Philosophy of Science matters: the philosophy of Peter Achinstein (edited by Gregory J.Morgan) Oxford University Press: New York (300 p.) 16,5 cm x 2,5 cm x 24 cm

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Dit boek is een aaneenschakeling van essays geïnspireerd door het werk van Peter Achinstein, één van de meest prominente wetenschapsfilosofen van zijn generatie, Jay and Jeanier Schottenstein University Professor aan de Yeshiva Univerity, en professor aan Johns Hopkins University. Naast 8 volumes te hebben geedit, is Achinstein de auteur van 7 boeken, waaronder 'The Nature of Explanation' (1983), 'The Book of Evidence' (2003) en het recente 'Evidence, Explanation, and Realism' (2010). Zijn boek 'Particles and Waves: Historical Essays in the Philosophy of Science' (1991) kreeg de Lakatos Award in 1993, de meest prestigieuse prijs voor een wetenschapsfilosofisch boek. Vreemd genoeg, is dit volume het eerste boek dat volledig gewijd wordt aan Achinstein's ideeën en de reacties hierop.

Dit boek bestaat uit 19 korte essays die verscheidene aspecten van Achinstein's corpus adresseren, elk gevolgd door gevatte reacties van de meester zelf. Sommige leggen zijn visies uit, sommigen bekritiseren ze, andere breiden ze uit en sommigen doen het alle drie. Hoofdstuk 1, geschreven door Steven Gimbel en Jeffrey Maynes, presenteert een historisch overzicht van Achinstein's wetenschapsfilosofische aanpak; de daaropvolgende bijdragen zijn in alfabetische volgorde geordend naar auteur. Alle behandelen zij Achinstein's meest recente werk over de notie van wetenschappelijk bewijs. Nancy Cartwright, Gerald Doppelt, Philip Kitcher, Helen Longino, Deborah Mayo, Richard Richards en Kent Staley onderzoeken allen hoe het Achinstein's conceptie van wetenschappelijk bewijs vergaat als deze geconfronteerd wordt met een breed gamma gevalsstudies of wanneer zijn visie vergelijken wordt met tegenstrijdige visies op bewijs. Richard Richards, bijvoorbeeld, argumenteert hoe en waarom Achinstein's bewijstheorie, in tegenstelling tot standaardtheorieën over bewijs, in staat is om te gaan met raadselachtige episodes in de wetenschapsgeschiedenis. Zo behandelt hij Charles Darwin's geloof in evolutie volgens aftakking vooraleer deze een mechanisme (later bekend als natuurlijke selectie) beschikbaar had, en diens klaarblijkelijk inconsistente visies over het al dan niet gejustifieerd zijn van deze gedachtegang. Specifieker kunnen Achinstein's noties van 'subjective' en 'epistemic situation evidence' hier een rol van betekenis spelen. 'Subjective evidence' is bewijs dat afhangt van wat een bepaald subject geloofd en waarom deze iets geloofd, terwijl 'epistemic situation evidence' objectieve bewijs omvat aangezien dit niet afhangt van de werkelijke overtuigingen van individuen maar relatief is aan een epistemische situatie binnen een bepaald tijdskader. 'Subjective evidence' geeft ons een descriptief raamwerk om dergelijke historische episodes te kaderen, terwijl het idee van 'epistemic situation' ons de hulpmiddelen verstrekt om de prescriptieve vraag relatief aan een verscheidenheid van contexten op te werpen.

De werken van Victor Di Fate, Frederick Kronz en John Norton onderzoeken de rol van inductie in wetenschappelijke methodologie, een onderwerp dat niet ongerelateerd is aan een eigenlijke benadering van wetenschappelijk bewijs. James Woodward en Adam Goldstein behandelen vragen gerelateerd aan Achinstein's pragmatische theorie van verklaring. Gregory Morgan beschouwt Achinstein's interpretatie van William Whewell, en Michael Ruse plaatst Achinstein's vroege werk over analogieën in theoretische wetenschap. Een typerend kenmerk van Achinstein's wetenschapsfilosofie is het gebruik van de geschiedenis van de fysica om filosofische visies te illustreren en inspireren. Larry Laudan beschouwt Achinstein's gebruik van het 19de eeuwse golf-deeltjesdebat om de kracht van inductivisme en hypothetisch-deductivisme te achterhalen. Stathis Psillos en Bas van Fraassen benadrukken Jean Perrin's relatie tot het realisme-antirealismedebat en hoe Achinstein Perrin's

redeneren gebruikt om wetenschappelijk realisme te verdedigen. Jordi Cat beschouwt Achinstein's meest recente artikel dat zich toelegt op een interpretatie van James Clerk Maxwell's methodologie.

De titel van dit werk is geïnspireerd door Achinstein's poging om een wetenschapsfilosofie te construeren die nuttig zou kunnen zijn voor wetenschappers in het veld. Een terugkerende anekdote doorheen de papers is Achinstein's aanhef van zijn boek 'The Book of Evidence' waarin hij zijn aanvaring met de decaan van Johns Hopkins University beschrijft. De decaan daagt Achinstein, en menig ander wetenschapsfilosoof, uit eindelijk eens een wetenschapsfilosofie te produceren die van nut kan zijn voor wetenschappers. Of Achinstein dit ideaal al dan niet bereikt heeft doorheen zijn carrière laat ik open voor de lezer. Op zijn minst, dit doel voor ogen stellen is een bruikbare heuristiek bij het schrijven van goede wetenschapsfilosofie.

Peter Achinstein heeft, naar de woorden van de editor, "perfected the fine balance between stinging criticism of poor argumentation and cheerleading the pursuit of academic excellence", wat hem tot een uitstekende mentor en adviseur maakt. Als men één boodschap van zijn werk mag meenemen is het wel dat het steeds belangrijk is filosofische strengheid te combineren met historische gevoeligheid.

Dit boek is dan ook geen ordinair Festschrift, aangezien de bijdragen Achinstein's werk direct confronteren en hem dwingen om op nieuwe manieren over zijn visies na te denken, implicaties te zien die hij voordien niet inzag,e n compacte antwoorden te formuleren die de lezers in staat stellen zijn visies duidelijker te begriijpen en identificeren. Waar hij meer dan behoorlijk in geslaagd is.

In all his work, he
combines historical expertise with an unfailing sense for
when formal precision is in place and with a natural
approach to conceptual analysis.

11: book of evidence (2001): "Peter, you have never made a contribution of interest to scientists." (p.3) Achinstein acknowledges that the dean is not only rights that philosophical theories of evidence are ignored by scientists, but that they ought to be ignored, "because they propose concepts of evidence that are based on assumptions incompatible with ones scientists make when they speak of, and offer, evidence for hypotheses" (p.3)

44: victor di fate: One unmistakable characteristic of Peter Achinstein's work on scientific method is his emphasis that certain issues, often assumed by philosophers to be logical or conceptual matters, really have an empirical character. Achinstein is not alone, of course, in arguing that empirical information is indispensable for methodology, even among his contemporaries; but he does have a distinct point to make.
Rather, it is that scientists themselves contribute to the resolution of methodological issues through the empirical information obtained in the normal process of investigating our world, as well as by their own methods and procedures.

45: the title of this chapter refers to Achinstein's empiricism as Newtonian because, as will be argued here, there is good reason to think that Newton and Achinstein share strikingly similar views on the empirical character of certain issues in the epistemology of science, and on the contribution that scientists make in deciding them.

59-60: Doppelt: Achinstein's aim is to provide a conception of evidence that (1) captures the assumptions of scientists when they claim that certain facts e constitute evidence that a certain hypothesis h is true; and (2) can resolve scientific disagreements concerning what is evidence for what . I call this aim the test of relevance to scientist's inferential practices. Achinstein argues that standard philosophical conceptions of evidence – Bayesianism, hypothetico-deductive accounts, enumeratative induction views, inference-to-the-best-explanation, and others fail the relevance test. They fail for three reasons. First, these standard accounts are too weak to capture the conditions under which scientists take facts e to be evidence for some hypothesis h. Scientists do not take e to be evidence for h if and only if it satisfies the Bayesian condition of increasing the probability of h over its prior probability; similarly, they do not take e to be evidence for h if and only if h deductively implies e and e occurs, as the hypothetico-deductive account claims. The second failing of such standard accounts of evidence is that they offer a priori analysis of the notion, whereas an empirical approach is more promising. The third failing is that they fall short of the notion of objectivity operative in successful scientific inquiry. Achinstein has provided a demanding standard for evaluating his own account of evidence. I argue that his account falls short of meeting his "relevance test" and that a rival scientific realist account of evidence promises more success.

72: Goldstein: The nature of explanation is a central theme of Peter Achinstein's work, as indicated by his book of that title. He has consistently advanced a view that may be alternatively called the pragmatic view, the illocutionary theory, or contextualism about explanation. Moreover, he can lay title, I think, to the claim of having elaborated this view in greater detail, and with greater breadth, than any other philosopher. Bas van Fraassen might be considered by many to be a contender for this title – although according to Achisntein, van Fraassen is not even in the game: one of Achinstein's bolder claims is that van Fraassen's views about explanation, advanced as pragmatic, are not so at all.
Achinstein's pragmatism informs a pluralistic view: there are many kinds of good explanations, because success in explanation depends on features of the context in which the explanation is requested.

81: Goldstein: My defense of pragmatism about explanation is intended to challenge the claim that scientific explanations have a single aim – the central aim of universalism about explanation. I propose that an explanation aiming at promoting scientific advance is better, all other things being equal, than one that does not. I also propose that science aims at responding to deeper curiosities about human nature and our place – small though it may be – in the universe. I believe that these two aims of science are important enough, and that the case I present concerning alternative explanations of population-genetic change is compelling enough to establish that universalism about explanation is in error. Besides the intrinsic importance of this result, it plays an important role in the larger project of characterizing the explanatory strategies used by evolutionary biologists. The population-genetic models I offer as evidence can be interpreted along Hempelian lines, because, as I mentioned above, they are true empirical generalizations that can support counterfactuals, and so are laws of nature. In addition, I claim, there is an important role for historical explanations in evolutionary biology. Arguing for this claim takes direct aim at Hempelianism, because it aims to displace explanation-seeking why-questions as the sole kind of explanation-seeking questions asked by scientists. What remains is to describe these strategies of historical explanation by characterizing the kinds of explanation-seeking questions they are intended to answer, and the contexts in which they most naturally arise.

85: Kitcher: let the utilitarian dean speak: Peter, this is all very clever. You are very good at thinking up examples to show what is wrong with particular proposals, and you show that your own suggestion – that evidence requires a probability greater than half of an explanatory connection between hypothesis

and evidence – survives the various cases you have used to test other ideas. But how do we scientists figure out these probability judgments? Most of the time you deal with toy examples, in which the probabilities come for free; occasionally that's true in scientific research, but most of the time it isn't.

87: Kitcher: scientific rationality: my own view is that there are genuine difficulties in weighing and balancing evidence, that these are indeed prominent in the episodes studies by Kuhn, that they are not intractable, and that they occur in a variety of scientific contexts. I think the utilitarian dean would like philosophers to provide him with clear ways of sorting out these situations, and the contemplative dean would like some account of what goes on when scientists actually do sort them out.

88: Kitcher: Scientists have a conception of scientific responsibility that embodies: (R) A scientist, S, is responsible only if S's judgments accord with the evidence. What is it for a judgment to accord with the evidence? To a first approximation, we might say that there are three forms of judgment: one may accept a statement, reject it, or withhold assent; judgment accords with the evidence just in case one accepts those statements that are supported by the totality of available evidence, rejects those whose negations are supported by the totality of available evidence, and withholds assent in cases where neither the statement nor its negation is supported by the totality of available evidence.

96: Kronz: Following Newton and Mill, Peter Achinstein maintains in Evidence, Explanation, and Realism that there are universal rules of induction. He also maintains that such rules may be formalized, but that their validity cannot be determined formally, as in deduction. More precisely, he maintains that inductive inferences are warranted by material facts or empirical assumptions, in contrast with deductive inferences in logic and mathematics, which can be evaluated by formal means alone. These elements of Achinstein's view are provisionally accepted. Two additional components of his view, that induction and hypothetico-deduction are mutually exclusive and that induction is the core of the scientific method, are not. The corresponding themes defended here are that induction and hypothetico-deduction are mutually complementary, that one is not subsidiary to the other, and that the scientific context determines which of the two modes is most appropriate.

135: Mayo: Achinstein: the problem with philosophical accounts is (1) they are far too weak to give scientists what they want from evidence, and (2) they make the evidential relationship a priori whereas establishing claims of evidence requires empirical investigation. From this agreement it became clear that we share fundamental theses about evidence. As Achinstein has recently noted, we concur "that whether e, if true, is evidence that h, in the most important sense of 'evidence', is an objective fact, not a Subjective one of the sort many Bayesians have in mind. We agree further that it is an empirical fact, not an a priori one of the sort Carnap has in mind."

191: Richards: in his book of evidence, Achinstein lays out what would be required for a concept of evidence to be useful for scientists. First, it must be strong enough to warrant belief in a hypothesis – not merely raise its probability. Second, it must be empirical – not a priori, semantic, or merely mathematical. In other words, scientists assume that evidence provides a good reason to believe a hypothesis – not just some reason, and on the basis of the empirical facts, and not just logical or formal relations either.

Comments:
255: According to Achinstein (1984), an explanation-sentence is "strongly pragmatic" if (1) it contains terms that refer to an explainer or audience and (ii) the truth-value of the explanation-sentence can vary with the person giving or receiving the explanation.

259: Cartwright an I use somewhat different terminologies, but she is right in claiming that evidence (which she also calls "evidential relevance") requires correct explanation. Even if p(h/e) or p(e/h) are high, ore even if p(h/e) > p(h), this is not sufficient. On my own view, e is (veridical) evidence that h (the kind that scientists in general seek), given background information b, if and only if (1) p (there is an explanatory connection between h and e/e&b) > ½; (2) e, b, and h are all true; (3) e does not entail h; and (4) (in the strongest and most interesting type of veridical evidence) there is an explanatory connection between h and e. By an explanatory connection between h and e I mean that the fact that e is true correctly explains why h is true; or the fact that h is true correctly explains why e is true; or some hypothesis correctly explains why both e and h are true.

266: my aim is to define a concept of evidence that is "relevant" to scientists in the sense that it reflects what scientists actually seek when they seek evidence.

267: pragmatic account of explanation: on this view an explanation is something that provides an answer to certain types of questions – an answer that can be given in an act of explaining.

287-288: Richards: I his essay Richards concentrates on a very interesting but puzzling claim made by Darwin, namely, that although he (Darwin) at one point came to believe in branching evolution on the basis of the taxonomic facts, and he believed that he was justified in doing so, he also believed that these same taxonomic facts were not sufficient for other scientists to believe in branching evolution. B the latter, according to Richards, Darwin means not that these taxonomic facts would fail to persuade others to believe, but that others would not be justified in so believing. =Richards rightfully finds this puzzling as asks how it can be explained. Using my concept o ES (Epistemic Situation) evidence, he claims that what Darwin was saying is that relative to the sort of epistemic situation he (Darwin) was in, the taxonomic facts did provide ES-evidence; but others were in a different sort of epistemic situation, relative to which the taxonomic facts did not provide such evidence.

288: More frequently they (historians of science) are concerned with simply identifying the reasons scientists in fact had for believing what they did, whether these reasons are epistemic or non-epistemic (the latter including causal factors influencing those beliefs). But evaluating those beliefs, particularly in the way Richards suggests, is a much more difficult task. It involves not only
(1) discovering what reason e the scientist had for believing h,
But also
(2) identifying what particular epistemic situation the scientist was in (which, among other things, involves identifying what beliefs other than e and h he had or was in a position to have),
And
(3) determining whether anyone in that epistemic situation would have been justified in believing h for the reason e

297: woodward: I will raise just one question. Can't there be "unstable" causes? Woodward's "stability" condition is concerned with the extent to which the relationship between cause C and effect E will continue to hold as various background factors change, where a background factor is any factor distinct from C and E. Now think of those scary warnings on prescription blurbs for pills, for example, "this pill can cause stomach upset." Suppose that the pill caused John's stomach upset yesterday, but not today when he took another one. (We rule out other causes one by one.) C can cause E (the blurb tells us, although this is rare), and yesterday it did in John's case, but there isn't much "stability" here, since if background conditions distinct from C and E change, the causal connection will probably not exist. To be

sure, Woodward says that “stability” is not a necessary condition for the selection of a cause, only a “relevant” one. Whether this is so, and also what exactly is supposed to count as “stable”, are questions I will leave for Jim for another occasion. But I do welcome his pragmatism in these matters.

Elliott Sober: “historians and sociologists study science as it is, whereas philosophers of science study science as it ought to be. Philosophy of science is a normative discipline.” (2008, XV). (‘Evidence and Evolution: the logic behind the science’ Cambridge University Press: Cambridge University). Elliott Sober claims that the philosophy of science is a normative discipline, telling us what science should be like. That may be true, but that fact does not require that philosophy of science ignore the descriptive and contextual. An adequate philosophy of science must be able to help us understand the complexities of the history of science, as scientists came to believe their hypotheses on the basis of empirical facts, and in light of what they were in a position to know or believe.