Metascience
DOI 10.1007/s11016-012-9703-0

ESSAY REVIEW

Thinking about Achinstein's philosophy of science

Gregory J. Morgan (ed.): Philosophy of science matters: The philosophy of Peter Achinstein. Oxford and New York: Oxford University Press, 2011, 300pp, $39.95 HB

Mark Newman

The nineteen papers in this collection provide sophisticated critical analysis of Achinstein's work, rather than being merely essays on related issues that are dedicated to him. This is a significant plus for those who wish to learn the intricate details of Achinstein's thought. The papers focus on Achinstein's theories of evidence, scientific realism, induction, and explanation. I only have space here to touch on the primary criticisms to, and Achinstein's replies regarding, evidence and realism. The papers I will not discuss by Cat, Di Fate, Goldstein, Kronz, Laudan, Morgan, Norton, Richards, Ruse, and Woodward, are also of very high quality and deserve considerable attention.

Evidence

Achinstein has developed four different concepts of evidence, but argues that scientists are interested only in what he calls "veridical evidence" where the following conditions on evidence e, hypothesis h, and background information b are met:

1. p (there is an explanatory connection between h and e/e and b) $> 1/2$
2. e, b, and h are all true
3. e does not entail h
4. There is an explanatory connection between h and e

This definition is objective in the sense that evidence is independent of any particular person or group, and it is empirical in the sense that determining whether e is evidence requires empirical investigation rather than a priori analysis. The notion of probability used in (1) is objective as is the concept of an explanatory

M. Newman (✉)
Rhodes College, 2000 North Parkway, Memphis, TN 38112, USA
e-mail: newmanm@rhodes.edu

Published online: 11 October 2012

connection, which is given by the way the world is: if there is a correct explanation for e in terms of h, or h in terms of e, or both in terms of a common hypothesis, then there is an explanatory connection between e and h.

Readers new to Achinstein's work will wonder what use it is to give such an abstract characterization of evidence. Surely, the average scientist has no idea whether hypotheses are actually true or even whether they really are connected to evidence in the way she imagines. To those more familiar with his approach, there is no surprise. Since his book *Concepts in Science*, Achinstein has adopted the method of precise explication of scientific concepts. For him, this means using the tools of conceptual analysis as well as the history of science to clarify the meaning of statements of the form "e is veridical evidence that h." He is not in the business of providing a general account of how to determine if some particular fact is evidence for some hypothesis. He is in the business of providing a clear definition for the meaning of "evidence" both as it is used and as it should be used by scientists. This is very nicely explained and emphasized by Gimbel and Maynes in the opening essay of the collection.

What is quite shocking about the papers devoted to this topic (Cartwright, Doppelt, Kitcher, Longino, Mayo, Richards, Staley) is that many of the authors seem to so blatantly overlook this important point. Doppelt for instance accuses Achinstein of failing to provide a characterization of evidence "relevant" to scientists, where relevance is supposed on Doppelt's account to include the ability of a scientist to determine whether e is in some particular case actually veridical evidence for h. This is an epistemic requirement where the subject must be able to pass judgment on the evidential relation in some given context. Achinstein, however, rejects this demand. His aim is to provide a definition of the meaning of evidence rather than a set of conditions which tell us when we have got it.

Perhaps, some of the confusion is because in *The Book of Evidence* Achinstein uses both conceptual and historical arguments. Conceptually, he argues against weak notions of evidence which require only that e increases the probability of h, arguing instead that scientists should use a notion of evidence where it provides objectively "good reasons to believe" rather than just some reason to believe. Historically, he argues against a priori approaches to defining evidence by pointing to historical cases, such as Thompson's discovery of the electric charge on cathode rays. The evidence against cathode rays being neutral particles was empirically revealed by the deflection of phosphorescent illumination in cathode-ray tube experiments. Achinstein argues therefore that evidence is an empirical, not an a priori matter.

Achinstein's argumentative strategy may be confusing to begin with, but he clearly explains in the second chapter of that book that his aim is not to capture what justifies a scientist in treating some fact as evidence for a hypothesis, but rather with defining in an objective empirical manner what evidence claims mean. To drive home that point, he argues that his mind-independent concept of evidence is preferable to alternatives because this is just what scientists are after. As he likes to put it, the sense of evidence he is using is like the sense of "sign" and "symptom." There is an objective sense in which regardless of what anybody thinks or believes, spots on one's belly are a sign of measles. No one has to recognize them as such, but there they are: objective evidence for measles.

Doppelt is not the only one to push against Achinstein's objective notion of evidence. Kitcher complains that the concept of evidence Achinstein uses is not relevant to scientists because they want their own judgments to "accord with the evidence" in a way Achinstein's definition cannot. Kitcher's worry is that although scientists want veridical evidence (in that evidence really should support the judgments we make), it is far from obvious how this can help scientists make their judgments accord with the evidence. He sees this as on a par with telling scientists to believe the truth but not telling them how to recognize it. Achinstein's response is that the aim Kitcher pushes is an epistemic issue of justification rather than the semantic project on which Achinstein is focused. The criticism therefore misses the mark.

We see this issue arises again in Staley's essay which criticizes Achinstein for unpacking the wrong notion of evidence, instead advocating for the importance of what he calls "epistemic situation" (ES) evidence—e is evidence for h if it is reasonable to take e as evidence for h given the beliefs of those in that situation. Staley has his own take on how to modify Achinstein's account of ES evidence, but the overarching problem is that Achinstein's critics reject the semantic project and embrace the epistemic. Achinstein's reaction to this general criticism is strong: who cares if we have ES evidence? The important sense of evidence is clearly veridical. Scientists want evidence that is a sign of the truth, not merely reasonable to believe as supporting the hypothesis given ones epistemic situation.

Is Achinstein correct in thinking scientists want a concept of evidence defined as (1)–(4)? His argument for this claim is sprinkled throughout his *Book of Evidence* but can be found in section 10 of chapter 2 appropriately titled "What type of evidence is most important for scientists?" Here, he argues historically that when Thompson discovered the negative charge on cathode rays, his goal was to provide a good reason to believe his experiments conclusively proved his conclusion. From this, Achinstein concludes Thompson's "aim was not simply to provide potential evidence. It was rather to provide potential evidence that is also veridical." (Potential evidence requires that e is true and the reasoning from e to h is correct, but allows that h may be false). And to argue the same point, he next shifts to a thought experiment: Sam has spots on his belly, and on Monday, his doctor takes these to be evidence for measles. On Friday, test results reveal Sam does not have measles, but some other rare virus that generates the same spots in 1 % of cases. The spots are still potential evidence for measles because they indicate measles in most cases, but they are not veridical evidence because if they were then Sam would have measles (since veridical evidence requires the truth of the hypothesis). The doctor seeks veridical not merely potential evidence. So, too does the scientist Achinstein claims. Who after all would think e was evidence for h if h was false?

The problem here seems to be that when Achinstein asks what type of evidence is most "important" for scientists, he is ignoring an obvious ambiguity. Veridical evidence is surely what any scientist would like in the sense that not only are there good reasons to believe h in light of e, but also h is true. Still, since we have to admit that it is often impossible for the scientist to know that they have obtained veridical evidence, what use is this concept over and above either ES or potential evidence? The distinction is analogous to the externalist epistemologist who argues that what is most important in our notion of justification is the process by which we come to

hold a belief, rather than the internalist approach which might emphasize being able to actually give reasons for holding that belief. We can appreciate the distinction but which one captures the more "important" aspect of justification? Intuitions pull in both directions here, and for Achinstein to think he has shown scientists want one concept of evidence rather than the other is premature.

The analogy holds also for Achinstein's notion of "explanatory relation," which of course plays a crucial role in his account of evidence. The concept is left almost undefined, Achinstein taking an explanatory relation to hold between e and h if either one correctly explains the other or if some other hypothesis explains both. All other considerations about explanation will be pragmatic. This is not very enlightening because we have no explication of what a correct explanation is supposed to look like. Again, Achinstein is going objective and refuses to play the game of providing conditions for an explanation, instead settling for the semantic task of defining "explanatory relation" as an "external" concept. Again, this will be frustrating for those who had hoped for a more edifying discussion on explanation.

Realism

The papers by van Fraassen and Psillos address Achinstein's arguments for scientific realism. Achinstein has argued that Perrin's experiments give good grounds for believing in the reality of molecules and atoms. Van Fraassen provides an alternative interpretation of the Perrin case, reading the scientists involved as looking to "empirically ground" their theories rather than prove them true. (Empirical grounding requires that theoretical components of a theory are determinable or calculable only by connecting them to observably measurable quantities using assumptions within the theory itself.) The idea is that Perrin can just as well be read as an empiricist. Achinstein's response is twofold:

1. The theoretical assumptions used by Perrin to independently derive Avagadro's number in multiple different ways are each argued for by appeal to other experimental work by other physicists working with other theories, so the circularity is not localized as van Fraassen seems to think. The strength of these arguments is also much greater than usually appreciated.
2. Perrin (and Maxwell, who is also discussed in van Fraassen's criticism) is clearly a scientific realist.

While (1) is well argued by Achinstein, (2) is not, which makes it look like he is flatly refusing to play the interpretation game.

Psillos poses a different sort of challenge, one that will be of more interest to most scientific realists. With his own reconstruction of Perrin's argument, Psillos argues that Achinstein's case is too weak, only establishing that the probability of an explanatory relation holding between e and h is greater than 1/2, where this is clearly not enough to establish the existence of a theoretical entity. Achinstein's response is to reiterate that his definition of evidence although only requiring this high a probability does not establish the actual probability in any given case. It is open, Achinstien thinks to still make empirical arguments for the likelihood being

much higher, as Psillos requires. But that is an empirical and scientific issue not a philosophical concern.

There is a final point that arises here of significant interest: Achinstein's rejection of Psillos' notion of second-order evidence. For Psillos, there is evidence that scientists have for a hypothesis (first order) and then there is also evidence regarding science more generally (second order). He appeals to the track record of science which motivates the pessimistic induction as an example of the latter and counsels us that we need to balance these two types of evidence, where such balancing is a context-dependent matter. For the Perrin case, he argues that the first-order evidence trumps the second order. Achinstein, however, thinks we need no such weighing of evidence: there is no explanatory connection between first-order and second-order evidence so the latter cannot be evidence in conflict with the former. The idea is simply that a philosophical–historical assertion about the failings of past science cannot either explain some empirical fact, be explained by some empirical fact, or be connected via some common hypothesis that explains both.

This is an important point in defense of scientific realism for if one can simply ignore the pessimistic induction, then one has potentially solved the single most pressing concern for scientific realism. But the move fails, for one surely can imagine an explanatory connection between first and second-order evidence. For example, if we found that failed theories in science have all suffered from common unreliable methods of investigation then surely that would explain both the failure of those theories, and assuming those same methods are in use in current theory, it would explain the falsehood of current theory. This clearly is an explanatory connection between the two levels of evidence contrary to Achinstein's claim.

Achinstein might object that physical facts are what explain the truth or falsity of our theories, not appeals to scientific methodology. Molecular motion explains the pressure in a tube of gas, not how we discovered it. However, it is quite plausible to explain our theory of molecular motion being true because of the physical facts and because of the reliable methods we used to develop it. To privilege one type of explanation (material) at the expense of another (intentional or methodological) is, to use Achinstein's own point, a matter of pragmatics. Anti-realist concerns therefore are not so easily dismissed.

Lastly, there are a couple of down sides to this collection: the essays are regrettably quite brief, no doubt due to publisher constraints, and this is frequently a frustration to the reader because these papers are in general of very high quality. They are written by many highly accomplished philosophers of science, and it would be nice to see a fuller account of their difficulties with Achinstein's work from each of them. The brevity results in the reader having to work hard to extract much of the philosophical detail hidden in the papers and even take on faith some of the historical claims since there is no room for the use of extended quotes and examples. This is most amplified with the historical essays by Cat and Laudan. Another general drawback of the collection is its organization. Papers are sequenced alphabetically by author surname, which although undoubtedly avoiding some editorial dilemmas does detract from the reader's experience since a topic-based sequence would be far more useful. Most of the papers focus quite narrowly on one

or another of the topics listed above, and it would be organizationally preferable to collect papers on similar topics together.

That, however, is where my negative comments on the book end. This is a wonderful collection of papers written on the work of a first-rate philosopher, and careful study of these essays is amply rewarded. Achinstein has thought long and hard about these issues, and his critics are mostly similarly experienced and insightful. With the inclusion of a final chapter with replies to his critics, this is a comprehensive collection on Achinstein's philosophy of science that will edify those familiar with his work as well as opening the door to his views for the uninitiated.