chapter 8, section 4 Timpson does very little more than make gestures at these potential positive contributions. Yet it is precisely these that I, and I am sure many others, would have preferred to read about. In fairness to Timpson, however, it is sometimes necessary to clear the road of obstacles before one ventures to travel down it.

In any case, let me reiterate that Timpson's book is a must read for those interested in the topic of quantum information theory. Moreover, it is an important contribution to the philosophy of information theory in general, and I have no doubt that it will be much discussed in the years to come.

Michael E. Cuffaro, Ludwig-Maximilians-Universität München

Peter Achinstein, ***Evidence and Method: Scientific Strategies of Isaac Newton and James Clerk Maxwell***. New York: Oxford University Press (2013), xv+177 pp., $24.95.

This book contributes valuable new insight into what philosophers of science can learn from the scientific methodologies of Newton and Maxwell. Each of these great scientists was also an extraordinarily able philosophical thinker about method and evidence.

Peter Achinstein has long been well known for developing philosophical analyses of concepts of evidence (e.g., his *The Book of Evidence* [New York: Oxford University Press, 2001]) and is an important defender of the objectivity of evidence and its role in the philosophy of science (*Evidence, Explanation, and Realism* [New York: Oxford University Press, 2010]). He motivates this book with what he describes as the problem about evidence generated by a challenge from a former dean of his, who was a physicist: "How are we supposed to apply your theory to real cases?" (39). Achinstein's chapters on Newton and Maxwell are a response to this challenge.

Achinstein argues for interpreting Newton's famous, and much discussed, Rules for the Study of Natural Philosophy as rules of strategy for guiding empirical investigation aimed at finding general causal laws. A fundamental feature of Achinstein's interpretation is his claim that whether you have followed the rules in an empirically defensible way needs to be established independently of the rules. This is a valuable contrast between Newton's rules and the sort of logical inference rules familiar to many philosophers of science today.

Achinstein discusses three methods Maxwell describes for situations in which you do not have a theory that you can establish or confirm. These discussions are a valuable contribution toward having philosophers of science engage with exploratory research in science, rather than just look at problems of assessing evidence for established theories.

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Achinstein's first chapter offers a review of, and philosophical motivation for, definitions of conceptions of evidence he has developed in earlier work. These definitions include an interesting conception of objective evidence relativized to an epistemic situation. They also employ a concept of probability, construed objectively, as degree of reasonableness of belief. His insight that "in general, whether *e*, if true, is evidence that *h* is an empirical, not an a priori, question" (25) is motivated by effective counterexamples to reject the assumption that whether *e* is evidence for *h* is a matter to be determined by a priori calculation not empirical investigation.

Chapter 2 is devoted to Newton's rules for reasoning in natural philosophy. Achinstein begins by listing them and briefly discussing each. He informatively points out that Newton's rule 4 (Isaac Newton, *The "Principia": Mathematical Principles of Natural Philosophy*, trans. I. B. Cohen and A. Whitman [Los Angeles: University of California Press, 1999], 796), in conjunction with the "vera causa" clause in Newton's rule 1 (794), suggests some central ideas of scientific realism. Scientists should aim to arrive at propositions that can be provisionally accepted as true, and not merely as saving the phenomena, and by using causal and inductive reasoning they can arrive at conclusions that can justifiably be regarded as true. Achinstein calls attention to the anti-Cartesian stipulation that the force of an inductive conclusion is not blunted simply by imagining a contrary hypothesis to explain the phenomena. His discussion of Newton's use of the term "phenomena" correctly points out that phenomena are accepted not as rock-hard data but as starting points that can be corrected as investigation proceeds.

Achinstein gives a brief summary of Newton's (*Principia*, 802–11) basic argument for his theory of universal gravity. A more detailed treatment (W. L. Harper, *Isaac Newton's Scientific Method: Turning Data into Evidence about Gravity and Cosmology* [Oxford: Oxford University Press, 2011], chaps. 3–8) of this basic argument offers more support for Achinstein's positive assessment of it.

Achinstein offers his strategic counterpart interpretations of Newton's rules as very general rules of strategy for achieving the aim of establishing a general law that invokes a cause to explain a range of phenomena. One excellent feature of these strategic counterpart rules is that they are not based on Newton's (*Principia*, 794) expressed Occam's razor principle that assumes the world is simple.

In chapter 3, Achinstein argues for Newtonian extensions of the methodology he has characterized as corresponding to that endorsed by his counterpart interpretations of what Newton explicitly cites in his four rules. Achinstein adds an additional rule 5 to cover the application of universal gravity to demonstrate additional phenomena endorsed by Newton (*Principia*, 382; *Optics; or A Treatise of the Reflections, Refractions, Inflections and Colors of Light*,

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4th ed. [New York: Dover], 404–5). As George Smith ("Closing the Loop: Testing Newtonian Gravity, Then and Now" [lecture, Tufts University, February 22, 2007], http://www.stanford.edu/dept/cisst/visitors.html; "Revisiting Accepted Science: The Indispensability of the History of Science," *Monist* 93 [2010]: 545–79) has argued, the corrections to solar system phenomena to take into account gravitational interactions achieved by Newton's successors added empirical support for Newton's theory that far exceeded the empirical support afforded by his basic argument for it. Achinstein proposes to take his strategic counterpart rules 1–4 supplemented by his new rule 5, together with a causal-eliminative variant, to represent the core of Newton's methodology (92). He argues that Newton's methodology is superior to versions by Whewell and by Lipton of the method of inference to the best explanation.

In chapter 4 Achinstein discusses three methods Maxwell describes for situations when you do not have a theory that you can establish or confirm. The first of these is what Achinstein calls the method of physical analogy that Maxwell described in his paper "On Faraday's Lines of Force." In this paper Maxwell uses the analogy of fluid flow to develop a detailed mathematical extension of Faraday's representation of electromagnetic phenomena by lines of force on a test particle for each point in space around a body that is either electrically charged or magnetic. The detailed mathematical extension Maxwell develops is our important modern concept of an electromagnetic field. Achinstein quite informatively points out that Maxwell explicitly counts his fluid as imaginary. Fluid flow is a useful analogy for helping to develop the mathematical organization of electromagnetic phenomena, not a causal explanation of them. It would be interesting to get Achinstein's account of the evidence that led to the eventual acceptance of the field-theoretic account over rival Newtonian, action at a distance, accounts of electromagnetic phenomena.

The second method of Maxwell's that Achinstein considers is an exercise in mechanics employed in his ground-breaking first paper on the kinetic-molecular theory of gasses. In this 1860 paper Maxwell introduced the assumption that molecules are spherical and interact only by contact. Achinstein emphasizes that what Maxwell is doing in this paper is showing that it is possible to understand the behavior of gasses by reference to mechanical causes. He also points out the problem of correctly recovering the empirically determined ratios of specific heats that Maxwell counts as among what Achinstein takes to be "hitherto unconquered difficulties" (156).

The third method of Maxwell's that Achinstein considers is a method of physical speculation developed in Maxwell's 1875 paper, "On the Dynamical Evidence of the Molecular Constitution of Bodies." Achinstein points out that in this later paper Maxwell uses a very general virial equation and does without the assumptions that molecules are spherical and interact only by contact. Here, according to Achinstein, Maxwell has obtained more than

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mere possibility. He has been able to develop a theory with respect to whose basic assumptions he was justifiably in a "confident but less than perfect belief state" (171). Achinstein quotes Maxwell: "The law of molecular specific heats is less accurately verified by experiments, and its full explanation depends on a more perfect knowledge of the internal structure of a molecule as we yet possess" (165). Maxwell appears confident that what is needed to correctly account for specific heats is more perfect knowledge about details of molecules, even though he does not count his epistemic situation as one in which the existence of molecules is established.

Achinstein takes successful applications of what he identifies as Maxwell's three methods to a set of phenomena to produce a type of understanding, even though it does not show that those phenomena constitute evidence sufficient to establish some theory, hypothesis, or law. I believe it would be quite informative to follow up what Achinstein has discussed here by, expanding on his excellent earlier discussion (e.g., "Is There a Valid Experimental Argument for Scientific Realism?" *Journal of Philosophy* 99 [2012]: 470–95) of the evidence supporting the acceptance of the reality of molecules, discussing the very compelling and interesting way that research generated by problems with molecular specific heats did lead to more perfect knowledge of the internal structure of molecules (see Clayton A. Gearhart, "'Astonishing Success' and 'Bitter Disappointment': The Specific Heat of Hydrogen in Quantum Theory," *Archive for History of Exact Sciences* 64 [2010]: 113–202).

William Harper, University of Western Ontario

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