Comments on William Harper's

"Isaac Newton's Scientific Method"

Kent Staley

1 Harper's Newton

William Harper's forthcoming book (Harper 2011) served as the basis for a talk on "Isaac Newton's Scientific Method," presented at the Henle Conference on Experimental and Theoretical Knowledge on March 26, 2010. Here I would like to summarize the main points made by Harper in his talk and provide a perspective of my own on the issues raised by his stimulating presentation.

Harper makes a compelling case for a view of Newton's method that integrates Newton's professed disavowal of hypotheses and embrace of inferences by "deduction from phenomena" that are "made general by induction" and Newton's actual scientific practice in the Principia. The argument in Harper's book rests on careful consideration not only of the *Principia* but of Newton's correspondence with other natural philosophers of his day. It's an impressive scholarly achievement that I am sure Newton scholars will be discussing for quite some time.

But in these comments I will not attempt to adjudicate the historical question of whether Harper's understanding of Newton is completely satisfactory in light of the textual evidence. I am going to assume for the sake of argument the Harper is right about how Newton conceived of the method he espoused and the method he sought to follow in practice (this assumption should be considered operative in subsequent references to "Newton's method," "Newton's reasoning," and the like). My aim here is to consider the philosophical merits of the method that Harper attributes to Newton. What can philosophy of science gain by thinking about the problem of evaluating theoretical claims on the basis of experimental data in the way that Newton does?

More specifically, I will examine the way in which Harper's account of Newton's method can be used to highlight a crucial problem in the evaluation of experimental data as evidence for theoretical claims, but also draws attention to a powerful resource for addressing that problem.

The problem concerns the stability, or as I will term it, the security, or evidential assessments under the accumulation of new information and in particular in cases of change from one theoretical framework to another.

Suppose the claim that certain phenomena E constitute evidence for a given theory T (for example, Newton's law universal gravitation) assumes the truth of some other theoretical claim T' (for example, Newton's third law of motion, that "[t]o any action there is an equal and opposite reaction" (Newton 1999, 417)). We subsequently learn that T' is not true, or does not apply in the case at hand, and thus come to doubt or even reject outright the original claim of evidence for T.

One might worry that by making evidence claims depend on theoretical claims, we make what we learn from our data hostage to theoretical change, such that when significant theoretical change occurs we "unlearn" everything that we thought we had learned within what we might as well here call the old *paradigm*.

In another setting (Harper 2007), Harper has contested the Kuhnian claim that there is a strong connection between a theory's ontological and explanatory commitments and the methodology used to vindicate those commitments — a connection so memorably expressed in Kuhn's comment that "[w]hen paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular" (Kuhn 1996, 94). Here I am raising a concern about a different, though seemingly related, Kuhnian thesis: that scientific knowledge changes in a non-cumulative manner. I would like to use the same methodological account advocated by Harper, complemented with an epistemological framework of my own, to address this thesis.

More specifically, I will examine three challenges to Newton's argument for universal gravitation, all of which have been discussed by Harper. The first is Cotes's challenge that Newton is not justified in applying his third law of motion to gravitational attractions. The second is the challenge from vortex theories. The third, and greatest, is the successful challenge posed by Einstein's General Theory of Relativity. I will argue that Harper's analysis allows us to see how Newton successfully secured his inference to universal gravitation against the first and second challenges. I will then argue that although Newton did not succeed in securing inference against the possibility posed by Einstein's radical reconceptualization of gravity, Harper's methodology allows to see how nonetheless we need not regard ourselves as having to disregard what we learned from the data via Newton's theory. Rather, we should understand ourselves as having discovered the limits of what we had previously learned. Indeed, this may be the most distinctive feature of Harper's Newton: he espouses a methodology not only for learning about theories from data, but also for learning where the limits lie in the propositions we take to describe what we learned.

2 Harper's account of Newton's methodology

According to Harper, the key features of Newton's methodology are the following:

- An empirically successful theory must not merely make successful predictions, but must also have its parameters accurately measured by the phenomena that it predicts.
  - Example: Newton's use of area-rate dependence to measure the direction of the gravitational force: a deduction from the phenomena
- Measurements made within the framework of the theory ("theory-mediated measurements") provide an empirical basis for answering theoretical questions. Such measurements might contribute to the empirical success of a theory when they converge.
  - Example: Using Newton's gravitational theory, the mass of the sun

could be measured by the orbits of the six known planets and these measurements were in agreement.

- However, these measurements can also provide an empirical basis for casting doubt on the adequacy of a theory when they diverge.
  - Example (post-Newton): The amount of precession of Mercury's perihelion precession could not be brought into agreement with Brown's measurement of the limit on divergence from the inverse-square dependence on distance in Newton's law of Gravity.
- Theories that are sufficiently empirically successful may be provisionally accepted as guides to research.

In his book, Harper employs this account to give a detailed analysis and appraisal of the way in which Newton reasons about gravity in light of the data available to him, contrasting Newton's method with hypothetico-deductive methods and showing how Newton's method can be used to understand post-Newtonian developments in theorizing about gravity.

3 Securing inferences

In order to see how Newton's methodology addresses the concern raised previously about evidential stability in the face of the challenges posed by new information and theoretical change, it will be helpful to have a framework for thinking about evidential stability. Elsewhere I have introduced, defended, and applied a framework that I submit is well-suited for this task (see, e.g., Staley 2004, 2011a, 2011b; Staley & Cobb 2010).

We justify our beliefs by pointing to the evidence that supports them, but we can only succeed in this if that evidence actually obtains. It makes a difference then, to our success in justification, whether some fact e really is evidence for our conclusion h. Does this mean that we have to in turn give evidence that e is evidence that h? We might sometimes do just that, but if this were generally *required*, it would clearly lead to a regress. I propose that epistemologists here might learn from scientific practice, where the demands of scientific communication and argumentation require that epistemic activities be made more explicit and more publicly available than is typical of our ordinary epistemic life.

I begin with an observation about scientific practice: when presenting an argument from experimental data, scientists often undertake an explicit discussion of ways in which the claims being advanced might be wrong. In particular, when a claim is put forth that some data e resulting from an experiment T is evidence for some hypothesis h, a great deal of effort is put into thinking about ways things might be such that the claim advanced is false. Not all of this activity is reported along with the result. By the time a result is published, a great deal of effort has gone into both ensuring that the experiment was performed in such a way as to rule out many ways in which the resulting evidence claim might go wrong and thinking about what kinds of errors might have survived such pre-trial precautions.

These efforts, both pre- and post-trial, contribute in two ways to the justification of the evidence claims that are made on the basis of the experimental outcomes, and it is important for this analysis that these two tasks be distinguished. First, some such activities help create the conditions that make those evidence claims true. (This is typically, but not exclusively, true of pre-trial experimental design considerations.) Second, some such activities put investigators into an *epistemic situation* that allows them to defend their assertion of the evidence claim, by showing that some *possible scenario* is not actual in which a premise or background assumption for the inference being made would fail to be true (in what follows, I will call these *error scenarios*). It is the latter function that is the focus of my discussion, and I will begin be explaining the terminology I am using.

The term 'possible scenario' here can be thought of in terms of a "way the world might be." David Chalmers has used the notion of epistemically possible scenarios in his modal semantics, referring to scenarios as "maximally specific ways things might be" (Chalmers 2011).[1] Possible scenarios are thus similar to possible worlds, but I will continue to use the term 'scenario' to emphasize two points of distinction from most possible worlds–talk in philosophy: (1) Possible scenarios are to be understood as epistemically possible, not metaphysically or logically possible, and the modal locutions in which they are invoked are typically in the indicative rather than the subjunctive. (2) No ontological significance is here conferred on scenarios; they are not other worlds, but ways that this world might be. Moreover, Chalmers' requirement of maximal specificity has no obvious role to play in what follows. Possible scenarios will instead be invoked using abbreviated descriptions singling out some particular salient facet, such as "I might have forgotten to calibrate one of the phototubes in the calorimeter" or "I might have been looking at the wrong day's newspaper." Such abbreviations are appropriate because for the use to which I will put these scenarios, it suffices to attend to relevant *differences* between one scenario and another.

I have also invoked the notion of an *epistemic situation.* I have borrowed this term from from Achinstein (2001), who describes an epistemic situation as a situation in which "among other things, one knows or believes that certain propositions are true, one is not in a position to know or believe that others are, and one knows (or does not know) how to reason from the former to [a particular] hypothesis" (ibid., 20).

With these notions in place, then, I will introduce my proposal for an ideal of justification in two stages. First I will introduce a technical notion, *security.* I will then use that concept to define and ideal of justification: *Justified Evidence.*

Suppose that, relative to a certain epistemic situation K, there is a set of scenarios that are epistemically possible, and call that set Ω_0.

Definition (Security)**.** *If proposition P is true in every scenario in the range Ω_0, then P is* fully secure *relative to K. If P is true across some more limited portion Ω_1 of Ω_0 (i.e., $\Omega_1 \subseteq \Omega_0$), then P is* secure throughout Ω_1*.*

To put this notion more intuitively, then, a proposition is secure for an epistemic agent just insofar as, whatever might be the case for that epistemic agent, that proposition remains true. Although thus defined, security applies to any proposition, the application of interest here is to evidence claims and inferences. Specifically, an *inference* from fact e to hypothesis h is secure relative to K insofar as the proposition e is good evidence for h is secure relative to K.

It is important to stress that the methodological benefit of the security concept derives not from full security but rather from the ways in which various practices serve to *increase relative security.* I do not suppose that inquirers are ever called upon to determine the degree of security of any of their inferences. The methodologically significant concept turns out to be not security *per se*, but the *securing* of inferences, i.e., those practices that increase the relative security of an evidence claim either by expanding the range, in a fixed space of possible scenarios, across which that claim is true or by decreasing the range of scenarios that are possible in which that claim would be false.

With this terminology in hand, I propose the following as an ideal of justification:

Definition (Justified Evidence)**.** *An assertion of* h *as a conclusion inferred from observed fact(s)* e *is fully justified relative to epistemic situation* K *if:*

(1) e *is good evidence for* h*; and*

(2) the proposition "e *is good evidence for* h*" is secure throughout all scenarios*

that are epistemically possible relative to K.

This account articulates a notion of full justification as an ideal. The point is that methods of justification serve two distinct purposes. First, they aim (fallibly) to create conditions that will render (1) true for the inference at which the investigators arrive. Second, they aim to facilitate the pursuit of (2) by providing investigators with the resources to respond to the challenge of possible error scenarios and, thus, serve to secure the inference proposed. Though full security may remain an unachieved ideal, the increase in relative security puts investigators in a better epistemic situation, and it is in this sense that methods aimed at securing evidence claims provide justification.

Two strategies for securing evidence can be readily identified as playing prominent roles in evidential reasoning (Staley 2011b):

- A *weakening strategy* for securing an inference replaces a conclusion H with a weaker conclusion H' that is true across a broader range of epistemically possible scenarios.

- A *strengthening strategy* for securing an inference replaces epistemic situation K with a stronger epistemic situation K' in such a way that error scenarios epistemically possible relative to K are not possible relative to K'.

4 Securing Newton's inferences from the phenomena

Now consider two error scenarios for the law of gravity, and how Newton is able to eliminate them, while at the same time laying out the facts that suffice to make the relevant data good evidence for that law. Here I will not explain Newton's reasoning (which Harper does in detail in his book), but instead simply indicate the kind of consideration that is relevant.

First, could the universal law of gravitation be subject to a large error in the direction of the attractive gravitational force? This error Newton rules out on the basis of the fact that the rate at which areas are swept out by orbital radii can serve, as Harper explains, as a *measurement* of the direction of the attractive force. Second, could the law be subject to a large error in its specification of the distance-dependence of attraction? Again, Newton can use a theory-mediated measurement as the basis for his rejection of this possibility: the power of the distance dependence is measured by the precession of the moon's orbit around the earth.

These points are crucial to Newton's primary argument for the law of gravity. By appealing to such methods, Newton is able to argue that these phenomena do constitute good evidence for the law of gravity. What I would like to consider a little more closely is how Newton, having made that argument, secures his inference against error scenarios that threaten the underlying premises of the argument.

The first such challenge was raised by Cotes. Cotes challenged Newton's application of the Third Law of motion to gravitation. Granting that the Third Law is well justified in application to contact forces, can it be extended to forces of attraction relating two bodies separated in space? If it cannot, then this would undermine Newton's inference that makes gravitation universal: perhaps not all bodies exert gravitational forces upon one another. Cotes expressed this concern with the help of an imaginary scenario:

> Suppose two Globes *A* & *B* placed at a distance from each other upon a Table, & that whilst *A* remains at rest *B* is moved towards it by an invisible Hand. A bystander who observes this motion but not the cause of it, will say that *B* does certainly tend to the centre of *A*, & thereupon he may call the force of the invisible Hand the Centripetal force of *B*, or the Attraction of *A* since ye effect appears the same as if it did truly proceed from a proper & real Attraction of *A*. But then I think he cannot by virtue of the Axiom [Attractio omnis mutua est] conclude contrary to his Sense & Observation, that the Globe *A* does also move towards the Globe *B* & will meet it at the common centre of Gravity of both Bodies. (Letter to Newton, March 18, 1713)

Cotes' proposes an error scenario that threatens Newton's inference by undermining his reliance on the third law of motion. The error scenario in question holds that what appear to be motions produced by mutually acting centripetal forces are really produced by mechanical pushes on the orbiting body. Since the appearance of a force exerted upon one body by another is, under this scenario, an illusion, the third law does not apply.

Were Newton to simply assume hypothetically that the third law applies in this case, as alleged by Stein (Stein 1990), his inference would fail to be secure against this error scenario. Harper shows, however, that Newton does have resources for answering Cotes' challenge, which render his inference secure against this scenario. By appealing to these resources, Newton is able to strengthen his epistemic situation, ruling out the kind of error scenario invoked by Cotes. I will not attempt here to rehearse Harper's account of Newton's response to Cotes' challenge in its entirety, nor to explain the arguments in detail. Nonetheless, what follows will, I hope, suffice to establish that Newton is able to use a strengthening strategy to eliminate this particular error scenario.

First, Newton is able to appeal to other kinds of attractive forces between spatially separated body that evidently *do* exert equal and opposite forces upon one another: namely, magnetic attractions between a lodestone and a sample of iron, as Newton found in experiments that he performed (Newton 1999, 427–28). Even if these motions are the result of some kind of ether, Harper argues, as the mechanical philosophy would require, the mutual endeavor of the two bodies to approach one another would still be subject to the third law. Cotes' invisible hand does not qualify as such a mutual endeavor.

Newton's second argument appeals to the gravitational equilibrium of the parts of the earth itself. Were not the attractive forces exerted by, say, one hemisphere upon the other, not equal and oppositely directed, then there would result a net acceleration of the planet as a whole (Newton 1999, 428).

Finally, Harper explains how applying the third law of motion to the Sun and Jupiter leads to convergent results when combining independently evaluated acceleration fields. In a nutshell, the argument goes as follows: Newton can use estimates of the distances between the Sun and the planets to estimate the centripetal acceleration field directed toward the Sun, yielding an estimate of Jupiter's acceleration towards the Sun. Likewise Newton can use James Pound's data for Jupiter's four moons to estimate the acceleration field centered on Jupiter. Using the third law to extrapolate this field to the distance separating the Sun from Jupiter yields an estimate of the Sun's acceleration towards Jupiter. Using the ratios of these two quantities, one can then define a common center of rotation about which the Sun and Jupiter will orbit with a common period. This yields measures of the weight of each of the two bodies towards the other, weights that remain oppositely directed to each other as the two bodies orbit their common center. As Harper writes,

> They, therefore, fulfill one major criterion distinguishing what Newton counts as attraction from Cotes' invisible hand pushing one body toward another. To have these oppositely directed weights count as a single endeavor of these bodies to approach one another requires, in addition, that they be equal so that they satisfy Law 3. (Harper 2011)

Newton thus shows how the assumption that the third law does apply to gravitational accelerations leads to convergent estimates of acceleration fields based on distinct bodies of data, adding, according to his own methodology, to the evidence for the premises of his main argument.

Next I would like to show how Newton uses a weakening strategy to respond to a different kind of challenge. In a weakening strategy, the investigator weakens the conclusion of an inference, or opts for the weaker of two possible conclusions to an inference, so that an error scenario that would threaten to undermine the inference to a stronger conclusion is no longer threatening. The scenario may remain possible, but even if it is actual the inference remains probative.

Consider the following error-scenario: Maybe it is changes in motion of invisible particles in a vortex, such as that postulated by Huygens, that pushes the planets into orbital motion, rather than mutual gravitation of the Sun and planets.

The first thing to be said here is that, as Harper explains, Huygens' theory itself predicted that the planets would follow exact Keplerian orbits. As such the theory was only successful insofar as it predicted orbits consistent with the data available at the time. But Newton's "richer" ideal of empirical success requires more than this. Newton's application of the third law of motion, for example, yielded converging measurements of relative intertial masses in the solar system, something that Huygens' theory was unable to accomplish. Later work demonstrating perturbations to Keplerian orbits for solar system bodies, beginning in the 1740's but becoming truly compelling only in 1785 with Laplace's "Théorie de Jupiter et de Saturne," subsequently eliminated Huygen's theory decisively (though it had ceased to attract adherents well before this) (Harper 2011, ch. 9.IV.2).

But Newton's weakening strategy is a response to a less well-defined challenge. This strategy is essentially captured in his famous "hypotheses non fingo." The challenge takes the form of the following potential error scenario (or more precisely, a class of potential error scenarios specified only by a feature that they all share): Perhaps *somehow* etherial particles act in such a way as to *produce* the mutual attractions that Newton describes using the law of gravity.

Here Newton's weakening strategy is his decision to opt for a conclusion from the data available to him that is weak enough to be compatible with this possibility. I referred to this as a *potential* error scenario because it would be an error scenario were Newton to draw the stronger conclusion from his data that bodies endeavor to approach one another with forces that are attributable to gravitational attraction *and nothing else*. That he does not intend for the universal law of gravity to be given such a strong interpretation is clearly indicated by his oft-quoted statement that "I have not as yet been able to deduce from phenomena the reason for these properties of gravity, and I do not feign hypotheses" (Newton 1999, 943). In other words, his conclusion should be understood as "Gravity really exists and acts according to the laws that we have set forth and is sufficient to explain all the motions of the heavenly bodies and of our sea," rather than as, "Gravity consists of the mutual attractions of bodies acting upon one another at a distance in a way that does not involve locally acting causes (such as etherial particles).”

To be sure, Newton’s refusal to draw this stronger conclusion has an independent motivation insofar as he believes it would commit him to regarding gravitational attractions as instances of action at a distance, which he regarded as an “absurdity” (Janiak 2010). Even had he not held this commitment, however, it would remain true that such a conclusion would have been stronger than warranted, precisely because he would not have been able to rule out the potential error scenario in question.

5 Securing against theoretical change

In the end, Newton was wrong. Whatever Newton did do to secure his inference to the law of gravity against the error scenarios I’ve mentioned, he didn’t secure it against the radical shift to General Relativity.

Harper’s analysis of the transition from Newton’s gravity to Einstein’s gravity shows one sense in which that shift is not quite as radical as sometimes claimed: it was a shift underwritten by Newton’s very own methodology.

Moreover, Harper’s account of Newton’s method shows why we need not consider ourselves as unlearning what we learned before, but only the limits of what we learned. To be sure, the measured parameters in Newton’s law now appear not as the parameters of THE universal law of gravity but instead as the parameters in the weak-field limit of Einstein’s General Relativity. But the systematic dependencies that connect those parameters to the phenomena that measure them remain, and all the arguments that secured those inferences from phenomena against error scenarios that confronted them continue to support the conclusion that we learned some important theoretical facts about gravity from those phenomena. (And, as Deborah Mayo argues in her paper in this volume, it is more fruitful to think of experimental tests of theories in terms of what we learn *about* those theories than in terms of conferring confirmation or probability on those theories.)

That this is so places an important limitation on the Kuhnian view that scientific revolutions (such as the shift from a Newtonian to an Einsteinian view of gravity) prevent scientific knowledge from growing in a cumulative way. Gravity may not be *the kind of thing* that Newton thought it is, but the evidence from which we learned of Newton's error on this point is perfectly consistent with the evidence on which Newton based his conclusions about *the facts on which the behavior of gravity depends*.[2]

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Notes

[1]Hintikkas seminal 1962 takes expressions of the form It is possible, for all that S knows, that P to have the same meaning as It does not follow from what S knows that not-P. Just how to formulate the semantics of such statements is, however, contested (see, e.g., Chalmers 2011; DeRose 1991; MacFarlane 2011; Salerno 2009; Kratzer 1977).

[2]This point will no doubt be recognized by adherents to the epistemic structural realism advocated by Worrall and others (Worrall 1989). My point here is not to uphold epistemic structural realism as a general thesis, but rather to point to the kind of methodological efforts undertaken by Newton — and clarified by Harper — to secure the kind of continuity in the face of radical theory change that structural realists claim as support for their view.