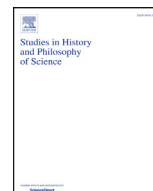




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Essay Review

Newton and the ideal of exegetical success

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William Harper, *Isaac Newton's Scientific Method: Turning data into Evidence about Gravity and Cosmology*. Oxford University Press, Oxford and New York (2011). pp. xviii + 410, Price US\$45.00 paperback, ISBN-13: 978-0198709428

William Harper's excellent, difficult, and provocative book — winner of the 2014 Patrick Suppes Prize for Philosophy of Science — is by far the most detailed available account of Newton's argument for universal gravitation in Book III of the *Principia*. It should be mandatory reading for philosophers interested in the relation of evidence to theory, as well as technically savvy historians of early modern physics. It should also be recommended to novices. Its chapters are mostly self-contained and its step-by-step approach make it a great companion for first time students of Newton's system of the world.

Harper's stated goal is to explicate *Newton's method*; i.e., Newton's use of evidence and inference in the process of theory construction.¹ To this end, Harper examines the *Principia* and the astronomical and experimental data available to Newton with antiquarian glee. But Harper also has a broader goal. By using methods that are sometimes presentist, he aims to show that from a contemporary perspective Newton's reasoning is *proper* reasoning. This is no trivial task. There are no guarantees that contemporary standards of evidence and reasoning can make sufficient sense of historical cases. Harper demonstrates that in Newton's case they do, and thus highlights a historiographical fact often neglected by highly contextualized, local histories of science: that the validity of Newton's argument transcends its context of composition. The book is thus a genuine study in history *and* philosophy of science. It juxtaposes two goals that are often at odds — revealing descriptive and normative truths — and moves frequently between them.²

Since Harper focuses on Newton's use of evidence, it seems fitting to focus on Harper's. His account draws on two main sources.

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¹References to Harper (2011) will be to page number only. Reference to the *Principia* will be to book number and proposition (e.g., III.3 is Book III, Proposition 3), quotations are from Newton (1999).

²Needless to say, there are many ways to understand the relationship between history and philosophy. For essays that almost uniformly belie my facile division, see Laerke, Smith, and Schliesser (2013).

Primarily, it is based on a step-by-step analysis of Newton's inferential practice, as embedded in the propositional structure of the *Principia*. Secondly, it is based on Newton's methodological remarks, as found in the “Rules for the Study of Natural Philosophy” that precede Book III and some scholia and letters.

How do these fit together? Mostly, the book suggests that Newton's practice elucidates his remarks and that his remarks capture his practice. Their fit also establishes authorial intent — that “Newton's method” was actually *Newton's* (p. 128). But intent is also established silently, through an implied question: How could Newton have made the myriad small, highly-technical decisions required to construct the *Principia* — a work that so clearly exemplifies his method — *without* being explicitly aware of it? At times, the question leads Harper to discount the evidential value of methodological remarks. For example, it leads him to ascribe Newton's method to historical actors even when they did not ascribe it to themselves (p. 377). There are also times when the evidential value of methodological remarks is unclear. Harper speaks of certain ideas as “informing” or “backing” both practice and explicit remarks and of practice and remarks as “realizing” or “exemplifying” certain ideas, leaving open whether these are logical/conceptual relations or accounts of actors' own thinking.

That Harper does not pause on these issues is natural — they are not his primary concern. However, his treatment invites us to explore them. It invites us to ask how well Harper's two evidential sources fit together, what is the evidential value of each, and what we can learn from their fit or lack thereof. In one sense, Harper's book offers an extended argument for one set of answers: that Newton's practice aligns with his remarks, that each supports our interpretation of the other, and that their mutual support shows that Newton practiced his method entirely self-consciously.

I'd like to suggest, however, that reconciling Newton's practice with his methodological remarks is more difficult than it seems. I'll demonstrate this with three short vignettes. Each is a variation on the same theme: that the complexity and nuance of “Newton's method” differ sharply from the simplicity of his reflections on it.

To draw the contrast, I must first outline “Newton's method” according to Harper. Readers less interested in the details of Newton's method can skip directly to the vignettes. I must also make one caveat clear: the few disagreements with Harper I raise below are greatly outweighed by unstated agreements.

1. Newton's method

According to Harper, Newton's method is guided by an "ideal of empirical success" [IES] according to which "a theory succeeds by having its theoretical parameters receive convergent accurate measurements from the [diverse] phenomena it purports to explain" (pp. 160, 370). This ideal is "richer" — by which Harper means both more informative (p. 42) and more stringent (p. 140) — than the ideal associated with the Hypothetico-Deductive method [HDM].

According to the HDM, "empirical success is limited to accurate prediction of observable phenomena" (p. 42).³ It entails that a theory becomes better confirmed when its consequences — predictions — match observations within some observational tolerance (p. vi). A mismatch, particularly an ineliminable one, indicates that the theory must be revised. But the mismatch carries no intrinsic information about which parts of the theory to revise or how to revise them. This is because the HDM allows for inferences from theory to predictions, but not from observations back to theory.⁴ Harper argues that Newton's method, in contrast, allows for inferences in *both* directions (p. 43). Its richness stems almost entirely from this more complicated inferential structure.

Let's start with informativeness. Because of its bi-directional structure, Newton's method allows phenomena to *measure* — i.e., provide information about — theoretical parameters. Consider an example (pp. 28, 119ff). In proposition I.45, Newton showed that the apsides of a body in near-circular orbit (the points of nearest and farthest approach to the central body) do not precess *iff* that body moves under the influence of a single centripetal force that is as $1/r^x$ from the force center, where $x = 2$. He also showed that forward precession corresponds to $x > 2$, while backward precession corresponds $x < 2$; both as a function of the precession angle, so that apsides that *approximately* do not precess correspond to an x that is *approximately* 2. This *systematic dependency* allows the precession angle to *measure* the distance exponent of the force law.⁵ It was exploited in proposition III.2. Newton noted there that the lack of noticeable precession in the orbits of Mercury, Venus, Mars, Jupiter, and Saturn shows "with the greatest exactness" that the force holding those planets in their orbits is as $1/r^2$ (Newton, 1999, p. 802). Newton's procedure was *not* to hypothesize a certain value for the parameter (e.g., $x = 2$ in $1/r^x$) and then check whether the observed phenomena bear it out, as the HDM recommends. Rather, it was to set up a sufficiently sophisticated inferential structure so that *even if the phenomena did not bear out the consequences of an inverse-square law*, useful information could be extracted from them. Newton could thus turn *any* data about precession into "far more informative evidence than can be achieved by hypothetico-deductive confirmation alone." Dependencies

of this sort were exploited throughout the *Principia*, in what Harper calls *theory-mediated measurements*.

Importantly, to the extent that a theoretical parameter can be involved in multiple dependencies, it can be measured by diverse phenomena. Agreement between such measurements indicates that the information extracted from them is consistent; that is, that they are truly informative about the parameter they measure. Harper notes that accruing agreement also entails that the extracted information is *resilient*; that is, less open to revision by new measurements. He demonstrates this by means of statistical analyses. We will return to this issue below.

Of course, for a systematic dependency to measure a theoretical parameter, it must be expressed using a theory. More precisely, it must be expressed using a theoretical "background framework" that is both general enough to leave some parameters unspecified (i.e., it must involve weak background assumptions) and powerful enough to entail a sufficient number of systematic dependencies that can be exploited in measurement (p. 22). In the *Principia*, the framework is constituted by the laws of motion and the account of space, time, and force on which they depend. It is drawn out in books I and II, and then used with real-world data in book III to measure the direction and strength of forces and the (relative) masses of solar system bodies.⁶ Theory-mediated measurement may not seem remarkable to contemporary readers — after all, we are used to inferring boson masses from patterns of luminescence in scintillator arrays — but it was relatively new in the seventeenth-century (p. 196). More to the point, it was used by Newton in a remarkably controlled way; namely, to tie together a *single* feature of the available data and a *single* theoretical parameter, so that one can fully determine the other. This enabled Newton to "turn theoretical questions into ones which can be empirically answered by measurement from phenomena" (p. 2).

This brings us to the stringency of Newton's method. Apart from the constraints on theoretical parameter values imposed by the IES, Newton's method involved a commitment to the *provisional acceptance* of claims established by means of theory-mediated measurement. Harper argues that Newton eschewed thinking of empirical support in terms of probabilities (pp. 36, 48). Instead, he took claims established by means of theory-mediated measurements to be provisionally true (or provisionally approximately true), where provisional truth (or provisional approximate truth) is understood as a commitment to using the established claims for the purpose of furthering the IES; i.e., using them in order to generate additional, better theory-mediated measurements (pp. 36, 260ff). Newton also took rejection or revision of previously accepted claims to be mandated *only* when those claims proved no longer useful for furthering the IES or less useful than available alternatives (p. 260).

These criteria entail that even if two theories have identical observational consequences, the one that better promotes the IES is preferable. Newton's method thus allows for theory-choice between empirically equivalent theories, ones between which the HDM cannot discriminate (p. 45). Almost trivially, the method also prohibits "mere contrary hypotheses" — i.e., claims that are logically compatible with the data but do not replicate *any* IES successes — from undercutting claims that are IES-backed. For example, it prohibits the possibility of a Cartesian-style vortex theory from casting doubt on universal gravitation, unless a vortex theory can be produced that bests universal gravitation *according to the IES*. Likewise, they prohibit broad inductive skepticism from undercutting generalizations from measurements established in

³ For the most part, Harper considers a rather spare version of the HDM inspired by Christiaan Huygens. When he considers more sophisticated versions (say, Bayesian formulations), it is to show that they still cannot "recover the features that we have seen to make Newton's method so successful in physics and cosmology" (374).

⁴ In certain cases, a mismatch might carry information even on the HDM. For example, if a theory hypothesizes a linear relationship between two variables, the data might straightforwardly suggest another factor. However, the informational content of the data in such a case is not a feature of the HDM, but a feature of the particular theory and mismatch under consideration. The HDM itself cannot guarantee that such information would be available.

⁵ Smith (2002) details how Newton builds sensitivity to approximations into Books I and II by means of *quam proxime* propositions, propositions whose antecedents are approximately true *iff* their consequents are also approximately true. This idea is folded into Harper's notion of a systematic dependency. For an additional, mutually illuminating account of Newtonian methodology, see also Ducheyne (2012).

⁶ Determining solar systems masses is a thorny issue. Harper's chapters 9 and 10 are essential reading. See also Smith (2013, 224ff).

accordance with the IES. Newton's method is thus more stringent than the HDM in that it can better discriminate between competing theories and better protect a theory from overthrow, once provisionally accepted.

The synergy between these three elements — the IES, theory-mediated measurement, and the criteria for theory acceptance/rejection — is where the true power and lasting importance of Newton's method lie. They “come together to form a *method of successive approximations* that informs applications of universal gravity to motions of solar system bodies” (p. 375, emphasis added). Let's continue with our example. Newton argued in proposition III.3 that the moon is held by an inverse-square force directed at the earth. In the moon's case, however, the precession is large enough that, according to proposition I.45, it corresponds to a force that varies as $1/r^{2.0167}$ not $1/r^2$ (p. 163). Prima facie, this would seem to invalidate the inference to the inverse-square law. Newton, however, had other theory-mediated measurements that suggested a very nearly (closer than $1/r^{2.0167}$) inverse-square force (e.g., the moon-test). According to the criterion of theory acceptance and the IES, these measurements recommend taking the claim that the moon is held by an inverse-square earth-directed force as provisionally *approximately* true.

This means taking action under an inverse-square earth-directed force as a baseline model for the moon's motion. Departures from the *exact, idealized* version of this model then count as “theory-mediated phenomena”: patterns in the data that arise only when coarser, first-approximation patterns are already accounted for. These “phenomena” then serve as new inputs for more refined theory-mediated measurements. In this way, departures from the first approximation do not undercut it, but provide additional sources of information. If that information can be collected in accordance with the IES, a better model can be constructed as a new baseline and the procedure repeated in an iterative, open-ended fashion. Importantly, each successive approximation also “increase[s] the resiliency of the commitment to accept” the entire theoretical framework on which it is based, since each refinement furnishes indirect support for all the previous theory-mediated measurements on which it depends (p. 368).

In the moon's case, Newton showed that the troubling precession can be accounted for by an additional inverse-square *sun-directed* force. The three-body model then served as a new baseline for investigating even finer perturbations, which over the next several hundred years continued to provide indirect support to Newton's initial measurements (pp. 186ff).⁷

The open-endedness of Newton's method is one of the main take-home messages of Harper's account. The method's ability to incorporate new information and continue to pose new research problems ultimately “led to the entrenchment of [Newton's] methodology ... This was a pivotal development in the transformation of natural philosophy ... into natural science as we know it today” (pp. 376–377).

2. Newton's rules

I spent nearly 1500 words on the previous section in order to highlight just how nuanced and complex Newton's method is. We can now compare it to his methodological remarks. The most explicit of these are the “Rules for the Study of Natural Philosophy.” Harper uses them to corroborate his account. I quote them here in their final form, mostly omitting Newton's explanatory comments:

Rule 1 No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena.

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

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. [The qualities include extension, hardness, impenetrability, mobility, inertia, and gravity]

Rule 4: In experimental philosophy, propositions gathered 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 (Newton, 1999, pp. 794–96).

Of course, Newton made additional methodological remarks, some of which I discuss below. But in the following vignettes, I suggest that they did not mean for Newton what they mean for Harper. I'll refer to the rules as R1, R2, R3, R4.

3. First vignette: induction vs. iterative approximation in R4

Harper writes of Newton's method and R4: “Newton [introduced] a paradigm where perturbations, divergences from idealized patterns, were themselves submitted to quantitative analysis. This ... is a paradigm of successively better approximations Newton's key statement of this method, in Rule 4 ..., is explicit in its openness to approximations” (p. 224, emphasis added). We saw in §1 that iterative, successive approximations combine all the elements of Newton's method and are the method's main pay-off. Did Newton intend R4 to recommend them? I believe that when we look at the genesis and broader use of the rule, we find reasons to think otherwise.⁸

R4 is only mentioned once in the *Principia*, in the scholium to proposition III.5 of the third edition. In that proposition, Newton argued that since gravity is a mutual interaction governed by the third law of motion, the sun gravitates towards the planets as the planets gravitate towards the sun, and the planets gravitate towards their moons as the moons gravitate towards their planets. In general, all planets and moons “gravitate toward one another.” Roger Cotes — the editor of the *Principia*'s second edition — objected.⁹ He suggested that the extension of the third law from well-studied, contact action to non-contact, gravitational action was unjustified, a mere hypothesis.

Newton responded by defending the third law. Some of his remarks concerned the tight connection between the first and third laws, but some concerned the validity of inducing the law from known to unknown instances. For example, he instructed Cotes to add the famous *hypotheses non fingo* passage to the General Scholium, which ended with:

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

⁷For a more detailed account of the open-ended nature of Newtonian methodology and its constitutive role in modern geodesy and astronomical research, see Smith (2014).

⁸Most of what Harper says about the rule — e.g., regarding the idea of provisional acceptance, the dismissal of *ad hoc* hypotheses, and the reliance on empirically backed claims — is revealing and entirely unobjectionable. I only wish to dispute the idea that R4 was meant to recommend iterative, successive approximations. Portion of this section are discussed in more detail in Biener (forthcoming).

⁹The full exchange is too rich to repeat here. See Harper's Chapter 9, Biener and Smeenk (2012), and Stein (1990).

motion and the law of gravity have been found by this method. (Newton, 1999, p. 943)

Newton first rehearsed the idea in a draft of the letter to Cotes:

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 ... (Newton, 1959–1977 V, p. 399)

The similarity to R4 is plain, as is the similarity to R3. This was no accident. Newton repeatedly appealed to R3 in his draft, noting that its rejection was tantamount to “destroy[ing] all arguments taken from Phenomena by Induction,” a phrase he later echoed in R4 (Newton, 1959–1977 V, p. 398). In general, the exchange suggests that R4 was an outgrowth of R3. However, R3 is not concerned with iterative, successive approximations. It concerns the validity of inducing from known instances. Newton’s use of the nascent R4 in this exchange concerned the same. It is true, the application of the third law in proposition III.5 does entail that the planets perturb one another and, consequently, that evidence concerning complex orbits gathered through successive approximation bears directly on the validity of the proposition. But this was *not* the subject of discussion. The subject was induction from instances, its validity, and its generality.

Newton made statements similar to R4 in other contexts. Consider 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 ... any Exception shall occur ... it may then begin to be pronounced with such Exception (Newton, 1952, p. 404)

This is an oft-cited passage, but its context is important to remember. The main query of Query 31 is: “Have not the small Particles of Bodies certain Powers ... by which they ... [produce] a great Part of the Phenomena of Nature?” Newton answers by listing a variety of physical, chemical, and biological phenomena that exemplify powers and concluding that, therefore, “all the great Motions ... and almost all the small ones” must be produced by similar powers (Newton, 1952, pp. 375, 397, emphasis added). In other words, Query 31 is a long, inductive argument. It seems natural to read the above passage as a comment on that induction. It’s more difficult to read it as a comment on iterative, successive approximations.

Newton’s acolytes also read R4 as a rule of simple induction. 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,” and continued that “[Newton] farther inforces [this method] 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, p. 26, emphasis added). The connection between R3 and R4, and thus the focus on induction from instances, is clear.

Finally, I should note that the notion of exactness which plays so large a role in R4 was not foreign to treatments of simple induction.

Isaac Barrow — Newton’s mentor and first Lucasian Chair of Mathematics — wrote in a passage that presaged many of the Rules:

[W]here any Proposition is found agreeable to constant Experience ... it will at least be most safe and prudent to yield a ready Assent to it ... [W]hen we still find our Expectations answered as accurately as possible [*quam accuratissime*] ... [we ought to] 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 [...] (Barrow, 1860, p. 82), (Barrow, 1734, pp. 73–74)

One can read “*quam accuratissime*” as demanding perfect accuracy, but one can equally read it as demanding the best accuracy that can be achieved given “frequent experiments.” If so, R4’s explicit openness to approximations would not have prohibited Newton or his colleagues from understanding it as a rule of induction from instances. R4 is certainly *compatible* with the additional complexity that licenses *iterative, successive* approximation, but these quotes suggest it was not intended to refer to it.

4. Second vignette: simplicity vs. resiliency in R1 & R2

Traditionally, R1 and its consequence, R2, have been interpreted as principles of simplicity and unification. Harper’s interpretation is more complex. In R4’s case, Harper’s additional complexity seemed like an over-attribution, but one that was compatible with Newton’s underlying reasoning. In this case, I believe the additional complexity distorts central features of Newton’s thinking.

Newton appeals to R1 and R2 in the “moon test,” his identification of the force keeping the moon in its orbit with terrestrial gravity. The crux of the argument is this: Measurements of the moon’s acceleration towards the earth, adjusted for its distance, agree with measurements of free-fall acceleration near the surface of the earth, as measured by the length of a seconds pendulum. The agreement, according to R1 and R2, suggests that the accelerations have a common cause. Newton identifies it as “gravity.” Harper argues that the inference to a common cause is not driven by general commitments to simplicity and unification, but rather by the IES’s demand for *convergent, agreeing* measurement (p. 35).

In §1, I explained that convergent, agreeing measurements indicate that the information extracted from them is consistent; i.e., that they are truly informative about the parameters they measure. That, of course, was a fudge. ‘Consistency of information’ is no more transparent a notion than ‘agreement of measurements.’ Moreover, neither reveals why they are valued. Harper’s account of the moon test demonstrates their true advantage: they increase the *resiliency* — resistance to large changes — of estimated parameter values (pp. 184ff, 215ff, 245ff). Using modern statistical methods and Newton’s data, Harper shows that estimating the strength of terrestrial gravity by combining data from pendulum measurements with data concerning the moon’s fall — i.e., taking the two data sets to measure the same parameter — makes the estimated value less prone to change when additional measurements are introduced. This is the real advantage of agreeing measurements — they have the potential to yield stable parameter values.¹⁰ It is also why

¹⁰ Harper’s treatment of parameter resiliency is one of the book’s most original features. In the case of the moon test, Harper uses a least-squares analysis and Gauss’s formula for a weighted mean of estimates of differing accuracies. Elsewhere, he quantifies agreement and accuracy of measurements in terms of the overlap in Student’s t-test 95% confidence intervals for given bodies of data. These analyses are highly recommended and are more perspicuous than my description of them. Interestingly, they seem to reintroduce the probabilistic considerations Harper eschews when talking about provisional acceptance.

Harper objects to the traditional reading of R1 and R2. “[I]t is surely implausible,” Harper writes, “that any general commitment to simplicity ... can do justice to this sort of empirical support,” i.e., to the quantifiable resiliency of parameter values produced by agreeing measurements (p. 35).

So what's the problem? Insofar as increases in resiliency are based on real-world measurements, they can be considered “empirical successes.” However, resiliency measures nothing in the world. It is a feature of estimated parameter values in relation to given data sets. It can justify certain inferences — like the inferences recommended by R1 and R2 — but it justifies them by their effect on *parameters within theories*.

Newton, however, justified R1 and R2 (and R3 and R4, for that matter) by how *the world* is. The rules were supposed to reflect the nature of reality, which Newton believed is essentially simple, uniform, and constant. In the *Opticks*, for example, he wrote “Nature [is] very consonant and conformable to her self and very simple” (Newton, 1952, pp. 376, 397). In an unpublished preface to the *Principia*, “[f]or if Nature be simple and pretty conformable to herself, causes will operate in the same kind of way in all phenomena” (Newton, 1962, p. 307). And in the *Principia* itself, R1 is justified by “nature is simple and does not indulge in the luxury of superfluous causes” (Newton, 1999, p. 794, in all editions).¹¹ The belief in natural simplicity is, in turn, underwritten by a commitment to a providential God that has constructed the world in a way that allows His subjects to understand it, and in consequence, understand something of Him: “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” (quoted in Snobelen, 2005, p. 234, Yahuda MS).

I should be clear: Harper does *not* suggest that Newton could have used statistical methods. However, the nuance and depth of his analysis of R1, R2, and the moon-test, although illuminating as regards the data, does not match the straightforwardness of Newton’s approach to these rules, and even distances us from his understanding of them. What we learn about the moon test comes at a cost.

5. Third vignette: the evolution of the rules vs. the evolution of the *Principia*’s inferential structure

The third vignette is a generalized version of the first. Harper uses the Rules as they appeared in the final edition of the *Principia* (1726) to corroborate his account. However, the Rules underwent significant changes during Newton’s life. For example, R3 first appeared as an “Hypothesis” in the first edition (1687) and its content was significantly different than the first “Rule” version that appeared in the second edition (1713). R4 only appeared in the final edition. More cautious, epistemic language was also introduced to the Rules in the final edition, as was the troubling last sentence of R3’s explication. In contrast, the evidential and inferential structure of the *Principia* — Harper’s main source of evidence for Newton’s method — remained relatively unchanged.¹² Why did the rules evolve while the method remained mostly constant?

One answer might be that Newton was simply not concerned with spelling out his method. He provided the Rules as window dressing, and only made changes to them when pressed by external circumstance. There is surely some truth to this. Another answer,

however, is that Newton’s understanding of his own method changed. That is, that he came to be more cognizant of the complex methodological nature of his practice, perhaps through both reflection and confrontation with opponents.

Yet if Newton did not, perhaps could not, fully articulate his method in 1687, why ought we to think that he was able to do so by 1726? Shouldn’t we allow for the possibility that, *even by* 1726, there were features of his practice that he did not, perhaps could not, properly characterize?

Needless to say, this possibility opens up a historiographical can of worms. It asks us to articulate the nature of tacit and explicit knowledge attributions, and it asks us to do so in the context of the sophisticated activity of mathematical theory construction. This activity is rather different from other discursive practices, like writing fiction or philosophy. It concerns language that does not carry the same wealth of meanings as natural languages and is highly constrained. The activity is also different from embodied practices like smoothing a lens or distilling saltpeter.¹³ It concerns a myriad of small, highly technical decisions that cannot be made without appropriate mathematical justification. The knottiness of these issues, however, is not evidence that Newton must have known what he was up to.

Really, it is not the above possibility, but Harper’s book that raises these issues. His account of Newton’s practice is compelling and the practice so complex that it is hard to imagine how Newton could *not* have engaged in it entirely self-consciously. However, if he had done so, we would expect his remarks to reflect his practice to a greater degree.

6. Conclusion

One can read the previous three sections as veiled charges of anachronism. To conclude, I want to dispel this reading. At the start of this review, I noted that Harper’s real goal is to show that from a contemporary perspective Newton’s reasoning is proper reasoning. And indeed, he successfully demonstrates that Newton’s inferences (mostly) stand up to current-day standards. But I don’t think that Harper is taking our standards and applying them in alien contexts, as the charge of anachronism would imply. Rather, I think his point is that some contexts are broad, and when it comes to cosmological research, our context is Newton’s context. Moreover, our context is Newton’s context because Newton created it. (This makes accounting for my three vignettes even more pressing, but that’s besides the point.) More work needs to be done on how we can delimit boundaries for contexts that stretch over 300 years, how these contexts can be constructed by historical actors, and how we can prevent ‘broad context’ from becoming a Whiggish dodge. The success of Harper’s book is that it provides an impetus for doing so.

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¹¹ In the third edition, Newton introduced more cautious, epistemic language to the rules. However, R1 and R2 were still justified by ontological considerations.

¹² There were significant changes to Book II and later parts of Book III. Harper pays particular attention to changes of data and the justification of claims in the moon-test. However, “Newton’s method” seems to be exemplified in the first edition just as it is in the third.

¹³ See Collins (2010), Rouse (2002). I believe, but cannot argue here, that highly mathematical theory construction falls through the cracks of much of the practice literature, as well as the literature on historical meaning.

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