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Theory, Evidence, Data: Themes from George E. Smith

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Introduction

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This volume grew out of the conference *On the Question of Evidence: A Celebration of the Work of George E. Smith*, which met at Tufts University in May 2018. It featured presentations by Jody Azzouni, Katherine Brading, Robert DiSalle, Allan Franklin, Michael Friedman, Bill Harper, Teru Miyake, Eric Schliesser, and Chris Smeenk. A number of those presentations evolved into chapters included here. To them we have added chapters by other scholars and thinkers who have been influenced, directly or indirectly, by George's work. Collectively, these pieces aim to honor that work by engaging with prominent themes from it.

In what follows, we begin with a brief introduction to George's philosophy of science, and to his more prominent and distinctive contributions to doing it. Thereby we highlight some of the challenges it poses to long-held commitments within philosophy, history and sociology of sciences. Then we elaborate on key themes in his philosophy as we summarize and introduce the chapters in this volume. We conclude with some remarks on George as a teacher, and on the philosophical impact of his teaching.[1]

George Smith on evidence in mature science

The history of evidential reasoning, if viewed over extended periods rather than in isolated episodes, and regarded as more than illustrations to support philosophical positions, could produce a decisive transformation in our understanding of scientific knowledge. Current views of the nature of scientific knowledge have been forged, in large part, in response to the threat of radical theory change made prominent first by Kuhn, then by subsequent historiography of scientific practice. To fend off that threat, any claim to have achieved a distinctive form of permanent knowledge must establish some form of continuity through transi-

[1] This volume began as a project jointly co-edited by Smeenk, Schliesser, and Stan. An encounter with long Covid led to Eric Schliesser being unable to remain at the helm as co-editor.

tions, such as from Newtonian gravity to general relativity and from classical mechanics to quantum theory. Many scholars have concluded that claims to establish secure knowledge should be dismissed as a misconception, reflecting a false image perpetuated by science pedagogy, for example, rather than genuine insights into what science has achieved. In addition, the program to find a single logic of scientific method or scientific inference seems to have failed—in virtue of accumulating evidence showing that actual scientists are often opportunistic in their practices. In so far as such projects still remain (e.g. Bayesianism or various kinds of causal modeling), they have a highly normative character.

Yet such a dismissive response to claims about the epistemic status of many sciences fails to account for evident progress in understanding the natural world. It is hard to deny that various scientific fields have succeeded in establishing theoretical claims that, in many respects, have also served as effective guides for action, even for those skeptical about whether the theories themselves have any claim to permanence.

George Smith has often described his work as inspired by asking how we came to have high quality evidence in any field. In particular, how have scientists in successful fields turned data from observations or experiments into strong evidence for substantive claims? What is the nature of the knowledge claims that have the best case for being firmly established? And in what sense, if at all, can we take these claims as permanent, stable contributions to scientific knowledge? In pursuing these questions he has developed a striking, distinctive account of the nature, scope, and limits of scientific knowledge. He based his account on historical and philosophical investigations of exemplary evidential reasoning spanning extended lines of research (over decades or centuries) in physics.

George considers extended lines of research for several related reasons (see the paper reprinted in this volume). He largely agrees with historians and sociologists who have argued that the scientific community typically accepts substantive claims based on surprisingly weak evidence. But he reframes the question of high quality evidence as more appropriately posed with regard to the consequences of accepting some claims as a first step in a line of inquiry. Rather than, for example, considering only the case Newton could make in favor of gravity at the time of writing the *Principia*, we should keep in mind the case Simon Newcomb could have made two centuries later. George strikingly disagrees with historians who are tempted to generalize from their apt criticisms of local case histories, often at the *advent* of a line of inquiry, to a general claim that would apply to Newcomb as well as Newton. His approach also contrasts with philosophers of science, who have too often focused exclusively on the kinds of arguments that scientists make in motivating the community to pursue new ideas—such as an emphasis on novel predictions. The strongest evidence in favor of a well-established theory often differs from the kind of case that could have been made for it when it was first introduced; or even when it found widespread acceptance. More importantly, what is epistemically distinctive about science—what enables it to make progress—will be more clearly discerned in how a line of research unfolds subsequently rather than in the initial debates.

This position leads to a distinctive innovation in how George practices philosophy of science—an innovation that has led to a new genre of philosophical scholarship. Many of his studies are working papers or case studies toward the development of what one might call a *longue durée* review article. They can be book-length pieces, as his famous 'Closing the Loop' attests. In such a *longue durée* review article, George surveys and evaluates how theory development and a field's evidential practices (which are constitutive of the field) interacted over an extended period of time.

The shift to a long view is only useful in concert with Smith's characteristically thorough assessment of what is actually being put to the test through subsequent research. This means that in practice George often redoes the calculations—and pays careful attention to attempts to replicate earlier results, as well as to the way that scientists assess the evidence in their own time as presented in textbooks and ordinary review articles. This practice is manifest in both his case studies and the more substantial, retrospective review articles. He redoes calculations not just from meticulous care to understand how past scientists would have evaluated the evidence, but, in particular, so as to enable his readers to assess which results or measurements were constitutive or evidentially relevant for particular lines of inquiry.

By contrast, philosophers of science too often rely on an abstract, hypothetico-deductive account of theory-testing: scientists test a theory T by deducing its consequences, then checking these via observation or experiment. According to this view, Newtonian gravity, for example, is tested by deducing predictions for planetary positions given some information about the initial conditions. One of the leitmotifs of Smith's work is that an exclusive focus on successful predictions neglects crucial aspects of scientific practice. Treating theories as a monolith obscures the fact that evidence bears *differently* on specific aspects of the theory. George has emphasized, for example, the stark contrast between ample evidence for the inverse-square variation with distance of the gravitational force, as opposed to the weak evidence for its dependence on the mass of both interacting bodies. The hypothetico-deductive account also misses the role of a different type of claim: in the case of planetary astronomy, calculations of planetary positions are based on the substantive assumption that the masses and forces taken into account in the derivation are complete. But if the comparison with observations requires a claim of this kind, what can we say is actually being tested?

Paying attention to the role that theories play in guiding an extended line of inquiry leads to a richer understanding of the logic of theory-testing, developed in detail for the case of celestial mechanics in George's magisterial 'Closing the Loop.' Based on surveying nearly 250 years of planetary astronomy, he argues that the main question being pursued was not, as philosophers would have it, whether Newtonian gravity "saves the phenomena." Indeed, throughout the period he considers there was only a brief time during which the existing models fully fit with available observations of planetary motion. At all other times there were systematic discrepancies between the calculated and observed motions. Celestial mechanics used these discrepancies to discover further physical features of the solar system.

The main aim of this line of research was to identify robust physical sources for existing discrepancies—such as new planets, but also subtle physical effects such as the changing rotational speed of the Earth—and then "close the loop," by adding this new feature to a more complete model. George once put it as follows:

> Newton's *Principia* forced the test question within orbital astronomy for his theory of gravity to be not whether calculated locations of planets and their satellites agree with observations; but whether robust physical sources can be found for each systematic discrepancy between those calculations and

> observation — with the further demand of achieving closer and closer agreement with observation in a series of successive approximations in which more and more details of our solar system that make a difference become identified, along with the differences they make.[2]

Pursuit of this iterative approach, adding further details to develop ever more sophisticated models and identifying even more subtle features of the solar system, lead to an enormously rich picture of, as Smith puts it, the configurational details of the solar system and the differences they make to observed motions. The identification of what George calls 'second order phenomena' depends on a contrast between the motions, as described at a specific stage of inquiry, and increasingly precise and carefully targeted observations. Success at each stage in discovering more subtle effects that have clear physical sources indirectly confirms the earlier steps in the process. A discrepancy at any given stage only has physical significance, and helps to identify another detail that contributes to observed motions, if the earlier calculations have incorporated the features with larger impacts accurately.

What makes 'Closing the Loop' so compelling is George's masterful history of this field, combined with the philosophical and technical acuity needed to recognize and articulate the logic of theory-testing that this history discloses. Part of his insightful analysis pertains to the specifics of celestial mechanics. And George often insists, in print and in person, that particular evidential strategies may not generalize to other fields; that one first has to do the work in reconstructing the *longue durée* evidential practices of them. This reluctance stems from George's recognition that each field faces often highly specific challenges to developing evidence, regarding (to put it roughly) accessibility to the kinds of quantities that allow for fruitful theorizing. This challenge is general, but giving a full account of how to respond to it quickly becomes intricate and domain specific.

Before we articulate (despite George's reticence) some general features of his approach to evidential reasoning, it is worth noting that his detailed work also corrects another tendency common among historians

[2] This is the first of three principal conclusions stated in the manuscript version of 'Closing the Loop,' which do not appear in the published text.

of philosophy. We have emphasized above that his work can be understood as incorporating and accepting the findings of those focused on scientific practice at a given time—and yet, by shifting toward extended periods, his research undermine their nominalist and skeptical tendencies. It is also worth noting that—while George reiterates the emphasis on theories familiar from an older historiography, e.g. Koyré—he undermines the older view that theories alone can explain important scientific debates. His work shows that often attitudes toward particular lines of empirical evidence shape how theories are adhered to.

George is cautious about generalizing from the results of his life-time of research. Still, we think there are also general features of his approach to evidential reasoning, revealed in other detailed case studies—such as his recent monograph (with Seth) on early-20th century experimental investigations of molecular reality. Here we will highlight two interrelated aspects of his account, before considering their implications for questions of continuity.

First, physical theories typically re-describe phenomena by introducing novel quantities, such as mass and force in Newtonian mechanics, and law-like regularities that hold among them.[3] This immediately raises a challenge: how do scientists reliably gain access to the proposed fundamental quantities, and justify their use? George's work shows the value of taking *these* questions—rather than obtaining successful predictions—as the central challenge. Scientists usually proceed by exploiting functional relationships between the target quantity of interest and a "proxy" quantity (or quantities) that can be measured (more) directly with high precision. Experimental design focuses on finding measurable proxies with a particularly clear and reliable connection to the target quantities. The role of theory is to establish such functional relationships, to enable *theory-mediated measurements* of the target quantities—that is, to show how the theoretical quantities are manifest in, and can be constrained by, observable phenomena, and can give rise to second-order phenomena. But, crucially, success in theory-mediated measurements provides evidence for the aspects of theory used to derive the functional relationship.

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[3] A note on our terminology: by 'phenomena' we mean roughly what Newton meant by it: a robust empirical regularity accepted by a scientific or expert community.

This success can take several forms: stability in the outcomes of repeated measurements of the same type; convergence in the values determined by measurements of different types, based on different functional relationships; and amenability to increasing precision. Smith and Seth (2020) clarify how each of these types of success contribute to justifying the use of novel physical quantities, and make a persuasive case that success in all three senses can eliminate the possibility that the target quantity is merely an artifact or useful fiction. On their account, early 20th century physicists established reliable access to the microphysical realm (of atoms and molecules) through stable, convergent measurements of three crucial parameters: Avogadro's number, N_o; the fine structure constant, α; and the charge of the electron, e. Contemporary physicists now rely on collaborations such as the CODATA group to determine consensus best values for dozens of fundamental parameters. Rather than seeing these as merely values to be inserted to facilitate comparisons with observations, on George's view the stable, consistent determinations of these parameters over time provides evidence for the array of physical theories that fix the relevant functional relationships used in these measurements.

To describe the second aspect, we can turn to a feature of Newton's methodology that George has elucidated wonderfully: namely, his response to the challenge of drawing conclusions from phenomena despite their complexity. Smith and Harper (1995) aptly characterized this approach as a "new way of inquiry" in their seminal paper, drawing a sharp contrast between Newton and 17th century contemporaries such as Galileo and Huygens. Consider Newton's realization, as he was revising the *De Motu* drafts that would lead to the *Principia*, that the planets do not travel along closed orbits. Due to the mutual gravitational interaction among the planets and the sun, they would instead move around a common center of gravity and follow trajectories too complex to admit of a simple geometric description. Rather than give up on the project of determining the forces from observed motion in light of this remarkable complexity, Newton developed the mathematical framework needed to make inferences regarding the force that hold even if the orbits can be described as only approximately elliptical. The first aspect of Newton's response to complexity is to establish functional relationships between observations and target quantities that are robust, in the sense that an *approximate* description—thus not necessarily exact—of the observed phenomena can still yield an approximate value of the target quantity. Borrowing a Latin phrase that Newton used frequently, George often describes this in terms of 'if *quam proxime* (very nearly) then *quam proxime*' reasoning.' In the context of reasoning from observed phenomena of motion to forces responsible for them, Newton proves that, if the (observed) antecedent holds very nearly (*quam proxime*), then the (inferred) consequent also holds *quam proxime*.[4] (We will discuss an example of this type of result momentarily.)

This is coupled with a recognition that a specific type of idealized representations play an essential role in guiding further inquiry. Astronomers in the 1670s faced an underdetermination problem: there were several distinct—and inequivalent—ways of representing planetary motions. Newton recognized that the planets only follow *approximately* Keplerian orbits for theoretical reasons, but in the case of the lunar orbit the departure from Kepler's area law was well-established observationally. What then is the status of the Keplerian orbits? As George puts it:

> The complexity of true motions was always going to leave room for competing theories if only because the true motions were always going to be beyond precise description, and hence there could always be multiple theories agreeing with observation to any given level of approximation. On my reading, the *Principia* is one sustained response to this evidence problem.

Newton recognized that the Keplerian orbits (unlike the alternatives) hold in idealized, counterfactual circumstances: if the sun were at rest, and the interactions among the planets neglected, then a planet would follow a Keplerian orbit. They thus have a physical status that alternative descriptions lack. Clarity regarding exactly what must be the case for Keplerian orbits to hold—counterfactually—also makes it possible to treat departures from Keplerian motion as second-order phenomena: as a means to identify further physical details relevant to orbital motions. In the case of the moon, despite enormous challenges to developing a

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[4] However, we must note that Newton employs his phrase, *quam proxime*, in several semantic regimes, not just one. As a matter of fact, a closer study of these regimes is part of George's current research on approximation in physical theory construction. Relatedly, see also the chapter by Guicciardini in this volume.

satisfactory theory, Newton could make the case that the departures from Keplerian motion were due to the perturbing effects of the sun. More generally, idealizations stating regularities that would be *exact in precisely specifiable circumstances* support the identification of discrepancies. In the best case, the character of these discrepancies further indicate which assumptions of the idealization have to be modified or relaxed.

Although we have focused on George's account of Newtonian methodology to elucidate this approach to complexity, we expect an account along these lines holds for much of modern physics, where it is constrained by robust background theory. His own work highlights how similar considerations apply to the study of molecular reality, and to the structure of molecules (see Smith and Seth 2020 and Smith and Miyake 2021, respectively). More broadly, there is a striking similarity between this account of methodology and the effective field theory approach in modern particle physics, developed by Ken Wilson and others.

Taken as a whole, George's account of methodology leads to a fundamental reframing and response to concerns about permanence and continuity stemming from Kuhn. Even profound transitions at the fundamental theoretical level do not necessarily generate discontinuities in evidential reasoning.[5] Following George's insights, the focus should be on whether the functional relationships presupposed in evidential reasoning continue to hold in light of a new theory—for example, as an approximation within a limited domain. This has a family resemblance to a kind of structural realism that his friend, Howard Stein, has espoused, or the real patterns that his long-time Tufts colleague, Dan Dennett, has defended. But, in George's work the emphasis is on the challenges to sustaining a particular line of evidential reasoning—not on ontology. In one sense this is a much weaker demand than what philosophers typically consider, and one that is often imposed by scientists developing new theories, but in another sense it is much more ambitious: a record of success in obtaining stable, convergent measurements, and of identifying further details that make a difference in an iterative approach, in fact make a compelling case that we should demand continuity of evidential

[5] This is not to say that Smith rejects the possibility of Kuhnian discontinuities in evidence; Buchwald and Smith (2002) consider the discontinuity in the treatment of polarization through the transition from ray to wave optics.

reasoning, just as Einstein took recovery of a Newtonian limit as a criteria of adequacy in formulating general relativity. Finally, even though the evidential reasoning itself presupposes and depends directly on (some aspects of) theory, the standards of success do not. Whether a line of research has led to increasingly precise, convergent measurements of fundamental parameters, or continued to identify robust physical sources that can be checked by a variety of independent means, can be evaluated in relatively theory-neutral terms.

We hope that this brief précis of one aspect of George's thought is sufficient to show that it has deep ramifications for central debates in history and philosophy of science. But, we also want to highlight a different aspect of his work. As we noted, he approaches historical material with a meticulous attention to detail—perhaps inspired by his working practice as an engineer interested in failure of complex systems (in particular, turbine engines)—often leading to profound reassessments of canonical texts, or lines of argument, that one would expect to have few surprises left to divulge. Several philosophers have, for example, taken Perrin's case in favor of atomism based on his own experimental measurements of Brownian motion, in conjunction with an array of other results, as exemplifying a successful argument in favor of unobservable entities (while disagreeing, of course, about the precise nature of the argument or the correct conclusions to draw from it). Smith and Seth (2020) show that many aspects of the philosopher's lore about this case, even those that are particularly relevant to the case for atomism, are simply incorrect. For example, Perrin's own measurements of N_0 were rejected within a decade after the publication of *Atomes*, due to a substantial and persistent discrepancy with other precision measurements, threatening philosophical arguments that rely uncritically on Perrin's claim of sufficient agreement among the different measurements.

George's most influential contributions along these lines have led to a dramatic re-evaluation of Newton's achievements and the reception of Newton's work. Smith and Schliesser (2000) give an account of the early response to Newtonian gravity that corrects lore about the debate between Newton and his continental critics, in particular Huygens and Leibniz. Those debates are not best understood as merely a clash of metaphysical systems, but as driven, in large part, by the limitations of available empirical evidence. In addition, George's critical reading of

Newton has revealed aspects of the argumentative structure of the *Principia* that seem to have escaped notice from Newton's own time. Our favorite example concerns an argument that Newton did *not* make, even though it is often attributed to him: namely, that the inverse-square law of gravity follows from an observed Keplerian ellipse. Even a relatively cursory reading of the *Principia* reveals that Newton did not actually give this argument, but Smith gives an extremely insightful analysis of the argument he actually gave and what it reveals about Newton's methodology. The incorrect argument requires that the antecedent holds exactly: if a body moves on an ellipse with a force directed at one focus, then the force varies as the inverse square of the distance. But Newton also proved that if a body moves on an ellipse with the force directed at the center, then the force varies directly with distance. The planets all move on nearly circular orbits, so it is extremely challenging to draw a distinction between these two cases observationally. Any method that depends on establishing exact results observationally is extraordinarily fragile. By contrast, the argument Newton actually gives relies on a functional relationship that is robust. The apsidal precession theorem establishes a functional relationship between the motion of the apsides (the points on the orbit closest and farthest from the sun) with each orbit and the exponent of the force law (for approximately circular orbits). For a closed orbit, with no motion of the apsides, the force varies with the inverse square of distance. But, crucially, this result also implies—and this is an instance of 'if *quam proxime* then *quam proxime*' reasoning noted above—that for orbits with *nearly* stable apsides the force law is *nearly* inverse-square.

Themes from George Smith: a synopsis

George's work has made an impact on epistemology, the history of philosophy of science, and Newtonian scholarship. In casting new light on the *Principia*—its argument structure, epistemic support, and long term posterity—he has changed research agendas, and opened new vistas onto old problems and figures. The papers in this volume attest to that broad influence.

Epistemology

In work on Newton and in collaborations on late-classical physics, George had devised and relied on a tripartite division of the epistemic status a proposition can have in the course of research. Specifically, he has distinguished between a proposition counting as a hypothesis; being 'taken to be true'; and being established. What underpins this distinction? As we explained above, it is the quality of the evidence for that proposition—at a given time; and over long stretches—and the *constitutive* role assigned to it during ongoing research that involves it.

Knowledge. In his paper, Jody Azzouni asks how Smith's taxonomy of epistemic stances dovetails with some central distinctions in traditional epistemology. He argues that propositions taken to be true (by Smith's criteria for that notion) really count as *knowledge*. To make his case, Azzouni distinguishes two concepts of knowledge, viz. ordinary and philosophical. The latter is an artifact of conceptual engineering: it has an inbuilt constraint, namely, that knowledge be infallible. However, Azzouni sides with the ordinary notion—the one behind vernacular uses of 'to know.' This notion, he explains, does not require infallibility (about the specific proposition *P* at issue). And so, he concludes, if a community of inquiry has enough evidence for taking *P* to be true, then it knows that *P*.

Azzouni then finishes *à rebours*. From the vantage point of his dual picture, he examines Descartes' notion of knowledge. His verdict is that Descartes endorsed the infallibility condition, notwithstanding the Cartesian distinction between *scientia* and *cognitio*. Moreover, Azzouni argues provocatively, Descartes' above endorsement appears to have contaminated Newton's thought too, in two respects. First, both Newton and Descartes take deduction to be—if suitably carried out—a procedure that cannot fail. Second, both think that a proposition counts as knowledge only if it has been 'deduced.' There are, of course, key differences between their idea of deduction and our modern concept. Still, Azzouni concludes, Newton shared with Descartes the commitment that 'deduction' is the gold standard for knowledge, and knowledge requires infallibility.

This line of argument suggests two directions for further research. It should prompt us to take a deeper look at Descartes' and Newton's respective accounts of deduction, and how they took specific mathematics—frameworks and approaches—to underwrite the presumed infallibility of their 'deductions.' On a broader note, it encourages us to inquire into when and why the early modern tenet of infallibility (of knowledge) gave way to the current situation in which fallibility is the default position in epistemology.

Strong evidence. George first uncovered the very special role of systematic discrepancies in Newton's work, and their legacy for gravitation theory in the centuries after him. When they are systematic, such discrepancies become very valuable, as second-order phenomena in the sense described above: they drive research forward, by pointing to aspects of the theory that need refining or strengthening; and they yield *strong* evidence for the theory, if it accounts for them from its own resources. This particular insight suggests two lines of further research. Do systematic discrepancies play this role in science more broadly, beyond research in Newtonian gravitation, where Smith first uncovered it? And, did anyone else between Newton and Smith grasp the special value that discrepant phenomena have for research in the exact sciences? The paper by Teru Miyake takes up these questions, and answers them compellingly. In his recounting, it was the natural philosopher John Herschel who in the 1820s explained the evidential value of 'residual phenomena,' or observed discrepancies—between (first order) predictions from theory and the observed behavior of target objects. Such phenomena further confirm the theory, if it can account for them; or they require—and may even suggest—revisions to theory, if its laws cannot handle those 'residues.' Though he saw the method as working at its best in physical astronomy (thus foreshadowing, as it were, Smith's magisterial paper of 2014), Herschel seems to have thought the method was more general, potentially useful in many areas of exact science. His lucid case for it would influence key figures of Victorian logic and philosophy of science, as Miyake shows.

At the same time, against the backdrop of George's complex picture of evidence we presented above, we can see more clearly the limits of Herschel's insight into the confirmatory role of 'residual phenomena.'

Empiricism and metaphysics. A recurring theme in Smith's work is how, in the process of theory building, Newton always took great care to anticipate how he might go wrong, viz. how the evidence available falls short of supporting his physical claims. Robert DiSalle's paper takes up that theme, and extends its reach. In particular, he asks whether Newton took the same evidential care in regard to the *philosophical* foundations of his theory, not just the physical details of the solar system. DiSalle argues in the affirmative: he points out how much empirical import attaches to some of the key foundational terms in the *Principia*—absolute velocity, relative motion, true rest, uniform translation, absolute rotation, and the like. In his recounting, Newton gradually found a way to demarcate which of these terms are empirically well-grounded, and which ones are fated to remain 'hypothetical.' The key, DiSalle explains, was Newton's gradual refinement of the relativity of motion, and the resulting insight into how to formulate a concept of inertia compatible with Corollaries V and VI to the laws of motion.

Alongside his philosophical analysis, DiSalle offers a nuanced reconsideration of historical figures like Berkeley and Mach, commonly known as critics of absolute space and time. He suggests that their grasp of 'Newtonian relativity' was really deeper than we have appreciated so far. More broadly, we can see in his paper a plea, inspired by George's picture of Newtonian evidence, to reflect on the various ways in which a metaphysics could be empiricist—by way of looking at the empirical credentials of the metaphysical ingredients proffered as foundations for physical theory.

Measurement and evidence. A very important theme from Smith is the evidential value of measurements. In his reconstruction, when measured values of a key parameter converge over time, they count as evidence (for the theory in which the parameter is embedded).

That evidence is even stronger when that particular ingredient—the expression that connects a theory-bound quantity with a measured parameter—can be further refined. As we explained above, behind such expressions, or functional dependencies, are tacit idealizing assumptions. If these assumptions get relaxed (with the functional expression refined accordingly); and if new, further measurements bear out the newer, refined formulas linking theory to metric proxies—then that counts as strong evidence for the theory.

Allan Franklin's study brings this theme to the fore. His diachronic survey highlights how attempts to measure a certain parameter (the mean density of the earth) yielded more exact values in the long run: from Cavendish to the late 19th century. At the same time, all such experimental attempts have to grapple with the challenge of disaggregation. Namely, the need to identify factors—temperature gradients, friction damping, material-specific properties—that might lead to discrepant measurements; and to screen them off, or at least to estimate their effects, so as to subtract them from discrepant phenomena.

In this respect, Franklin's paper dovetails with the studies by Miyake, and Biener and Domski in this volume. They too single out for reflection Smith's emphasis on discrepant phenomena, and to what extent we can marshal such discrepancies in support for theory.

Evidence in the sciences of the past. Certain key elements in Smith's account of strong evidence—for instance, the emphasis on systematic discrepancies, and on converging values of a measured parameter—suggests that such evidence is available just in advanced, strongly mathematized science. Which implies, inter alia, that the sciences of the past—and disciplines that study one-off events, in particular—are at an evidential disadvantage. That is because their specific domain exhibit neither repeatable patterns (from which we could discern *systematic* discrepancies); nor *serial* values of measured parameters.

In recent years, however, Carol Cleland and others have argued that the historical sciences are not at an evidential disadvantage, relative to the physical sciences above. Craig Fox's paper takes a critical look at the reasons for their epistemic optimism. In their account, these sciences rely on a common pattern of confirmation. Specifically, they infer from present traces of past events—artifacts, material remnants, physical leftovers and byproducts of extinct processes—to some common cause responsible for those traces. Underwriting this generic pattern of inference is a key assumption, which these authors call the 'Common Cause Principle.' Fox inquires into the warrant for this assumption. That warrant, he explains, is a claim further upstream, namely, that the past is overdetermined. That claim is really two ideas. One is epistemological, and says that, for us to infer reliably to some past event, we do not need all (causal) traces of that event as evidence for it—just *some* traces are enough. The other regards ontology, and says that past events always leave at least *some* causal traces into the present.

These authors think their key claim—that the present overdetermines the past—follows from David Lewis' analysis of causation in terms of possible worlds. However, Fox shows, the analysis does *not* support their metaphysical premise—because in his account Lewis excluded certain sets of possible worlds by fiat. In particular, Lewis' analysis ignored 'backtracking counterfactuals.' But that move, as Fox points out, in effect excluded—illegitimately—the possibility that our present is causally and nomically compatible with many, *different* pasts. And so, their key premise (that the historical past is overdetermined) holds by stipulation, not in virtue of metaphysical argument. Thus Fox's incisive scrutiny undermines (or challenges) one case for optimism about evidential reasoning in the historical sciences—and leaves the important task of developing a more compelling case open to further work.

Fox's piece is on the cutting edge of recent studies that investigate the structure of confirmation in science about the past. That enlarges and sharpens our picture of the descriptive import and explanatory power of George's account of evidence.

Newton scholarship

George's research has drawn renewed attention to Newton's methodology—a generous term that covers both heuristics, or guidance for "reasoning more securely" in natural philosophy, with empirical results appropriately guiding inquiry; and also logics of confirmation, viz. patterns of evidential reasoning, and constraints on admissible inferences.

Newton on methodology. In regard to the former—heuristics—Monica Solomon in her paper seeks to uncover a new aspect of Newton's thought on that topic. She argues that a key element in the famous Scholium on space, time, and motion is best understood as a piece of methodology. In particular, Solomon claims, Newton meant his example of the two globes (connected by a cord, rotating around each other in empty space) as a terse blueprint for setting up, and tackling, problems in orbital dynamics. As she explains, the globes are an epitome of the type of dynamical system that Newton studies in *Principia*. The example requires us to think of the globes as having the quantitative properties Newton sets down in his definitions—properties that covary in accordance with his laws of motion—and as being sufficiently far away from perturbing factors that we can treat the globes as a quasi-isolated system (a supposition that Newton at the end of his book, in the General Scholium, seems to reaffirm).

Thereby, Solomon breaks with a long tradition (going back to Mach) that saw Newton's globes example as making a point about the metaphysics and epistemology of true motion. In effect, her paper makes a case for further study of Newton's heuristic resources—a topic that harmonizes nicely with George's analyses, and which is sure to reward further scholarship on it.

The Principia and hypotheses. From his earliest work in optics and the disputes it prompted, Newton always tried to demarcate results he had established from "mere hypotheses." Much of George's work is a reconstruction of how Newton developed a method to 'reason more securely' in natural philosophy—by contrast with the explicitly hypothetical methods of his contemporaries. Huygens, for example, clearly endorsed a version of reasoning from hypotheses. The chapter by Zvi Biener and Mary Domski revisits the broad idea that, at least in mathematical mechanics, Newton did not reason from, nor did he endorse, hypothetical assertions.

For the case of the 'mathematical' Book I of the *Principia* (and its application in Book III to gravitational phenomena), Newton's rejection of hypotheses appears unimpeachable. However, when they turn to Book II—the mathematics of motion in resisting media, plus Newton's experimental basis for it—Biener and Mary Domski think that his rejection of hypotheses looks shakier. For one, Newton there starts from assumptions about the dynamics of a resisting medium: *suppositions* about which of its properties are causally relevant; and about their kinematic effects (on a solid moving in that medium). For another, it is hard, they argue, to find in Book II the key elements that allowed Newton to rise above hypothetico-deductivism: robust theorems (for inferring force laws from motion phenomena); convergent values of a measured parameter; or a mathematical treatment that allows him to disaggregate the respective contributions (of various physical causes and mechanisms) to a complex motion effect, e.g. the decay of pendulum swings in water. But, if those sources of evidential reasoning are lacking, Biener and Domski conclude, isn't Newton's pattern of confirmation in Book II closer to Galileo's and Huygens' hypothetical approaches than we have thought so far?

The architectonic of the Principia. The argument structure of Newton's treatise has long been an elusive puzzle. Katherine Brading in her chapter sheds new light on this difficult topic. The *Principia* unites two disciplines, or areas of inquiry, she argues. Books I and II establish results in rational mechanics—the applied mathematics of orbits induced by central forces of interaction. In contrast, Book III turns to physics: a theory of a force (gravity) seated in bodies, its effects on them, and its quantitative relations to other properties of material bodies, like mass, shape, and volume. According to Brading, in the *Principia*, these two disciplines are not simply juxtaposed; they are connected by a conceptual bridge, as it were. That bridge is the definitions and 'axioms, or laws of motion' that Newton placed at the outset, before his three Books. These elements serve a dual function in the treatise: they make up the axiomatic basis of his mathematical mechanics; and they are the nomic basis of his quantitative physics of gravity.

This framework allows Brading to elucidate the diachronic context of his theory, not just its synchronic makeup. In particular, she explains, with the *Principia* Newton continued, while transforming very drastically, a program that goes back to Descartes. It was the program of combining rational mechanics with physics into a 'philosophical mechanics,' as Brading calls it (because physics then counted as a branch of philosophy). He expanded the scope of rational mechanics well beyond what anyone had done by 1700. And, he showed how the physics of gravity is amenable to mathematization. Specifically, well established empirical methods of measurement give us a quantitative handle on the effects of gravity. From those effects, Newton's rational mechanics lets us infer the strength and direction of the gravitational forces responsible for them; and to treat these forces as endowed with a measure, or algebra. Thereby, Brading's study dovetails with George's well-known emphasis on Newton treating forces as quantities, viz. actions endowed with a ratio structure, or measure.

Mathematical methods. The *Principia* is famously geometric: geometric objects (e.g. lines, plane curves, areas) stand for properties of motion and force; and geometric methods (e.g. auxiliary constructions, diagrammatic reasoning) are among its key vehicles for proof in rational mechanics. Newton's reliance on this geometric framework—its scope of representation, heuristic merits, inferential limits, and equivalence to other frameworks then available—have been a topic for much scrutiny. Debates began already in his time, and scholarship in the last half-century has greatly advanced our understanding of Newton's formal methods. The chapter by Niccolò Guicciardini makes a major contribution to that understanding, by shedding light on a difficult, elusive, and little studied topic. Broadly described, that topic is integration in Newton's mathematical thought: its meaning, scope, key techniques, and comparative virtues. Guicciardini establishes a number of novel and important results. One is that, overall, Newton was a good deal more of an 'algebraist' than the *Principia* might lead us to expect. At least in matters of integration, he clearly favored proof procedures that rely on the rule-governed manipulation of algebraic formulas—rather than inferring from inspection of a geometric object. Another major result is that Newton had an incipient notion of differential equation, and had worked out ways to solve some classes—primarily, by expansion into power series; by numeric approximation, where feasible; and by change of variable, or substitution.

This particular result is subtle, but matters greatly, in two respects. For one, as Guicciardini notes, Newton's approaches to 'fluxional,' or differential, equations differs from our modern approach (which goes back to Leibniz and his disciples) that seeks analytic, closed-form solutions for them. We may be tempted to think that difference counts as a weakness of Newton's fundamental concept of 'fluxion.' But the reason lies elsewhere, as Guicciardini explains: many of Newton's intended applications—for the techniques above—are in the dynamics of perturbed systems. In general, those systems are *not* integrable in closed form. His approaches (numeric integration, and expansion into infinite series) are excellent approximations of those generally unavailable exact solutions. And so, what prima facie looks like a weakness is in fact a source of strength. For another, it helps us see that the gap between Newton's mathematical methods and modern formulations (of gravitational dynamics) is not as great as a casual look at the *Principia* might suggest. Thanks to Euler and Lagrange, mechanics settled into the form familiar to us late-moderns, viz. of a connected set of differential equations (of motion) derived from dynamical laws. In that regard, the *Principia* with its geometric language looks separated by a gulf from our versions of mechanics. Guicciardini's study reveals that to be an exaggeration, ultimately. Though he lacked a function concept (and the Leibnizian notation that eases their calculus so greatly), Newton after all did have *the* key ingredient of modern mechanics: describing the motion (over an instant) by way of a differential expression, which then must be integrated. Guicciardini's paper resonates with another theme from George Smith, among whose breakthroughs has been the careful study of Newton's mathematical methods at the more advanced stages of theory construction in the *Principia*.

History of philosophy of science

George's work has opened new lines of research on the diachronic aspects of foundations for mechanics, and on how major philosophical figures responded to Newton's achievement. A number of chapters in this volume explore these new lines.

The status of gravity. A widespread view has it that Newton was averse to metaphysical inquiry into the objects and results of the *Principia*, e.g. the ontology of gravity; and that when competitors took issue with his physics on metaphysical grounds, they were ultimately ill-advised to do so. In his paper, Andrew Janiak gives good reasons to resist this picture. His starting point is Newton's own attempts to make sense of his key result, in Book III, proposition 7, that 'gravity is in all bodies universally.' Namely, to elucidate its real semantic content, underlying ontology, and the broader metaphysical framework that supports them. As Janiak shows, this was no easy task, and Newton grappled with it at length. Nor was the answer clear and uncontroversial—not even to his fellow travelers like Roger Cotes, let alone to antagonists like Leibniz. Newton's struggles with this central question—what does it really mean to assert that gravity is universal—required him to make forays into the very metaphysics that he allegedly eschewed. Janiak's study has another, implicit benefit (in addition to turning the tables on the received view). In particular, it breaks a new path to better situating other, important figures (such as Emilie du Châtelet and Kant) in their own efforts to elucidate the meaning of gravity being universal.

Kant and Newton. George's careful work on Newton's approach to evidence has yet another important benefit: it has opened a new vista on

18th-century philosophers' dialogue with the *Principia*.[6] Michael Friedman's paper takes a further step on this new ground, by looking at Kant's response to these aspects of Newton. He thinks there are two significant aspects of this response. Kant strongly endorsed Newton's idea —which George was the first to highlight for us—that theory-mediated measurement is a very significant source of confirmation in gravitational dynamics. However, Friedman claims, Kant favored a notion of phenomena that is thicker than Newton's analogous concept. Specifically, Newtonian phenomena of motion were exclusively kinematic: patterns of planet- or satellite motion over time, equilibrium configurations, and the like. The notion Kant preferred, however, is that of phenomena as 'involving causal and dynamical information' as well, not just kinematic content. Friedman argues in favor of Kant's stronger notion, because he thinks it does useful work in contemporary philosophy of science: it can help us chart a middle path in the disputes between realism and instrumentalism (about the relation of theory to its target objects).

The paper by Katherine Dunlop unfolds in the same register as Friedman's investigation above. Thereby, our image of Newton's reception by the philosophers after him becomes clearer. At a critical juncture in his argument for universal gravity, Newton in Book III relies on his third law of motion. That key move won him few friends, it seems. On the Continent, some objected that it asserts bodies to interact without contact, which they dismissed as unintelligible. Others, like Roger Cotes —and also Euler, later in the century—demurred that Newton lacked enough evidence to claim the law applies to actions at a distance. In her study, Dunlop uses Kant so as to cast a new, and more positive, light on Newton's move.

Kant endorsed action-at-a-distance early in his career, and so he had none of the Continentals' qualms about it. Accordingly, his reaction to Newton's *Lex Tertia* was different. For one, Dunlop explains, he argued that Newton relies on that law well before the master argument in Book III. In fact, he needs it already in Book I; specifically, in section 11, where Newton shows how to reduce the two-body problem (of two par-

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[6] Studies of that dialogue are old, to be sure. However, before Smith they tended to be rather one-sided, narrowly focused on the inertial-kinematic basis of the *Principia:* its metaphysics of space, time, and 18th-century reactions to it. Representative for that work are Earman 1989, Friedman 1992, and Rynasiewicz 1995.

ticles interacting as they orbit around each other) to two separate, more easily tractable one-body problems, viz. of a mass in Kepler motion around a fixed center of force. Kant thinks that Newton needs to assume the third law for his approach to go through, but that he did not acknowledge it as an explicit premise.

For another, Dunlop adds, Kant's natural philosophy helps elucidate some important but otherwise baffling claims by Newton. Kant distinguished between 'dynamical' and 'mechanical' treatments of force. The former is quantitative, or mathematical, but it has causal import—it regards forces as causes (of acceleration) that a source could exert even while stationary. From that vantage point, Dunlop argues, Newton's treatment of force in section 11 counts as 'dynamical,' hence causally relevant. That would resolve the apparent tension between his well known claims that he treated gravity merely 'mathematically,' and yet that his treatment shows gravity to 'really exist,' and 'suffice for' celestial and terrestrial motions.

Foundations of classical mechanics. The three laws of motion that Newton asserted purport to be general: they apply to forced motion beyond the relatively narrow class of gravitational effects. Many theorems in Book I—stating relations between accelerations and their corresponding orbits—are about forces other than inverse-square and placed at a focus (which gravity is). And, Newton in a famous passage conjectured that many things led him 'to have a suspicion that *all* phenomena may depend on certain *forces*,' and hoped his theorems and methods will help his posterity discover those further forces (Newton 2004, 60; emphasis added). To be sure, many after him did continue his agenda. Still, a mere century after the *Principia*, Lagrange created a genuine alternative to Newton's foundation. His book, *Mécanique Analytique*, unified statics and dynamics from a dual basis—a principle of virtual work, and a postulate known as 'D'Alembert's Principle.' In some respects, Lagrange's framework is more powerful than Newton's. The chapter by Sandro Caparrini takes a closer look at *Mécanique Analytique*. It shines a light on the layered structure of that key treatise, and on the early growth of the theory it contains. In Lagrange's lifetime, the book went through two editions; in the interim, French mathematicians extended greatly the reach of mechanics, to novel and difficult phenomena. Caparrini shows convincingly how Lagrange was able to incorporate those new advances into his framework, turning it into an even more formidable competitor for the tradition of theorizing that came out of the *Principia*.

Caparrini's study resonates with an important theme from George Smith, though not explicitly. In Katherine Brading's terms above, the theory in Lagrange's book is a rational mechanics, not a physics. Warrant for its results cascades downstream: from its basic, most general principles, by deductive reasoning alone. In *Mécanique Analytique*, empirical facts are very scarce, adduced mostly as illustration, not confirmation. That raises the weighty question, where does empirical evidence—especially strong evidence, of the kind that Smith has so fruitfully explored so far—enter Lagrange's 'analytic' mechanics? The question is far from easy, to be sure, but it is to be hoped that scholars will take it up to grapple with it. Thereby, another theme from Smith would come to the fore, namely, that confirmation in advanced, strongly mathematized theories is *diachronic*: it is temporally extended, and rests on a historical record of accumulating, and increasingly stronger evidence for the theory.

Early scientific cosmology. The mathematical astronomy in Ptolemy's *Almagest* rests inter alia on a small number of extra-mathematical assumptions—about what is at rest, what moves, and how far the stars are from us. Ptolemy there merely gestures at argument for these assumptions, or 'hypotheses,' as he calls them. His proffered support for them in that book is cursory and rash; which feeds the suspicion that the *Almagest* is a collection of simulation software, as it were: mere algorithms for predicting or retrodicting ephemerides and select orbital parameters—not a genuine 'system of the world.' The basis for that system would come from physics, we may think. In particular, from Aristotle's philosophical physics, which—thanks to its doctrines of natural place, motion, and five elements—easily entails that the earth is at rest in the world center while stars and planets revolve around it.

In his chapter, the late Noel Swerdlow subverts this received wisdom. He does so by way of a synoptic study of *Planetary Hypotheses*, an important tract by Ptolemy. That work, Swerdlow argues, contains a theory of cosmology, or system of the world. But, it is not based on metaphysical premises from Aristotle. In fact, it is properly scientific. Specifically, it is quantitative: it makes claims about celestial distances and orbital parameters. Likewise, it is empirical: the inputs for theory building are empirical givens, e.g. long term observational data or patterns of perception; and the evidence for the theory is empirical as well. And, Swerdlow suggests, it is supported by physical assumptions, e.g. about the causal mechanisms of planetary motion, and about the physical consequences of counterfactual setups. Thus, Swerdlow concludes, Ptolemy's *Hypotheses* has every right to count as a scientific cosmology; indeed, it was the first one of its kind. To help the reader, he ends with a synopsis of the complicated transmission and reception of Ptolemy's theory above.

There is a broader lesson here, and it lies at the confluence of two strands of thought. For one, George's painstaking work has shown inter alia not just how subtle Newton's methods for gathering evidence were —but also how easy it was for many figures after him to miss those methods. For another, the papers above show how unclear—and far from obvious or uncontroversial—the foundations of Newton's theory were, in the century after him: its ontology, basic semantics, and generic methods. Together, these two strands suggest a revisionist conclusion that challenges an interpretive consensus going back to Kuhn's *Structure*. In that influential book, Kuhn had argued that when paradigm-making work transforms its field (into an arena for 'normal' science) it *ends* previous controversies (about the ontology and methods suited for that domain), and it produces consensus (about the basic objects, acceptable methods, legitimate problems, and criteria for solutions). Kuhn counted Newton's *Principia* among the epitomes for his case (Kuhn 1996, 11, 17). But, the chief results in the papers by Friedman, Solomon, Janiak, and Dunlop cast increasing doubt on Kuhn's picture of the *Principia*'s role in the history of exact science. Thereby, these results help clear the field for a new generation of scholars to step in and determine what Newton's book really did to physics and its philosophy—in effect, how 18th-century philosophers and their successors answered 'Newton's challenge to philosophy' (Schliesser 2011).

This volume ends, appropriately, with George E. Smiths' reflections on the two crafts—the philosophy and history of exact science—that he has cultivated and fostered so admirably. The occasion for his reflections is the theme of revisiting accepted science. More specifically, the diachronic process whereby pieces of theory—once they become 'accepted,' or used constitutively to carry out further research—get tested over and over again, often with increased stringency; and the long-term outcomes of such 'revisiting.' He illustrates this process with examples, discussed in exquisite detail, from gravitation theory and late-classical physics. These examples, and others, support his concluding message—really, a dual lesson for students of science. Philosophers who investigate the epistemic aspects of science ought to pay close attention to the diachronic side of its credentials as knowledge: for any given theory, they must study the history of the evidence for it, in Smith's memorable phrase. And, historians of science ought to avoid dogmatic allegiance to the idea that social-group dynamics holds the master key to understanding the birth and growth of scientific knowledge. Rather, they would do well to pay attention to the extended record of testing and revisiting the epistemic credentials of that knowledge as it grew.

To both communities above, in sum, George emphasizes the crucial importance of *longue durée*, fine-grained study of the confirmation processes behind the production of scientific knowledge. These processes begin when a theory has been accepted, not *before*. And so, a corollary of his lesson is that we ought to revisit—and be prepared to drastically revise—Kuhn's old picture of normal science. Thereby, his lesson resonates with the dominant note of the papers in this volume.

De magistro

George's work is unique as well in a respect that makes it hard to present synoptically in any introduction, not just this one. He has conveyed much of his philosophy of science by a route that goes beyond the standard of our time, viz. the journal article or book chapter qua discrete, printed units of research. In particular, that route has been his legendary two-semester course on Newton's *Principia*—really, a master class in the history and philosophy of evidential reasoning developed at Tufts University, but offered at a number of other institutions (including Stanford, Notre Dame and Duke). Roughly, the first semester puts the student in a position to read the *Principia* by studying 17th century primary texts (including Galileo, Kepler, Huygens, and pre-*Principia* Newton). The second is a close reading of the *Principia*, theorem by theorem; and then an overview of Newton's impact on mechanics after him.

The course is pitched to undergraduate students, but often the auditors include graduate students and faculty. For many years it was offered on Wednesday evenings with a three hour time slot interrupted by modest breaks. George *lectured* by partially reading from amazing lecture notes and using the blackboard when necessary. (The lecture notes would be made available after class, and after further careful editing.)

What made George's lectures mesmerizing was that he took all the students at all levels seriously as genuine interlocutors in the shared adventure of understanding Newton's method. And what made the whole point even more remarkable: many sessions would start with his excitement of his latest discovery—sometimes an overnight discovery—of the evolution and development of Newton's thought.

The paper assignments for students taking the course for a grade were all clearly designed to foster a collective endeavor to understand the evidential status of particular works at a given time. An assignment could read: 'what was the status of Kepler's laws in 1680?' This could open the door to more metaphysical papers on what exactly the nature of a Keplerian law was in the late 17th century; or to examinations of 17th-century discussions of Kepler in astronomical texts of the period.

George's pedagogic methods center on very high expectations from his students by assigning challenging primary texts (and a lot of them) without flipping the classroom. What he does do, and he does this amazingly well, is prepare the student *qua* student to be a co-equal in his astounding intellectual adventure. He makes sure they acquire all the technical background, one firm step at the time, and then puts them in the position to contribute to active research, if they so wish. (Many students end up writing term papers that could be the basis of a journal article.) Subsequently, George would often come to co-author with students, by drawing on their specialized mathematical, linguistic, or research skills. In turn, his lectures along the years become enriched by what he learns or discovers while collaborating with them or grappling with their questions. Along the way, he invites them out on frigid winter nights to stare through a telescope, so as to experience what Galileo might have felt when he turned one toward the Moon. (George makes sure to let them try to see *anything* with the magnification that Galileo had available.) Through his course, which he has taught for nearly three decades, George has reached and influenced some four generations of research, from senior luminaries to current graduate students. Inter alia, the chapters in this volume attest to the enduring influence of his teachings.

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