Newton’s Regulae Philosophandi

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The Oxford Handbook of Newton
Edited by Eric Schliesser and Chris Smeenk

Subject: Philosophy, Philosophy of Science, History of Western Philosophy (Post-Classical)
Online Publication Date: Jan 2018 DOI: 10.1093/oxfordhb/9780199930418.013.4

Abstract and Keywords

Newton’s *Regulae philosophandi*—the rules for reasoning in natural philosophy—are maxims of causal reasoning and induction. This essay reviews their significance for Newton’s method of inquiry, as well as their application to particular propositions within the *Principia*. Two main claims emerge. First, the rules are not only interrelated, they defend various facets of the same core idea: that nature is simple and orderly by divine decree, and that, consequently, human beings can be justified in inferring universal causes from limited phenomena, if only fallibly. Second, the rules make substantive ontological assumptions on which Newton’s argument in the *Principia* relies.

Keywords: Newton, method, universal causes, causal reasoning, induction

Newton’s *Regulae philosophandi*—the rules for reasoning in natural philosophy—are maxims of causal reasoning and induction. They appeared in some form in all three editions of the *Principia* (1687, 1713, 1726), where they opened Book III of the work—the book in which Newton “come[s] down to Physics” from the mathematical peaks of Books I and II and considers natural causes and their scope (*Principia*, 588).[1] Their location in Book III mirrors the location of the “Axioms, or Laws of Motions” of Book I, and for good reason. If for Newton the “main Business of natural Philosophy” was to “argue from the Phaenomena without feigning Hypotheses, and to deduce Causes from Effects,” the rules provided the principles for doing so (*Opticks*, 369). In the *Principia*, they were used to deduce the work’s controversial new cause: universal gravitation. As universal gravitation became increasingly accepted, the rules themselves struck root and became philosophical orthodoxy. Leonard Euler used them to justify his concept of force, William Whewell sought to subsume them under his comprehensive inductivism, and Charles Darwin structured *On the Origin of Species* to meet their demands (Harman 1983, Butts 1993, Hodge 1977). For some, their authority was even a mark of self-evidence. Thomas Reid declared the rules to be “maxims of common sense … practised every day in common life.” For him they were so central to the enterprise of knowledge that “he who philosophizes by other rules … mistakes his aim” (Reid [1764] 1997, 12).

But the commonsensical nature of the rules belies a complex history. They were called “*Hypotheses*” in E1 and only relabeled “*Regulae Philosophandi*” in E2 and E3.[2] In E2, Newton added a new rule, R3, which was kin to but substantially different from a hypothesis he chose to remove, H3. In E3, he added yet another rule, R4, and expanded the explanatory text of R3 to address debates concerning the status of gravity as an essential property of matter. He also rephrased the remaining rules in a distinctly cautious, “epistemic” tone. Newton even entertained a fifth rule (concerning the epistemic value of introspection), but never published it. These are not the only changes. A variety of finer ones—some discussed below—can be traced through Newton’s manuscripts and his annotated and interleaved copies of the *Principia* over a period of nearly forty years. Their scope, the years separating them, and the clear implication that Newton did not consider the rules to have reached a complete form led I. B. Cohen to note that “not only is it anachronous to lump together [the rules, as well as other texts], as if they all represented Newton’s state of mind at one and the same time: to do so is also to deny the dynamic quality of a creative mind by assuming that there were no changes on fundamental questions during a span of some forty years” (Cohen 1971, ix).

This chapter will provide an overview of the rules and their history. Although I consider various issues, two claims will emerge. First, the rules are not only interrelated, they defend various facets of the same core idea: that nature is simple and orderly by divine decree, and that, consequently, human beings can be justified in inferring universal causes from limited phenomena, if only fallibly. Second, the rules make substantive ontological assumptions on which Newton’s argument in the *Principia* relies. Although the chapter is more expository than argumentative, these claims go to challenge the idea that the rules are superfluous and merely formalize inferences that Newton was capable of making on entirely empirical grounds, as well as the idea that the aim of the rules is to express a sophisticated methodology Newton was the first to formulate. I call this a “conservative” interpretation of the rules. I begin by discussing the overall purpose of the rules and then examine them in descending order.

Analysis, Synthesis, and the Purpose of the Rules

Newton’s extensive revisions to the rules were almost entirely driven by debates with Cartesians and Leibnizians. Their crux was already well expressed in the *Principia*’s first review, by the Cartesian Pierre-Sylvain Régis:

> The work of M. Newton is a mechanics, the most perfect that one could imagine…. [But] one cannot regard [his] demonstrations otherwise than as only mechanical; indeed, the author recognizes … that he has not considered their Principles as a Physicist, but as a mere Geometer…. He confesses the same thing *at the beginning of the third book*, where he endeavors nevertheless to explain the System of

> the World. But it is only by *hypotheses* that are, most of them, arbitrary, and that, consequently, can serve as foundation only to a treatise of pure mechanics.
>
> (*Journal des Savans*, August 2, 1688, 153ff; in Koyré 1965, 155)

Régis appealed to a common distinction between *physica* (or *philosophia naturalis*) and *mechanica*. The first was thought to provide causal knowledge of natural change, the second knowledge that was perhaps instrumentally useful, but did not capture natural change through its real causes.[3] Régis's charge was that despite the *Principia*'s commendable treatment of mathematics, it did not capture real causes. He singled out the beginning of Book III—the location of Newton's *Hypotheses* and later *Regulae*—as the point at which Newton employed principles that were arbitrary, and so precluded the possibility of a genuine *physica*.[4]

This was a clear affront to Newton's conception of natural philosophy. To him, the "main business of natural philosophy" was to engage in what late scholastics called the *regressus*—a two-stage process of reasoning from effects to causes (the *analytic* or *reductive* stage) and then from causes back to effects (the *synthetic* or *compositive* stage).[5] The process was aimed squarely at causal knowledge. In Newton's words:

> [I]n natural philosophy … the method of analysis, ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction…. By this way of analysis we may proceed … from effects to their causes, and from particular causes to more general ones…. [T]he synthesis consists in assuming the causes discovered … and by them explaining the phenomena proceeding from them, and proving the explanations.
>
> (*Opticks*, 404–405)

The rules played an essential role in this two-stage process, at least in the *Principia*.

Their role was similar to that ascribed by some late scholastics to a third stage of investigation, the *negotiatio intellectus* (or *contemplatio mentalis*). The *negotiatio* separated analysis from synthesis and was thought to consist of mental operations that, first, winnowed down the set of causes discovered by analysis to only true causes and, second, allowed the natural philosopher to grasp how these causes could be applied universally despite being inferred from limited phenomena. The *negotiatio* was essential for the *regressus*, as it determined which causes were taken as fundamental in the synthetic stage (Randall 1940, 192–201).

To my knowledge, Newton never used the term *negotiatio*. However, the idea is useful for us, since it provides a rather accurate description of the function of the rules.[6] To begin with, the rules's placement at the beginning of Book III indicates they are not precepts regarding the practice of natural philosophy *in general*. If this was their purpose, we would expect to find them at the start of the entire *Principia*. Instead, their placement indicates that Newton was aware that principles of thought were required to negotiate the transition from an abstract treatment of forces to causal reasoning about the actual world.[7] The rules are principles for, first, winnowing down the multiplicity of possible causes of celestial phenomena to a single true cause—gravity—and, second, allowing gravity to be taken universally, despite being inferred from a limited range of phenomena. Like the *negotiatio*, the rules are guidelines for thinking, not for direct empirical investigation. And like the *negotiatio*, they provide the necessary link between the *Principia*’s analysis (i.e., the determination of the various instances of force involved in basic celestial phenomena) and synthesis (i.e., taking those instances as manifestations of the same cause—gravity—and using gravity to explain additional phenomena). Newton’s repeated changes to the rules were meant to clarify how they can execute *this* function, and thus to answer the charge that the *Principia* had not—perhaps could not—provide genuine causal knowledge.[8] Let’s see how the rules do this.

Rule 4: Provisionalism and Induction

Rule 4 *In experimental philosophy, propositions gathered (*collectae*) from phenomena by induction should be considered either exactly or very nearly (*aut accurate aut quamproxime*) true notwithstanding any contrary hypotheses, until yet other phenomena make such propositions either more exact (*accuratiores*) or liable to exceptions.* This rule should be followed so that arguments based on induction may not be nullified by hypotheses. (*Principia*, 796).

R4 was added to the *Principia* in E3 (1726).[9] Although it was largely ignored in the subsequent century (more on this below), it is nowadays the most discussed of Newton’s rules. To understand it, we begin with Newton’s long-standing opposition to “contrary hypotheses.” It was first expressed in his public debates about the nature of light with Robert Hooke, Gaston Pardies, and Christiaan Huygens.

In 1672, Newton claimed to have established *with certainty*, on the basis of experiments, that white light was composed of rays of different “refrangibility,” with each degree of refrangibility corresponding to a different color. The claim elicited fierce objections. First, Newton’s opponents held that since “refrangibility” was not a mechanical quality, its use in physical explanations—and thus Newton’s entire account—was suspect. Second, they held that although Newton’s account was consistent with the phenomena, it was only one among many possible “hypotheses.” To establish this account *with certainty*, Newton would have to eliminate all alternatives, and this was impossible. In general, the threat of what we now call “underdetermination” dominated natural philosophy in the latter half of the seventeenth century. Newton’s opponents, like many of their contemporaries, believed that humans were in an essentially restricted epistemic position: they could offer competing hypotheses to save the phenomena, but could never “see through” the phenomena to determine which hypotheses were true.[10] At best, they could reject hypotheses that were empirically inconsistent or did not abide by some privileged non-empirical considerations (e.g., an exclusive appeal to mechanical qualities). But no more discriminating procedure for theory selection was possible.[11]

Newton agreed that an eliminative method for theory selection was inadequate (precisely because "numerous hypotheses may always be devised"), but suggested that its failure ought to focus attention on the context of discovery and the process of theory construction (*Correspondence*, I, 164). He wrote:

> I cannot think it effectuall for determining truth to examin the severall ways by wch Phaenomena may be explained, unless where there can be a perfect enumeration of all those ways.… [T]he proper Method for inquiring after the properties of things is to deduce them from Experiments.… [T]he Theory wch I propounded was evinced to me, *not by inferring tis thus because not otherwise*, that is not by deducing it onely from a confutation of contrary suppositions, but *by deriving it from Experiments concluding positively & directly*. The way therefore to examin it is by considering whether the experiments wch I propound do prove those parts of the Theory to wch they are applyed, or by prosecuting other experiments wch the Theory may suggest for its examination.
>
> (*Correspondence*, I, 209, original emphasis)

For Newton, a proper theory is discovered by "deduction" from phenomena; that is, phenomena are used "positively & directly" to entail the theory that explains them, not only negatively to disqualify theories that are inconsistent with them. The logical, evidentiary tie that such a deduction establishes between phenomena and theory renders the theory immune to alternatives that are not themselves grounded in the phenomena or, worse, the mere possibility of alternatives. Such alternatives, as intuitively compelling as they may be, do not directly pertain to the established tie between phenomena and a deduced theory, and so cannot sever it. R4's purpose is to rebuff "contrary hypotheses" of this sort. Already in the 1670s, Newton was clear in his opposition to them.[12]

Yet R4 does not merely reiterate Newton's 1670s methodological stance. In the early debates, Newton had a zealot's confidence that natural philosophical claims can be known with mathematical certainty. By the 1710s, when R4 was articulated, his views had become considerably more nuanced and cautious.[13] They resulted from his development, in the process of composing the *Principia*, of what I.B. Cohen called "the Newtonian style" (1982).[14] Briefly, on the Newtonian style, real-world motions are approached by a series of increasingly accurate approximations. These are underwritten by "quamproxime" propositions—propositions relating motions to forces in which the conclusions are approximately true *iff* the premises are approximately true (G. E. Smith 2002b). An *increasingly* accurate characterization of motions and forces is achieved by specifying the conditions under which an initial approximation holds exactly and then taking systematic deviations from the exact conditions as physically significant; that is, taking them to have yet-unspecified physical sources and beginning a search for such sources. When these are found and their effects characterized by a new approximative model, the conditions under which it holds *exactly* become fodder for the next search for sources of deviation, and so on, iteratively. There is no guarantee, however, that the iterative process will always succeed. If at any point the process is hopelessly frustrated, the assumptions on which it is based—concerning the sources and nature of forces, their strengths, etc.—may be rejected. Even the most fundamental assumptions of Newton's system—for example, the Laws of Motion on which the entire investigative framework rests—may be rejected.[15] In that sense, all claims in Newton's system are provisional; they can be accepted as long as the iterative process continues successfully, but may be rejected in light of future research.

R4 captures the main elements of this style. First, the style's provisionalism is reflected in R4's concern with the attitude of investigators toward the propositions they entertain (propositions *should be considered* true, and so *taken* as holding exactly), not the objective truth of those propositions. Second, the style's reliance on approximative techniques explains R4's instruction that some propositions should be taken only as *very nearly* ("quamproxime") true, not true *simpliciter*. Finally, the style's provisionalism and reliance on approximations sanction the program for continuing research suggested by R4: in the course of future investigation, propositions ought to be made "either more exact or liable to exceptions."

But there are also reasons to be cautious about reading the Newtonian style into R4. Newton's provisionalism, as well as his condemnation of contrary hypotheses, are compatible with reading the rule as a defense of a more simple induction from instances.[16] On this deflationary reading, Newton's focus on the attitude of investigators indicates his recognition of the inherent risk involved in even the simplest inductive generalization. Moreover, his demand that some propositions not be taken as strictly true and that all propositions be subject to future correction merely indicates his recognition that inductions can be made stronger/weaker and more/less general by new findings. The primary evidence for this view is R4's use in the body of *Principia*, the circumstances of its initial formulation, and the various contexts in which Newton made statements similar to it. I rehearse this evidence at some length, since it highlights the interrelatedness of R4 and R3, one of the themes of this essay.

R4 is used only once in the *Principia*, in defense of proposition E3.III.5. That proposition (in all editions) argues that gravity is a mutual interaction, and that thus all planets and moons "gravitate toward one another"; for example, the sun gravitates toward the planets as the planets gravitate toward the sun, and planets gravitate toward their moons as the moons gravitate toward their planets. It relies on two assumptions. First, the proposition requires that the third law of motion (the action/reaction law) applies to the centripetal forces holding satellites in their orbits. The third law turns these into *mutual* interactions. Second, the proposition requires identifying the bodies toward which these centripetal forces point as their sources. This assumption turns the forces into interactions between satellites and the *central bodies* around which they revolve.

In 1713, Roger Cotes—the editor of E2—pressed Newton on these assumptions. The crux of his objection was that, while Newton had good grounds for applying the third law to contact action, he had no evidence how, or even that, the law applies to non-contact, gravitational action (Biener and Smeenk 2012, 119–124). To make the point vivid, Cotes asked Newton to entertain a hypothetical "invisible hand"—a non-gravitational, mechanical cause able to push a satellite toward its central body. If this hand were the cause of the satellite's acceleration, the third law would entail that it, not the central body, was subject to a reaction force. How did Newton know that the third law applied to the central body, not the hand? Cotes surely galled Newton by declaring that "'till this objection be cleared I would not undertake to answer one who should assert that You do *Hypothesim fingere*" (*Correspondence*, V. 392).

Newton responded by dismissing Cotes's hypothetical cause and defending the third law. Some of his comments concerned the close connection between the first law (the law of inertia) and the third. Others concerned the methodological validity of generalizing the third law from contact to non-contact action; that is, the validity of inducing the law from known to unknown instances. It is here that Newton first clearly articulated the line of thought that resulted in the addition of R4 to E3 some thirteen years later. In a draft response, he brushed off Cotes's objection:

> Experimental philosophy argues only from phenomena, draws general conclusions from the consent of phenomena, and looks upon the conclusion as general when the consent is general without exception, though the generality cannot be demonstrated a priori.… [I]n experimental philosophy it's proper to distinguish propositions into principles, propositions, and hypotheses, calling those propositions which are deduced from phenomena by proper arguments and made general by Induction (the best way of arguing in philosophy for a general proposition) and those hypotheses which are not deduced from phenomena by proper arguments.
>
> (*Correspondence*, V. 398–399)

For Newton, the invisible hand—since not directly implied by the phenomena—was a mere hypothesis, to be dismissed for the reasons discussed at the beginning of this section. The third law, in contrast, was gathered from phenomena by proper arguments (offered in the scholium to the Laws of Motion) and made general by induction. It was known to be true in well-understood, experimented-upon instances (contact forces), presumed by induction to be generally true, and so extended to less-understood instances (celestial centripetal forces). Newton knew this extension came with no guarantees—the law's generality "cannot be demonstrated a priori"—but he also believed Cotes did not provide good reason to doubt it. Newton's defense of the third law, in short, appealed to the validity of induction. His position, later captured by R4, was that the results of induction are presumptively true and trump the mere possibility of contrary instances (which are logically feasible, but not empirically supported). The Newtonian style was not at issue here. Proposition III.5 does raise questions about mutual interactions that can be answered by means of the sophisticated style, but neither Newton nor Cotes saw these as the foci of discussion.

Newton also made statements similar to R4 in other contexts. Take, for example, Query 31 of the 1717 *Opticks*:

> [Hypotheses] are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phenomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiment, it may then begin to be pronounced with such Exception as occur.
>
> (*Opticks*, 404)

The context of the passage is revealing. Query 31 begins by asking “Have not … Bodies … certain Powers … by which they act at a distance[?]” It continues by listing a variety of physical, chemical, and biological phenomena that exemplify powers, and concludes that “thus Nature will be very conformable to her self … Performing *all* the great Motions of the heavenly Bodies … [and] *almost all* the small ones [by similar powers]” (*Opticks*, 375, 397, emphasis added). In other words, the query is a long inductive argument, and the passage just quoted comments on it. Immediately after the passage, Newton also supplies a précis of his overall argument in the *Opticks*. But the Newtonian style is foreign to the *Opticks*, and, appropriately, the passage focuses on the generality and strength of induction from instances.

The deflationary view of R4 is perhaps best supported by one of the rule’s less apparent features, its close connection to R3, a rule that is certainly not concerned with the sophisticated approximative style of the *Principia*. In Newton’s final letter regarding III.5, he instructed Cotes to add the famous *Hypotheses non fingo* passage to the General Scholium.[17] It contains another echo of R4, but also an appeal to those qualities that are the subject of R3:

> In this experimental philosophy, propositions are deduced from the phenomena and are made general by induction. The impenetrability, mobility, and impetus of bodies, and the laws of motion and the law of gravity have been found by this method.
>
> (*Principia*, 943)

Newton’s draft letter contained a variety of similar statements. In fact, he explicitly notes that the inductive generalizations of III.5 “holds good by the *third* Rule of philosophizing” (*Correspondence*, V 398, emphasis added). He repeatedly appealed to (forms of) R3 to explicate R4, and even noted that the R3’s rejection was tantamount to “destroy[ing] all arguments taken from Phenomena by Induction,” a phrase that mirrors R4’s “This rule should be followed so that arguments based on induction may not be nullified” (*Correspondence*, V 398, *Principia*, 796).[18]

The connection between R3 and R4 was apparent to Newton’s followers. For example, Henry Pemberton, the editor of the edition in which R4 first appeared, wrote that on R3 “is founded that method of arguing by induction, without which no progress could be made in natural philosophy” (Pemberton 1728, 25). He added that “[Newton] farther inforces [the method of Induction] by this additional precept [R4], that whatever is collected from this induction, ought to be received, notwithstanding any conjectural hypothesis to the contrary, till such times as it shall be contradicted or limited by farther observations on nature” (Pemberton 1728, 25–26). To Pemberton, R4 was merely an extension of R3, not an additional precept regarding *quamproxime* reasoning or the inferential style of the *Principia*. Pemberton was a mediocre mathematician, however, and it is no surprise that he didn’t arrive at a more sophisticated interpretation. Still, one cannot blame him. Even an attentive reader could have easily juxtaposed R4’s “considered … exactly or very nearly true” with the *Opticks*’s “may be looked upon as so much the stronger, by how much the Induction is more general,” and considered R4 to be a further reflection on induction from instances. Perhaps this is why R4 was mostly neglected in the century after Newton’s death. Without a good deal of mathematically sophisticated Newtonian scholarship, the rule does indeed seem to contain, as Whewell averred, “little more than a general assertion of the authority of induction, accompanied by Newton’s usual protest against hypotheses” (Whewell 1840, 452).[19]

We have seen, however, that even in Newton’s mind, R4 was tightly bound with the nature of enumerative induction, particularly with the latter’s fallibility and the consequent provisionalism of natural philosophy. While this fallibility was the focus of R4, in R3 Newton took a more assertive, confident tone.[20] Let’s turn to R3, Newton’s most explicit statement about taking causes and propositions regarding them universally.

Rule 3: Induction and Universality

Rule 3 *Those qualities of bodies that cannot be intended and remitted and that belong to all bodies on which experiments can be made should be taken as qualities of all bodies universally.* [These include extension, hardness, impenetrability, mobility, inertia, and gravity.]

R3 is the most historically and conceptually complex of Newton’s rules. Like R4, it was used only once in the *Principia*. In proposition III.6.c2 of E2 and E3, it supported the claim that “all bodies universally” gravitate (literally: are heavy) in proportion to their quantities of matter (*Principia*, 809). Based on pendulum (and later free-fall) experiments, Newton determined that at equal distances from the earth, the weights of bodies made of any material (wood, gold, wheat, etc.) was directly proportional to their quantities of matter.[21] Since this proportion could not be “intended or remitted”—it could not be changed by material operations—and since it was “a quality of all bodies on which experiments can be performed,” R3 entailed that it could be generalized and “affirmed of all bodies universally” (*Principia*, 809). III.6 was the first proposition in which Newton asserted that weight is possessed by *every* body, regardless of size, spatial position or kind. The inference was intended to ward off the possibility of bodies that do not gravitate at all or gravitate differently, either the small aetherial particles many believed were responsible for gravitation, or bodies at planetary distances that were not directly accounted for by Newton's evidence. It was also intended to ward off the idea that gravity, which had been compared to magnetism by the likes of Robert Hooke and Gilles Personne de Roberval, did not act like magnetism: it affected *all* bodies, not a subset of them.

Curiously, gravity's universality was justified differently in E1. That edition contained a "Hypothesis 3" (henceforth, H3) which read:

> Every body can be transformed into a body of any other kind and successively take on all the intermediate degrees of qualities.
>
> (*Principia*, 795)[22]

This "transmutation hypothesis" was replaced by the familiar R3 in E2 and E3. The change is less substantial than it first seems. Although H3 was abandoned, the argument it supported remained in E2 and E3, albeit in an ancillary role to the main inductive argument. Newton argued that if there were kinds of bodies that gravitated in different proportions to their quantities of matter, H3 would entail they could transform into one another and "take on all the intermediate degrees" of gravity. If this were the case, however, we would expect to find variation in the gravitational response of bodies of different kinds (wood, gold, wheat, etc.). Since we do not, there must not be kinds of bodies that gravitate in different proportions to their quantities of matter—"all bodies universally" must gravitate in the same way (*Principia*, 809).[23]

Several issues surround R3 and H3. I can address only a few.[24] To begin with, we might ask what Newton meant by "all bodies *universally*" or "*universal* gravity." The question is important, as Newton also asserted that gravity was not *essential* to bodies. Some have claimed that Newton thought gravity was a necessary property of bodies, or perhaps a primary property. Others have claimed that Newton really did think gravity was essential, but wished to avoid controversy.[25] A common implication is that Newton chose "universal" as a placeholder for a more loaded term, or perhaps was just imprecise about the variety of ways in which properties can be associated with their bearers.

But this is wrong. Questions concerning gravity's metaphysical status are apropos, of course—and they were immediately raised by Newton's contemporaries—but Newton's use of "universality" mirrored common technical uses and was quite natural. It was also deflationary, in the sense that it did not directly entail any position regarding gravity's essentiality, necessity, or primacy. In the late seventeenth century, it was not unusual to think of *universal* qualities (or propositions, principles, causes, etc.) as those that applied to every member of a given class, *without* necessarily implying more loaded ontic/epistemic terms. "Universal" (and cognates) was used synonymously with "general" and "common," as Newton used it in R3:

> *The qualities of bodies that cannot be intended and remitted [etc.]… should be taken as qualities of all bodies universally* [qualitatibus corporum universorum].

> For the qualities of bodies … that square with experiments universally [*generaliter*] are to be regarded as universal [*generales*] qualities….
>
> (*Principia*, 795)

Knowledge of such general qualities (propositions, principles, causes, etc.) was frequently taken to be the result of induction from particulars.[26] For example, Samuel Smith wrote in a book that Newton heavily annotated that induction proceeds "A singularibus ad universale."[27] Robert Sanderson, in another book Newton knew well, held that induction is the process "by which we make up a universal conclusion summoning many experiences" (quoted in Mamiani 2001, 12). Importantly, induction from particulars, then as now, was an inference to some *relevant class*, not necessarily to all objects in the universe. Newton, too, used "universal" to capture members of restricted classes. For example: "near the surface of the earth … the accelerative gravity … is the same in all bodies universally [*in corporibus universis*]," and "there is gravity toward all planets universally [*in planeta universa*]" (*Principia*, 408). The first class concerns only objects near the surface of the earth, the second only primary and secondary planets (fewer than twenty objects). English translations of *universus* (and cognates) are particularly misleading, as they tend to render the single adjective as a determiner and an adverb: "all … universally." This might suggest that "universally" adds a layer of meaning to "all," but it doesn't. The adverb is sometimes dropped altogether; for example, "Therefore the sun gravitates toward all the primary and secondary planets [*planetas universos*], and all these [*universi*] toward the sun" (*Principia*, 390). In general, universality connoted application to every member of a class, but no more. It did not entail, at least not generally, a position about whether the universality of *x* rested on *x*'s essentiality, necessity, etc. Thus, when Newton asserted that gravity is universal, he was just asserting that it is attributable to every body within the relevant class of bodies. His incredulity at accusations that he had done otherwise is understandable.

Moreover, given the well-discussed nature of induction, Newton would not have expected —nor did he receive—objections regarding the validity of inductive inference itself. Rather, the objections he expected—and received—concerned how to delimit the relevant class of bodies in III.6, that is, they concerned the induction's scope. We can see now why H3 provided the initial justification for gravity's universality, and why the argument from transmutation remained in III.6.c2 after H3 was removed. The transmutation hypothesis asserted that, despite the apparent multiplicity of kinds of bodies in common experience and despite the common belief among vortex theorists that the insensibly small constituents of matter lacked gravity, there were no insuperable differences between types of bodies. As Newton put it in a draft conclusion to E1: "the matter of all things is one and the same, which is transmuted into countless forms by the operations of nature" (Newton 1962, 341). H3 provided an explicit reason for taking the relevant inductive class in III.6 to be the class of all bodies.[28]

Before addressing the homogeneity of matter further, we might ask why Newton abandoned H3 if he retained the argument it supported. Opinions vary. I. B. Cohen suggests that H3 was too vulnerable to criticism by supporters of alternate matter theories (Cohen 1999, 203). Ernan Mcmullin suggests that Newton came to realize that H3 conflicted with atomism, since it allowed even mechanical qualities like impenetrability to be transmuted (McMullin 1978, 143.22). J. E. McGuire defends the compatibility of H3 with atomism—a position I assumed in this section—but suspects that Newton didn't want to defend his atomism publicly. McGuire also argues that, in the early 1690s, Newton adopted Locke's primary/secondary distinction and realized that the distinction was incompatible with a literal reading of H3 (McGuire 1967). Alan Shapiro suggests that H3 could not properly certify the qualities to which Newton appealed in optical explanations (Shapiro 1993, 40ff). For the most part, these positions are compatible, at least insofar as they emphasize Newton's move from the explicit ontological commitments of H3 to the more methodological approach of R3.

I suspect, however, that there is also another reason for Newton's formulation of R3. Once again, the rule's context of composition is informative. Still called an Hypothesis, R3 was first mentioned in an errata sheet to E1, dated March 13, 1689/90 by Fatio de Duiller, a young mathematician who was Newton's confidant at the time. Fatio was also a close associate of Christiaan Huygens, a Dutch mathematician of international standing. In the previous summer, Newton had met with Huygens to discuss, among other things, the Dutchman's soon-to-be-published *Discours de la cause de la pesanteur* (*Discourse on the Cause of Gravity*), which contained an "addition" challenging the *Principia*. In February of 1689/90, Fatio indicated to Newton that he was soon to receive a copy of the work, and suggested that "it being writ in French you may perhaps choose rather to read it here [London] with me" (*Correspondence*, V 390). Newton left Cambridge on March 10, and likely spent the month in London with Fatio, reading Huygens's work. The errata sheet dated March 13 was later sent to Huygens and ended up in Leibniz's Hanover manuscripts (Cohen 1971, §VII.10).

Huygens's *Discours* began with:

> In order to find an intelligible cause of gravity we must see how gravity can come about while presupposing in nature only bodies that are made *from a like matter*, and considering in these neither a quality nor a tendency (*aucune qualit ni aucune inclination*) to draw near one another, but only their different magnitudes, figures, and motions.… We judge rightly from the start that there is nothing in the appearance attributed to the figure, or *in the smallness of its corpuscles*, with an effect that resembles that of gravity, which, being an endeavor or a tendency to motion, must in all likelihood be produced by a motion. So, there remains only to find in what manner it can act, and *in which bodies it can be encountered*….
>
> (Huygens 1690a, 451, emphasis added).

Huygens, a Cartesian, did not find any problem with Newton's position on the uniformity of matter. He believed, however, that "modern authors" triumphed when they rejected "some internal and inherent quality that makes [bodies] tend downward … or a tendency for the parts to join together" (Huygens 1690a, 455). Clear thinking showed that matter was purely mechanical and so only mechanical qualities could be used in genuine physical—that is, causal—explanations. Huygens toed the Cartesian line in asserting that weight was not such a quality; it was not possessed by all bodies and so could not be appealed to as a genuine cause. But, importantly, Huygens also opposed Descartes. He included in his list of mechanical attributes "perfect hardness" that renders fundamental bodies "[1] impenetrable and [2] incapable of being … broken" (Huygens 1690a, 473). This last claim was made in a paragraph about Newton's rejection of vortices—no doubt Newton took note.

Although this is no smoking gun, R3 seems like a direct rebuttal to Huygens. To begin with, the rule does not offer an explicit defense of the uniformity of matter (as H3 did), since this was a point of agreement. Instead, it focuses on which features of bodies can be "declared general" and thus be used in physical explanations. Even in the initial version Fatio transcribed, the rule already stressed experience and induction: "the laws and properties on which it is possible to institute experiments are the laws and properties of all bodies whatsoever (*universorum*)" (Huygens 1950, X 155; McGuire 1968b, 241). In all versions, Newton argued that the rule explained why mechanical qualities were valued to begin with, and so revealed the true, non-speculative grounds of Huygens's position. That Huygens was a target is most clearly implied by Newton's caveats. First, he made a point of arguing that [2] was an overreach: the indivisibility endorsed by Huygens did *not* satisfy the criterion, since there were no phenomena corresponding to it. He even used the occasion to lecture on the value of the *experimentum crucis* for disproving indivisibility, perhaps a methodological grievance carried over from his optical debates with Huygens.[29] Second, he hoisted Huygens by his own petard by noting that "the argument from phenomena will be even stronger for universal gravity than for [1] the impenetrability of bodies," precisely that feature of bodies that Huygens proudly championed against Descartes!

Whether or not R3 was aimed at Huygens, comparing it to the *Discours* reveals why gravity's universality was important to establish. By showing that weight (more precisely, weight-towards) was attributable to every body and on par with extension and motion, Newton licensed its reintroduction—contra Huygens—into genuine physical explanations. We should take seriously Newton's claim that R3 provided "the *foundation* of all natural philosophy" (*Principia*, 796, emphasis added). The rule furnished the physical vocabulary for fundamental natural philosophical explanations. That is, it provided a roster of those basic features of the world that could ground the synthetic portion of Newton's *regressus*, a roster of features that could be taken as genuine properties and causes. Of course, as Galileo's introduction of time into the study of free fall opened a range of questions and answers that was unavailable to his predecessors, so Newton's reintroduction of weight opened a range of questions and answers that was unavailable to mechanical philosophers. For the many (like Régis) who could not understand the complexity of Newton's mathematics or his empirical arguments, *this* was the *Principia*'s main claim.

To close our discussion of R3, let's return briefly to the homogeneity of matter. It is important to appreciate the extent to which it supported several of the *Principia*'s inferences, not only the inductive generalization of III.6. For example, in proposition III.5—the proposition that elicited R4—homogeneity supported the extension of gravity from planets with moons to planets without moons: "[f]or no one doubts that Venus, Mercury, and the rest are bodies of the same kind as Jupiter and Saturn" (*Principia*, 806).[30] In the Classical Scholia to propositions III.4–9, the homogeneity of matter was a repeated theme: "For all bodies, inspite of appearances, are composed of identical parts and of a single material" (Newton 2001, 229). Most importantly, in *The System of the World*—Newton's preliminary version of Book III—there were no analogs to the *Hypotheses* or *Regulae*, yet the "universal nature of matter" played an explicit justificatory role (Newton 1727, §25).

On what ground did Newton assert the homogeneity of matter? In R3 and its drafts, Newton cited "the analogy of nature"—the idea that "nature is always simple and ever consonant with itself" (*Principia*, 795). Once again, a full discussion is impossible.[31] However, the simplicity of nature turns our attention to R1 and R2, as it was also their explicit justification.

Rules 1 & 2: Simplicity and Uniformity

Rule 1 *No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena.* … For nature is simple and does not indulge in the luxury of superfluous causes.

Rule 2 *Therefore, the causes assigned to natural effects of the same kind must be, so far as possible, the same.*

R1 and R2 are principles of causal simplicity and uniformity that remained virtually identical in all editions of the *Principia*.[32] They are also the most cited. In all editions, R2 is cited in support of III.5, and in E3, both R1 and R2 are cited in support of III.4 and III.5. Proposition III.4 shows that the force holding the moon in its orbit is centripetally directed at the earth and has the same strength as the force associated with free fall, to a high degree of precision. Given this similarity of mathematical attributes, and following R1's demand for parsimony of causes, the proposition concludes that the two forces must actually be one.[33] III.5 is likewise premised on the similarity of mathematical attributes of the forces holding the moons of Jupiter and Saturn in their orbits and the force holding the earth's moon in its orbit: all are centripetal and inverse square. Following R2's demand for parsimony of kinds of causes, the proposition concludes that the forces must be of the same kind. They are all instances of gravity.

Both inferences go to eliminate the possibility of forces that are mathematically similar but belong to different physical kinds. In III.4's case, we might imagine that objects on earth are drawn by a force that affects terrestrial objects, but not the moon; and the moon is drawn by a force that affects moon-matter, but not terrestrial objects. In III.5's case, we might imagine a Jupiter-force and a Saturn-force that affect Jupiter-moon-matter and Saturn-moon-matter, but cannot affect anything else. R1 and R2 advise against taking such possibilities seriously, since nature is "simple and does not indulge in the luxury of superfluous causes." It is more reasonable to suppose a paucity than a diversity of forces. Moreover, it is more reasonable to suppose a paucity of kinds of forces.

Importantly, identifying forces also entails identifying types of matter. If the force affecting bodies on earth is capable of affecting the moon, earth-matter and moon-matter must be relevantly similar.[34] The converse is also true. If the earth and moon are made of the same matter, a single force should be able to affect both (*ceteris paribus*). Similarly, if Jupiter and Saturn are made of the same matter, there isn't an obvious sense in which to maintain a distinction between a Jupiter-force and a Saturn-force. The principle of causal parsimony is thus bound up with the homogeneity of matter.[35] In the same way, it is bound up with the idea that natural operations are uniform in space, time, and across non-fundamental genera. As Newton wrote in the explanatory text to R2:

> Examples [of causes that are likely to be of the same kind] are the cause of respiration in man and beast, or of the falling of stones in Europe and America, or of the light of a kitchen fire and the sun, or the reflection of light on our earth and the planets.
>
> (*Principia*, 795)

But why should we suspect that the cause of the falling is the same in Europe and America? Because we already believe that Europe and America are relevantly alike. And, *sotto voce*, because Europe and America *are* relevantly alike, we are justified in thinking that the cause of falling in them is the same. In other words, uniformity is what allows parsimony to be an effective methodological strategy. Moreover, uniformity is what allows induction to be an effective methodological strategy. It should be equally apparent—if only from Newton's direct appeal to Nature's consonance in R3—that the "Rule of Induction" also relies on spatial and temporal uniformity. Without it, Newton's generalization from bodies "on which experiments can be made" to distant bodies, bodies at any scale, and future bodies would be rash.[36] Of course, uniformity cannot guarantee the success of any given induction; hence R4.

R1 and R2 thus express a commitment to natural simplicity *cum* uniformity *cum* homogeneity which R3 and R4 presume. Their central role did not go unnoticed. None other than David Hume—one rather sensitive to the presuppositions of induction—wrote that R2 was "Newton's chief rule of philosophizing" (Hume [1751] 1998, §3.48).[37] Perhaps he was following Roger Cotes, who in the preface to E2 discreetly disputed Newton's characterization of R3 as the "foundation of all philosophy" by claiming that "All philosophy is based on *this* rule [R2], inasmuch as, if it is taken away, there is then nothing we can affirm about things universally" (*Principia*, 391, emphasis added). At any rate, Newton's deep commitment to natural simplicity, uniformity, and heterogeneity is a central and repeated theme in his work. He asserted it throughout his life, often playing on the same phrase: "Nature is ever simple and conformable to herself" (e.g., unpublished preface to the *Principia* (Newton 1962, 307), draft conclusion to the *Principia* (Newton

1962, 333), "Di vi electrica" (*Correspondence*, V 366), and the *Opticks* passages already quoted.)

Here, finally, our discussion must reach past the rules. Although Newton once suggested that the rules are themselves the products of induction, his commitment to Nature's simplicity is more likely due to his ardent theological convictions.[38] Newton believed that the world was created by a providential God in a way that allowed His servants to understand it, and, in consequence, to understand something of Him. Of course, created beings cannot understand everything about the world. Newton was clear that knowledge of essences, for example, was impossible (*Principia*, 942; Newton 2004a, 27). However, as the General Scholium and letters to Bentley demonstrate, he was deeply committed to arguments from design, and these presupposed that God has made the world so that His design is at least partially apparent (*Principia*, 940–943; Newton 2004b, 94–97).[39] The link between God, His design, and simplicity was made plain by Newton some ten years before composing the *Principia*. In a treatise on revelation, he wrote:

> As the world, which to the naked eye exhibits the greatest variety of objects, appears very simple in its internall constitution when surveyed by a philosophic understanding, and so much the simpler by how much the better it is understood.… It is the perfection of God's works that they are all done with the greatest simplicity. He is the God of order and not confusion. And therefore as they that would understand the frame of the world must indeavour to reduce their knowledg to all possible simplicity, so it must be in seeking to understand these visions.
>
> (Untitled Treatise on Revelation (§1.1), 14r, *Newton Project*, quoted in Snobelen 2005, 234).

The perfection of God is demonstrated in His works, and their perfection is demonstrated in their simplicity and order. All in all, it is God's relation to creation that makes the Rules of Philosophizing reasonable precepts to follow. That relation is the tacit premise behind them and, indeed, behind Newton's physical investigations. Without it, there would be no sense to the Rules and no sense to natural philosophy.

Conclusion

While addressing various smaller issues, I have traced a line of thought through Newton's four rules to the substantive assumptions about God (and thus Nature) that underlay them. Although we can spend much more time on the character of Newton's theological commitments, the point should be clear. Newton believed that Nature is simple and orderly because it is divinely decreed, and that, consequently, human beings can reason about it effectively. In particular, they can (fallibly) determine causes and make inductive generalizations on which they can base natural philosophical knowledge. I have also tried to describe the highly interrelated nature of the rules and the ways in which Newton often used different rules for one and the same purpose, as well as one and the same rationale for different rules. I believe this interrelatedness shows that the rules were less distinct in Newton's mind than they appear.

To close, I'd like to point out that despite Newton's thorough reorientation of the methods and presuppositions of physico-mathematics, his broader methodological outlook vis-á-vis the rules, particularly their relation to theology, was not unusual. The following passage is from Isaac Barrow, Newton's mentor and first Lucasian professor. It anticipates virtually all of the rules and demonstrates their tight interrelations. Moreover, it clearly ties questions of methodology to premises about God. It even makes a failure to abide by these methodological precepts akin to mortal sin! No doubt Newton approved.

> [W]here any Proposition is found agreeable to constant Experience, especially where it seems not to be conversant about the Accidents of Things, but pertains to their principal Properties and intimate Constitution, it will at least be most safe and prudent to yield a ready Assent to it. For as we are justly accused of a rash Temerity, by suffering ourselves to be so much as once deceived by our Faith, so we are guilty of the greatest Imprudence, if we shew the least Distrust, and do not yield our stedfast Assent and obstinately adhere, when we still find our Expectations answered as accurately as possible (*quam accuratissime*), after a thousand Researches; and especially when we have the constant Agreement of Nature to confirm our Assent, and the immutable Wisdom of the first Cause forming all Things according to simple Ideas, and directing them to certain Ends: Which Consideration alone is almost sufficient to make us look upon any Proposition confirmed with frequent Experiments, as universally true (*universaliter vera*), and not suspect that Nature is inconstant and the great Author of the Universe unlike himself. Nay sometimes, from the Constancy of Nature, we may prudently infer an universal Proposition (*colligamus universalem propositionem*) even by one Experiment alone.
>
> (Barrow 1860, 82; Barrow 1734, 73–74, originally published in 1683, translation modified)

References

Achinstein, Peter. 2013. *Evidence and Method: Scientific Strategies of Isaac Newton and James Clerk Maxwell*. Oxford: Oxford University Press.

Arnauld, Antoine, and Pierre Nicole. 1696. *Logic: or, The Art of Thinking*. London: F. Taylor.

Barker, P., and B. R. Goldstein. 2001. "Theological Foundations of Kepler's Astronomy." *Osiris* 16: 88–113.

Barrow, Isaac. 1734. *The Usefulness of Mathematical Learning Explained and Demonstrated: Being Mathematical Lectures Read in the Publick Schools at the Univerity of Cambridge*. Translated by John Kirkby. London: Stephen Austen.

Barrow, Isaac. 1860. *The Mathematical Works of Isaac Barrow*. Edited by William Whewell. Cambridge: Cambridge University press.

Belkind, Ori. (unpublished manuscript). "The Divisibility Criterion in Boyle and Newton."

Bertoloni Meli, Domenico. 1993. *Equivalence and Priority: Newton Versus Leibniz*. Oxford: Clarendon Press.

Biener, Zvi. 2016. "Newton and the Ideal of Exegetical Success." *Studies in History and Philosophy of Science A* 60: 82–87. Doi: 10.1016/j.shpsa.2016.07.001

Biener, Zvi and Eric Schliesser. 2017. "The Dual Newton of Newton's *Axiomata sive Leges Motus*." *Monist* 100(3): 311–325.

Biener, Zvi and Christopher Smeenk. 2012. "Cotes' Queries: Newton's Empiricism and Conceptions of Matter." In *Interpreting Newton: Critical Essays*, edited by Andrew Janiak and Eric Schliesser, 103–137. Cambridge: Cambridge University Press.

Bolton, Martha. 2003. "Univesals, essences, and abstract entities." In *The Cambridge History of Seventeenth-Century Philosophy*. Edited by Daniel Garber and Michael Ayers. 2nd Rev Ed., 178–211. Cambridge: Cambridge University Press.

Butts, Robert E. 1993. "Whewell on Newton's Rules of Philosophizing." In *Historical Pragmatics: Philosophical Essays*. Volume 155, 293–312. Dordrecht: Kluwer Academic Publishers.

Cohen, I. Bernard. 1971. *Introduction to Newton's Principia*. Cambridge, MA: Harvard University Press.

Cohen, I. Bernard. 1982. "The *Principia*, Universal Gravitation, and the *Newtonian Style*." In *Contemporary Newtonian Research*, edited by Zev Bechler, 21–108. Dordrecht, Holland; Boston, MA: D. Reidel.

Cohen, I. Bernard. 1999. "A Guide to Newton's *Principia*." In *The Principia: Mathematical Principles of Natural Philosophy*, 1–370. Berkeley: University of California Press.

De Pierris, Graciela. 2006. "Hume and Locke on Scientific Methodology: The Newtonian Legacy." *Hume Studies* 32(2): 277–329.

Dear, Peter. 1995. *Discipline and Experience: The Mathematical Way in the Scientific Revolution*. Chicago: University of Chicago Press.

Di Fate, Victor Joseph. 2011. "Is Newton a 'radical Empiricist' about Method?" *Studies in History and Philosophy of Science Part A* 42(1): 28–36. doi: **http://dx.doi.org/10.1016/j.shpsa.2010.11.024**.

Dobbs, Betty Jo Teeter. 1991. *The Janus Faces of Genius: The Role of Alchemy in Newton's Thought*. Cambridge: Cambridge University Press.

Ducheyne, Steffen. 2012. *The Main Business of Natural Philosophy: Isaac Newton’s Natural-Philosophical Methodology*. New York: Springer.

Ducheyne, Steffen. 2014. “Newton on Action at a Distance.” *Journal of the History of Philosophy* 52(4): 675–702.

Guicciardini, Niccolò. 2009. *Isaac Newton on Mathematical Certainty and Method*. Cambridge, MA; London, England: The MIT Press.

Harman, P. M. 1983. “Force and Inertia: Euler and Kant’s *Metaphysical Foundation of Natural Science*.” In *Nature Mathematized*, edited by William R. Shea, 229–249. Dordrecht, Holland: D. Reidel Publishing Company.

Harper, William. 2007. “Newton’s Methodology and Mercury’s Perihelion Before and After Einstein.” *Philosophy of Science* 74(5): 932–942.

Harper, William. 2011. *Isaac Newton’s Scientific Method: Turning Data into Evidence about Gravity and Cosmology*. Oxford: Oxford University Press.

Harper, William and George E. Smith. 1995. “Newton’s New Way of Inquiry.” In *The Creation of Ideas in Physics: Studies for a Methodology of Theory Construction*, edited by Jarrett Leplin, 113–166. Dordrecht, The Netherlands: Kluwer Academic Publishers.

Hazony, Yoram and Eric Schliesser. 2015. “Newton and Hume.” In *The Oxford Handbook of Hume*, 673–707. Oxford: Oxford Univerity Press. doi: 10.1093/oxfordhb/9780199742844.013.28.

Henry, John. 1994. “‘Pray Do Not Ascribe That Notion to Me’: God and Newton’s Gravity.” In *The Books of Nature and Scripture: Recent Essays on Natural Philosophy, Theology and Biblical Criticism in The Netherlands of Spinoza’s Time and the British Isles of Newton’s Time*, edited by James E. Force and Richard Henry Popkin, 123–147. Dordrecht: Springer.

Hobbes, Thomas. 1656. *Six Lessons to the Professors … in the University of Oxford*. London: Andrew Cook.

Hodge, M. J. S. 1977. “The Structure and Strategy of Darwin’s ‘Long Argument’.” *British Journal for the History of Science* 10: 237–246.

Hume, David. (1751) 1998. *An Enquiry Concerning the Principles of Morals*. Edited by Tom L. Beauchamp. Oxford: Oxford University Press.

Huygens, Christiaan. 1690a. *Discours de la cause de la Pensanteur (Discourse on the Cause of Gravity)*. Translated by Karen Bailey and George E. Smith. In *Oeuvres complètes*, Vol. XXI. M. Nijhoff, La Haye, 1950.

Huygens, Christiaan. 1690b. *Traité de la Lumière (Treatise on Light)*. In *The Scientifc Background to Modern Philosophy*. Edited and Translated by Michael Matthews. Indianapolis and Cambridge: Hackett, 124–132.

Huygens, Christiaan. 1950. *Oeuvres complètes*. La Haye: M. Nijhoff.

Janiak, Andrew. 2008. *Newton as Philosopher*. New York: Cambridge University Press.

Jardine, Nicholas. 1976. “Galileo’s Road to Truth and the Demostrative Regress.” *Studies in History and Philosophy of Science* 7(4): 227–318.

Keill, John. 1702. *Introduction ad Veram Physicam, seu Lectiones Physicae Habitae in Schola Naturalis Philosophiae Academiae Oxoniensis*. London.

Keill, John. 1720. *An introduction to natural philosophy: or, Philosophical lectures read in the University of Oxford anno Dom. 1700. To which are added, the demonstrations of Monsieur Huygens’s theorems, concerning the centrifugal force and circular motion*. London: Printed by H. W. for W. and J. Innys.

Koyré, Alexandre. 1965. *Newtonian Studies*. Cambridge, MA: Harvard University Press.

Leibniz, Gottfried Wilhelm. 1989. *Philosophical Essays*. Translated by Roger Ariew and Daniel Garber. Indianapolis & Cambridge: Hacket Publishing Company.

Mamiani, Maurizio. 2001. “To Twist the Meaning: Newton’s *Regulae Philosophandi* Revisited.” In *Isaac Newton’s Natural Philosophy*, edited by Jed Z. Buchwald and I. Bernard Cohen, 3–14. Cambridge, Mass.: MIT Press.

Mancuso, Paolo. 1997. *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century*. Oxford: Oxford University Press.

McGuire, J. E. 1967. “Transmutation and Immutability: Newton’s Doctrine of Physical Qualities.” *Ambix* 14. Reprinted in McGuire 1995, Ch. 7, 69–95.

McGuire, J. E. 1968a. “Force, Active Principles, and Newton’s Invisible Realm.” *Ambix* 15. Reprinted in McGuire 1995, Ch. 5, 154–208.

McGuire, J. E. 1968b. “The Origin of Newton’s Doctrine of Essential Qualities.” *Centaurus* 12. Reprinted in McGuire 1995, Ch. 6, 233–260.

McGuire, J. E. 1970. “Atoms and the ‘Analogy of Nature’: Newton’s Third Rule of Philosophizing.” *Studies in History and Philosophy of Science* 1. Reprinted in McGuire 1995, Ch. 2, 3–58.

McGuire, J. E. 1995. *Tradition and Innovation: Newton’s Metaphysics of Nature*. Boston: Kluwer Academic Publishers.

McMullin, Ernan. 1978. *Newton on Matter and Activity*. Notre Dame, IN: University of Notre Dame Press.

Milton, J. R. 1987. “Induction Before Hume.” *British Journal for the Philosophy of Science* 38(1): 49–74.

Newton, Isaac. (1726) 1999. *The Principia: Mathematical Principles of Natural Philosophy*. Translated by I. Bernard Cohen and Anne Miller Whitman. Berkeley: University of California Press.

Newton, Isaac. 1727. *A Treatise of the System of the World*. London: F. Fayram. In *Sir Isaac Newton’s Mathematical Principles of Natural Philosophy and His System of the World*. Translated, edited by Florian Cajori, and Andrew Motte. Berkeley, Los Angeles, London: University of California Press, 1934 [1975].

Newton, Isaac. (1930) 1952. *Opticks, or, A Treatise of the Reflections, Refractions, Inflections & Colours of Light, Based on the Fourth Edition London 1730*. New York: Dover Publications.

Newton, Isaac. 1959–1977. *The Correspondence of Sir Isaac Newton*. Edited by A. R. Hall, H. W. Turnbull, J. F. Scott, and L. Tilling. Volumes 1–7. Cambridge: Cambridge University Press.

Newton, Isaac. 1962. *Unpublished Scientific Papers of Isaac Newton*. Edited by A. R. Hall and M. B. Hall. Cambridge: Cambridge University Press.

Newton, Isaac. 2001. “Newton’s scholia from David Gregory’s Estate on the Propositions IV through IX Book III of his Principia; edited, translated, and annotated by Volkmar Schüller.” In *Between Leibniz, Newton, and Kant: Philosophy and Science in the Eighteenth Century*, edited by Wolfgang Lefèvre. Volume 220. Dordrecht: Kluwer Academic Publishers.

Newton, Isaac. 2004a. *De Gravitatione*. In *Isaac Newton: Philosophical Writing*. Edited by Andrew Janiak. New York, Cambridge, UK: Cambridge University Press, 12–39.

Newton, Isaac. 2004b. *Isaac Newton: Philosophical Writings*. Edited by Andrew Janiak. New York, Cambridge, UK: Cambridge University Press.

Okruhlik, Kathleen. 1989. “The Foundation of All Philosophy: Newton’s Third Rule.” In *An Intimate Relation: Studies in the History and Philosophy of Science*, edited by James Robert Brown and Jürgen Mittelstrass, 97–114. Dordrecht; Boston: Kluwer Academic Publishers.

Pemberton, Henry. 1728. *A View of Sir Isaac Newton’s Philosophy*. London: Printed by S. Palmer.

Poppi, Antonino. 2004. “Zabarella, or Aristotelianism as a Rigorous Science.” In *The Impact of Aristotelianism on Modern Philosophy*, edited by Riccardo Pozzo, 35–63. Washington, D.C.: Catholic University of America Press.

Randall, John Herman Jr. 1940. “The Development of Scientific Method in the School of Padua.” *Journal of the History of Ideas* 1(2): 177–206.

Reid, Thomas. (1764) 1997. *An Inquiry into the Human Mind On the Principles of Common Sense*, edited by Derek R Brookes. The Edinburgh edition of Thomas Reid. Edinburgh: Edinburgh University Press.

Schliesser, Eric. 2009. “Without God: Gravity as a Relational Quality of Matter in Newton.” In *Vanishing Matter and the Laws of Motion from Descartes to Hume*, edited by Dana Jalobeanu and Peter R. Anstey. London: Routledge.

Schliesser, Eric and George E. Smith. (unpublished manuscript). “Huygens’s 1688 Report to the Directors of the Dutch East India Company on the Measurement of Longitude at Sea and the Evidence it Offered Against Universal Gravity.”

Sgarbi, Marco. 2013. *The Aristotelian Tradition and the Rise of British Empiricism: Logic and Epistemology in the British Isles (1570–1689)*. Netherlands: Springer. doi: 10.1007/978-94-007-4951-1.

Shapiro, Alan E. 1989. “Huygens’ *Traité de la lumière* and Newton’s *Opticks*’: Pursuing and Eschewing Hypotheses.” *Notes and Records of the Royal Society* 43: 223–247.

Shapiro, Alan E. 1993. *Fits, Passions, and Paroxysms: Physics, Method, and Chemistry and Newton’s Theories of Colored Bodies and Fits of Easy Reflection*. Cambridge [England]; New York: Cambridge University Press.

Smith, George E. 2002a. “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*, edited by David B. Malament, 31–70. Chicago: Open Court.

Smith, George E. 2002b. “The Methodology of the *Principia*.” In *The Cambridge Companion to Newton*, edited by I. Bernard Cohen and George E. Smith, 138–173. Cambridge: Cambridge University Press.

Smith, George E. 2014. “Closing the Loop: Testing Newtonian Gravity, Then and Now.” In *Newton and Empiricism*, edited by Zvi Biener and Eric Schliesser, 262–351. Oxford: Oxford University Press. doi: 10.1093/acprof:oso/9780199337095.003.0011.

Smith, Samuel. 1656. *Aditus ad logicam in usum eorum qui primò academiam salutant*. Oxford: Rich. Davis.

Snobelen, Stephen D. 2005. “‘The true frame of Nature’: Isaac Newton, Heresy and the Reformation of Natural Philosophy.” In *Heterodoxy in Early Modern Science and Religion*, edited by John Brooke and Ian Maclean, 223–262. Oxford: Oxford University Press.

Spencer, Quayshawn. 2004. "Do Newton's Rules of Reasoning Guarantee Truth … Must They?" *Studies in History and Philosophy of Science Part A* 35(4): 759–782. doi: 10.1016/j.shpsa.2004.02.001.

Spinoza, Benedictus de. (1677) 1985. *Ethics*, in *The Collected Works of Spinoza*. Edited and translated by E. M. Curley. Princeton, N.J.: Princeton University Press.

Stein, Howard. 1967. "Newtonian Space-Time." *Texas Quarterly* 10: 174–200.

Vailati, Ezio. 1997. *Leibniz and Clarke: A Study of Their Correspondence*. New York: Oxford University Press.

Wallace, Walliam A. 1995. "Circularity and the Demonstrative Regressus: From Pietro d'Abano to Galileo Galilei." *Vivarium* 33: 76–97.

Whewell, William. 1840. *The Philosophy of the Inductive Sciences: Founded upon Their History*. London: J.W. Parker.

Zemplén, Gábor Á. and Tamás Demeter. 2010. "Being Charitable to Scientific Controversies: On the Demonstrativity of Newton's Experimentum Crucis." *Monist* 93(4): 640–656.

Notes:

([1]) I refer to the *Principia*'s editions as E1, E2, and E3, and to the rules as R1, R2, etc. I refer to propositions by book, proposition, and corollary; for example, III.6.c2 is corollary 2 of proposition 6 of book III. *Principia* quotations are from Newton (1999). *Opticks* quotations are from Newton (1952). *Correspondence* quotations are from Newton (1959–1977).

([2]) The relabelling occurred between 1694 and 1706, most likely in the 1700s. Although it is important, it does not indicate that Newton initially thought of the rules as *mere* hypotheses. Newton was following a convention of contemporary physico-mathematical works (like Huygens's *Horologium Oscillatorium*) according to which "hypotheses" are starting points of physical explanation. Conversely, the relabeling does not indicate that Newton came to think of the rules as unassailable. Rather, he thought of them as broadly reliable principles that could nevertheless admit of exception or inexactitude—rules of thumb, so to speak. Achinstein (2013) calls them "rules of strategy"; Stein (1967) calls them "guiding principles." Newton even suggested that the rules were the results of induction, and so as fallible as induction (McGuire 1970, 70).

([3]) Even before Newton, this blunt distinction was debated, and this debate was crucial for the rise of mathematical physics (Mancuso 1997, Dear 1995).

([4]) One of the principles Régis considered arbitrary was universal gravitation itself. Leibniz and his followers also held that universal gravitation was not metaphysically well founded. For Leibniz's criticisms, see Koyré (1965, 115–138), Bertoloni Meli (1993), Vailati (1997). Particularly clear is Leibniz's "Against Barbaric Physics" (Leibniz 1989, 312–320). This is not to say that all objections to the *Principia* were of a broad methodological and metaphysical nature. There were many mathematical and evidentiary reasons to seek corrections to Newton's theory and to maintain agnosticism pending further evidence.

([5]) For Newton's familiarity with the Scholastic textbook tradition, see Ducheyne (2012, Ch. 1). For the *regressus*, see Poppi (2004), Wallace (1995).

([6]) Although Newton did not use the term, education in Cambridge included several treatments of Aristotelian logic that appealed to the concept, if not the word (Sgarbi 2013, 35–39). In general, in the latter half of the seventeenth century, the function of the *negotiatio* was often folded into an inductive component of analysis (Jardine 1976[28ff.]; Dear 1995). This is how Newton conceptualized it.

([7]) I thank Mary Domski for this point.

([8]) There are two important corollaries to this way of contextualizing the rules. The first is that they do not capture Newton's method as a whole, but only a single, albeit crucial, element of it. The second follows from the idea that the *negotiatio* "often appealed to information or explanatory principles beyond the original subject matter" (Barker and Goldstein 2001). This characterization belies the claim that the rules merely formalize reasoning that Newton could justify on purely empirical grounds; see Di Fate (2011), Spencer (2004), and Harper (2011).

([9]) Portions of this section are also discussed in Biener (2016).

([10]) A *locus classicus* is Descartes's *Principles of Philosophy*, IV, 204.

([11]) The methodological stances of Hooke, Pardies, and Huygens were actually subtly different. For example, Huygens held that underdetermination was a theoretical but mostly unreal threat. He believed that consistency with phenomena was so hard to achieve that consistent propositions have "a probability nearing truth" (Huygens 1690b, 125). See Shapiro (1989) and Zemplén and Demeter (2010).

([12]) Newton's replacement of *Hypotheses* with *Regulae Philosophandi* was likely meant to suggest that the principles of Book III were not hypothetical in this way. It is also possible, however, that their replacement was a response to the 1701 Latin publication of Descartes's *Regulae ad directionem ingenii* (in *Opuscula posthuma, physica et mathematica*, Amsterdam). Perhaps Newton modified his *Principia* to jab once again at Descartes.

([13]) Even in the 1670s debates, Newton ultimately weakened his position, conceding that the certainty of physical theory was not identical to that of mathematics (Shapiro 1993, 12ff. Guicciardini 2009, 19ff.). In the 1670s debates, Newton also tended to run together inductive and deductive inferences. Later in life, he was more careful to distinguish between them.

([14]) Current understanding of the Newtonian style, too briefly sketched here, is further due to the groundbreaking work of George E. Smith and William Harper. See, in particular, Harper and G. E. Smith (1995), G. E. Smith (2002a), Harper (2007), Harper (2011), and G. E. Smith (2014).

([15]) This is precisely what happened in the transition from Newtonian to Einsteinian gravity. See G. E. Smith (2014) and Harper (2011, Ch. 10). The prospect of revising the laws illustrates clearly that although the laws open the "mathematical" portion of the *Principia*, they are not purely mathematical propositions. They are open to overthrow by empirical finding.

([16]) By induction from instances, I mean a process that "set[s] forth a certain number of cases and observe the resulting [properties], and then compare[s] them with one another in order that the universal proposition can then be known" (Jonn Wallis, *Opera*, I, 365; in Guicciardini (2009, 142)). Wallis's procedure concerns a mathematical context different from the one we are considering, but his terminology is strikingly similar to Newton's.

([17]) At the time of the exchange, Newton could not make modifications directly to III.5, as that portion of E2 had already been printed.

([18]) In drafts to R4, Newton also claimed that without it "the arguments of inductions on which all experimental philosophy is founded could always be overthrown," a claim that mirrors his assertion, in the *Principia*, that R3 is "the foundation of all natural philosophy" (Koyré 1965, 269). In an annotated copy of E1, Newton explicitly justified III.5.c2 "per Hypoth III."

([19]) One wonders how R4 would have been received if John Keill, one of Newton's earliest expositors, had lived past 1721. In lectures delivered in 1700, he stressed that in Newtonian natural philosophy "it is necesary to make use of a more lax sort of Reasoning, and to exhibite Propositions that are not absolutely true, but nearly approaching to the Truth [*ad veritatem quam proxime accendentes*]. As, for example, when it is demonstrated that all the Vibrations of the same pendulum made in the small Arches of a Circle, are of equal Duration" (Keill 1702, 74, Keill 1720, 88). Keill made no mention of "induction" in this context.

([20]) The contrast between R4's provisionalism and R3's confidence is discussed further in Biener and Schliesser (2017).

([21]) As Mary Domski pointed out in conversation, this claim already involves an inductive leap from all bodies on which experiments *have been* made to all bodies on which experiments *can be* made. Even for a single body, the claim that weight is proportional to quantity of matter requires a leap from measurements that have been made to all possible measurements. In general, measurement and induction are intertwined. As Newton wrote, on induction "all experimental philosophy is founded" (Koyré 1965, 269).

([22]) For H3's alchemical and neo-Platonic background, see Dobbs (1991, Ch. 3) and McGuire (1970).

([23]) H3's gradualism is essential. Denying it but holding on to transmutation can entail, for example, that non-gravitating bodies may transform without any intermediate steps into bodies that gravitate in direct proportion to their quantities of matter. In such a case, there could be no change to the observable proportion of gravity to quantity of matter, and so evidence about gravitating bodies would not entail anything interesting about non-gravitating ones. Gradualism eliminates this possibility by demanding detectable variation.

([24]) I will not address the intellectual background to the intention/remission criterion, the connection of R3 to atomism and mechanism, H3's relation to metaphysics of "the great chain of being," the differences between universality and essentiality, the idea of treating gravity as a quality vs. an interaction, Newton's appeal to the *experimentum crucis*, and the logical status of transduction (i.e., induction to in-principle unobservable entities). The most thorough treatments of H3/R3 are still McGuire (1967), McGuire (1968b), McGuire (1968a), and McGuire (1970).

([25]) For different approaches, see Koyré (1965, 149ff), McGuire (1968b), McMullin (1978), Janiak (2008), Schliesser (2009), Henry (1994), and Ducheyne (2014).

([26]) The connection between induction and particulars traces to Aristotle, for example, *Topics* I.12. Similar ideas were repeated in the seventeenth century, without any necessary commitment to induction being the only way to arrive at universals. See Arnauld and Nicole (1696); Spinoza ([1677] 1985, IIp40s1); and Hobbes (1656, Lesson 5). Some disputed that induction can result in universals, for example, Leibniz (1969, 129–30). See Milton (1987) and Bolton (2003).

([27]) S. Smith (1656, 111ff, 144ff, 154ff). Sgarbi (2013, 156) notes: "Sanderson emphasizes particularly the extreme utility of induction for discovering first principles and universals of the causes and of all other universal things to be proved. But he also recognizes the intrinsic weakness of induction, in that a single exception or counterexample can overturn its conclusions."

([28]) Newton also had more technical reasons for asserting the size-invariability of the universal qualities of matter; see Belkind (unpublished manuscript) and Schliesser and G. E. Smith (unpublished manuscript).

([29]) The 1690 *Discours* was printed alongside Huygen's *Trait de la Lumire*.

([30]) In an annotated copy of E1, Newton explicitly backed III.5.c2 "per Hypoth III."

([31]) See, for example, McGuire (1970) and Okruhlik (1989).

([32]) Their most significant revision was the introduction of "so far as possible" to R2 in E3. Ducheyne (2012, Ch. 5) puts the addition in the context of Newton's overall "epistemic" turn in E3.

(33) Matters are more complicated in E1. In that edition, no rules are cited in support of III.4, and only R2 (then H2) is cited in support of III.5. In fact, R1 (then H1) is not cited at all. Although I cannot defend the following conjecture here, I believe that H1’s curious status indicates that Newton did not initially see it as the relevant principle for III.4; or, at least, saw it as primarily applying elsewhere. Specifically, I believe that H1 was intended to support the mutuality of gravitation. It recommends taking the attraction between two bodies not as two forces—one body acting on the other, and the other acting on the one—but as one action that “is simple and single [*simplex est et unica*].” That inference was highlighted in Newton (1727, §21), but was more submerged by E1.

(34) The Classical Scholium to III.4 begins with “That the earth’s moon is a dense body made of earth.” Newton (2001, 219).

(35) III.4 offers an additional “empirical” argument, which some believe stands independently of causal parsimony, and thus renders R1 and R2 superfluous: If the forces in question were not identical, *two* forces would affect bodies, and thus bodies would fall twice as fast; contrary to experience. I believe this fails to appreciate the extent to which the homogeneity of matter is involved in the “empirical” argument, and thus the extent to which a substantive assumption about natural uniformity is nevertheless required; see Di Fate (2011).

(36) In the *Opticks*, Newton speculates that different types of matter and different laws may exist in different parts of the universe. For the compatibility of this claim with R3, see Biener and Schliesser (2017).

(37) De Pierris (2006) has argued that this is a reference to R3, but Hazony and Schliesser (2015) argue for the present interpretation.

(38) In a revision to E2’s rules, Newton wrote, “A Rule I call every Proposition that is (established from) gathered from Phenomena through the argument of Induction and agrees with them” (quoted in McGuire 1970, 70). The thought seems to have been quickly abandoned.

(39) I thank John Henry for stressing this to me.

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