena under study.

We can see this more clearly by considering a working hypothesis of this kind in a more troublesome context. The later Descartes's stated position is that the magnitude of a body's motion (proportional to its speed) and the direction of its motion are essentially separate: a change in the magnitude of motion ought to have no effect on its direction, and conversely.[41] Descartes treats the measure of each as an independent quantity, most famously in his flawed rules of collision.[42]

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[40] E.g., DiSalle 2002. In general, although mathematical natural philosophy was not new in Newton's time, philosophers were still concerned with spelling out why it is appropriate to represent bodies and motions mathematically. See, e.g., Reid 2012.

[41] Its more complicated but still. Actually determination, etc.

[42] We say 'resembling' because his 'determination'

However, in another context, Descartes treats motion differently. In the *Optics*, Descartes derives the sine law of refraction by analogizing the behavior of light to the motion of a tennis ball. He imagines a moving ball changing its speed as it reaches a barrier and constructs a diagram in order to determine the direction of motion on the other side of the barrier (this is the 'refracted' side). The motion of the tennis ball is represented as a line inclined to the barrier. Descartes's procedure for determining the direction of the resulting motion relies on decomposing the original line by means of the parallelogram rule into two orthogonal components, one parallel and one perpendicular to the barrier. Descartes claims that the motion along the parallel component will be unaffected by the barrier, and so halving the speed will only change the vertical component. Because the motion is changed *only* along the vertical component, the 'refracted' ball must have a different inclination to the barrier, and this inclination is the resulting angle of refraction.

The details of this construction are interesting, but inessential for us. This is because by Descartes's own lights, a change in speed should have no effect on the direction of motion. The construction is clearly in conflict with his stated position. Why? It seems that his choice of mathematical representation leads him down a garden path. Instead of treating the magnitude and direction as two independent quantities, he represents them jointly as the direction and length of a line segment. This representation allows him to apply a mathematical operation – the vectorial decomposition of the line by the parallelogram rule – that, in effect, ties together the magnitude of motion with its direction. In this way, Descartes's stated position and the diagram lead to different analyses of motion. If we follow Descartes's stated position, the magnitude of a body's motion and its direction should be distinct phenomena, requiring distinct explanations and able to be invoked separately in explanations. Much of the time, this is how Descartes approaches them: he has two conservation laws (one to explain change in magnitude and one to explain changes in direction) and he uses speed and direction separately to explain features of his laws of collision.[43] We can think of the independence of magnitude and direction as a "working hypothesis": it provides fundamental inferential posits regarding motion and tell us how to think about and individuate the phenomena under study. When Descartes uses the parallelogram rule to decompose a line segment that represent a directed motion, he puts in place an implicit, but conflicting working hypothesis – one that treats motion vectorially. This way of treating motion provides a different set of fundamental inferential posits, and so constitutes and individuates phenomena differently. For example, two independent conservation laws are no longer needed to explain motion, because motion is one thing: a magnitude-with-direction.

[43] (Letter Clreselier 1645), provide the quote.

Newton Laws treat motion as essentially vectorial, and so he can represent motion as he does in the first corollary to the Laws (and the remainder of the *Principia*) without a problem. But that his choice of representation fits with his stated commitments does not change its logical status. He could have chosen a wrong representational vehicle (as Descartes did), which would have, in effect, put in place a different description of motion (and all that follows from it). This is why we believe choices of mathematical representation should be understood as working hypotheses in themselves.

And, finally, we can point to the theoretical posits that do mediate what we earlier presented as the non-conditional evidence for the third law. The evidence from pendulum motion discussed above relies on the idea that motions can be combined vectorially, it is the idea that motion is to treated vectorially that

, in this predicate empirical evidence is collected without assuming any of the theoretical propositions under study and thus does not provide a check on whether we've constituted the relevant phenomena in more-or-less the right way

They underly even the comparison of motions that provides the non-conditional evidence for third law by constituting the phenomena of motion with both magnitude and direction jointly. In the end, that evidence turns out to be conditional, but not on the laws of motion themselves. The commitment can still be wrong, that's what makes them working hypotheses. (This may rely on going through why Descartes representation should have told him something about motion). These working hypotheses

Need to end with: they allow phenomena to provide a check on specific issue, not just on the theory as a whole.

We can clarify our point using Smith's Quinean image of Newton's logic of theory-meditation. As we read Smith, he suggests that in the web of belief that is to be found in the *Principia*, the laws of motion are at the center. Everything that follows is incumbent on them:

> Newton's laws of motion made generic claims about the relation between unbalanced static forces and motions. In reaching so far beyond the available evidence, I submit, they acquired the status of working hypotheses. The role they play in the *Principia* is one of enabling

phenomena of planetary motion (and, in Book 2, phenomena of motion in resisting media) to become evidence pertaining to physical forces. Substantial further evidence accrued to them from Newton's success in deriving the law of gravity from phenomena of planetary motion, and still more compelling evidence accrued to them indirectly from the subsequent successes of celestial mechanics. Taking the laws of motion to have the status of working hypotheses [can suggest] that Newton's theory can be viewed as "a single extraordinarily complex hypothesis." But this surely is not the best way of viewing the theory, for it radically obscures the logic of the evidential reasoning… The laws of motion have a very different logical status … than the law of gravity. (Smith 2001b, 335-336)

We take this "very different logical status" to mean that the laws are presupposed by everything that follows and that, consequently, they belong in the center of a Quinean web. It can also mean (although Smith does not say so) that, because of their central location, all evidence in favor of the laws is conditional.

What we're suggesting is that this can't be quite right. Newton thought that there were some phenomena that served as direct evidence for (at the very least) the third law. And given that these phenomena require (at the very least) the mediation of mathematical representation to enable them to play the role of evidence, the laws themselves can't be the most central element in the Quinean web. Instead, we suggest that the most central element of Newton's web of belief is the working hypothesis that only phenomena that can be assigned determinable magnitudes – that is, only phenomena that are subject to basic mathematical representation – can serve as evidence about motions and forces. On our reading, this is the most fundamental enabling posit of Newton's logic of theory-meditation.

But we must also qualify this idea. It is possible that the mathematical framework may not follow the logic of working hypotheses *entirely*. Although all evidence conditional on the mathematical framework—i.e., all evidence—is evidence for the framework itself, it is not clear under what conditions this mathematical framework could be revised. It seems, in fact, that Newton thought it was unrevisable. In the pre-*Principia* tract *De Gravitatione*, he puts the point in the language of metaphysics and claims that phenomena necessarily take place in a mathematical arena whose structure directly follows from God's existence by emanation: "And hence there are everywhere all kinds of figures, everywhere spheres, cubes, triangles, straight lines, everywhere circular, elliptical, parabolical, and all other kinds of figures, and those of all shapes and sizes, even though they are not disclosed to sight" (Newton 2004, 22). For Newton, the phenomena are always mediated by the language of mathematics precisely because they occur within a structure already rife with mathematical meaning. In other words, for Newton, there is no gap "between pure mathematics, on the one side, and the sensible and material world, on the other" (Friedman 2012, 345). The sensible, material world just is a world characterized by a mathematical framework. In turn, on the reading we've forwarded, the sensible, material world can be investigated using the tools of natural philosophy because it is a world of phenomena that are fit for the mathematical representation that is required for them to be individuated, and for them to serve as evidence.

6. Conclusion

We began this essay by contrasting the "Newtonian Style" with the Cartesian, hypothetico-deductive method. Following Smith, we stressed (in Section 2) that the Newtonian style relies on theory-mediation to turn otherwise inchoate phenomena into well-behaved *evidence*, that is, to render phenomena theoretically meaningful. We also emphasized (in Section 3) that theory-mediation—often through the introduction of working hypotheses—is required to turn inchoate experience into well-defined *phenomena*. Following Smith again, we used the fundamental assumption of Newton's theory of fluid resistance as our primary example. But we've also extended his framework (in Section 4) by introducing the complementary notions of conditional and direct evidence, and we did so to distinguish between evidence generated in favor of a working hypothesis by means of the working hypothesis itself and evidence generated through other means. In light of Newton's suggestion that he had (at least some) direct evidence for the laws of motion, (in Section 5) we located at the core of his "web of belief" the proposal that evidence must be represented in mathematical terms – that there can be no evidence independent of the spatio-temporal framework for magnitudes and motions that Newton adopts in the *Principia*. CONCLUDING SENTENCE FROM NOTEBOOK,

Our reasoning leaves us with a portrait of the *Principia* that is consonant with other neo-Kantian readings of Newton (e.g., DiSalle 2013, Friedman 2012, Janiak 2008). What we hope to have demonstrated, however, is that one can arrive at this portrait not only through conceptual clarification of the proposals Newton makes in texts such as *De Gravitatione* and the scholium on space and time (e.g., DiSalle 2002). One can arrive at it by taking Smith's robust account of Newton's logic of theory-mediation to its logical conclusion. Clearly, the conclusions that we present are not ones that Smith forwards or explicitly endorses. But it should be just as clear that our understanding of the role that mathematical representations play in Newton's evidential reasoning, as well as our appreciation for their centrality in Newton's logic of theory-mediation, would have been impossible without his work.

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