

*Law and Explanation: an essay in the philosophy of science.*

By PETER ACHINSTEIN. Oxford: Clarendon Press, 1971.

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Law and explanation bulk large in science as philosophers have until very recently conceived it. Nor are these yet exhausted topics, even if theories now bulk larger still. The grand, if erratic, succession of theories in the history of science is perhaps claiming too much professional attention at present. Some philosophers of science, one feels, cannot see the trees for the wood. Yet the prime interest of scientists who plant and grow a theory is in its foliage of laws and explanations rather than in its place in the historical plantation.

Professor Achinstein can see the trees all right; his detailed interest in them is refreshing and I wish I could give his book a warmer welcome than I can. But my complaints all stem, I think, from a temperamental aversion to conceptual botanising of the sort he undeniably does well, and those who do not share it will profit far more from the book than I have been able to do. But it is anyway as well for those of us who tend to hack our way rather impatiently through the jungle of usage (to confuse the metaphor!) to be reminded how crooked are the natural paths we prefer to try and make straight.

Achinstein starts by discussing laws, generalisations and regularities. Laws for him are, or describe, not any regularities but only those which 'underlie others for which they provide a physical analysis and explanation' (p. 18). This restriction certainly cuts out many Hempelian 'laws' which would not be so called by scientists themselves (which is part of Achinstein's object). It is intuitively plausible enough, but not obviously clarifying without more account of 'physical analysis', 'regularity' and 'explanation'. Explanation comes later, but physical analysis not at all, and it is hardly the most pellucid notion. I have moreover immediate qualms about Achinstein's definition of the regularities laws describe (p. 4): They consist, he says, 'in the fact that (1) under certain conditions there is always certain behaviour or the occurrence of certain properties, and (2) this behaviour or these properties are exhibited by all objects of certain types'. This *prima facie* excludes (i) unconditional, (ii) statistical and (iii) relational regularities, not to mention cases where not all objects of the type concerned get into the 'certain conditions' (as with wires, of which Hooke's Law is true, that are never stretched). Similarly, Achinstein's early dismissal of generalisations rests on his peremptory definition of them as 'arrived at . . . by generalising from instances'. Now that hardly fits common parlance, in which one can surely recognise a generalisation without delving into the biographies of those who have 'arrived at' them (and perhaps rejected?—I am not clear if the definition admits of generalisations being believed to be false). And since men arrive at generalities (shall we say) in diverse ways, what is a generalisation for one man need not be so for another, it would seem. No wonder Achinstein finds little use for the concept; but he cannot make that a complaint against those who use it in a less Pickwickian sense.

Achinstein indeed admits generality to be 'of the essence of a law' in

his second chapter and is there at pains to define it. He resists the formalism that takes laws to be about everything, and the paradoxes (e.g. of confirmation) which result from that assumption. He distinguishes the 'subject of a law' as (p. 19) 'whatever objects or substances it is or is supposed to be (a) that could be said to exhibit a regularity described by the law, (b) that would be investigated in testing the law, and (c) to which the regularity noted in (a) could reasonably be attributed as a property or characteristic'. Again it must be said that this account buys its plausibility at the price of unclarity. Conditions (b) and (c) presuppose, rather than provide, solutions to confirmation paradoxes and category disputes. Achinstein indeed gives clear examples of what specific laws are and are not about, but that does not seem to me the point. (I am with Dr. Watson's complaint to Sherlock Holmes that a fresh illustration of a puzzling inference is not an explanation of it!) No one supposes scientists to be often in practical doubt as to the subject of a law. The problem is to spell out what that is, and it is not help enough to me to be told that it depends on context and is relative to a science or field.

Anyway, given the concept of subjects for laws, Achinstein produces some important criteria of generality. Apart for the obvious syntactical ones, he makes (pp. 26–27) laws hold of every 'sample or instance' of their subjects and it *not* be a sufficient condition of their truth that all such samples as 'do now, will later, or once did satisfy the antecedent condition' also then satisfy the consequent one. But what else must then be true of the subjects of a law? No doubt that if they *were* to satisfy its antecedent they *would* satisfy its consequent. And what are the truth conditions for that? Hardly a new problem, but Achinstein rests content without either giving or referring us to a solution or showing that, despite all appearances, he does not need one.

He does indeed discuss counterfactuals in his next chapter, on the necessity of laws. But his discussion there simply takes this problematic ability of laws to support counterfactuals as one source of their supposed necessity (p. 51). Another source may be 'the fact that it has strong support', and the third 'the fact that it is analytic (if it is)' (p. 58). I do not see how Achinstein can find such characteristics of laws as their supporting counterfactuals, having strong support and being analytic to be so much clearer than their necessity as, without more ado (or at least reference to more ado elsewhere), to provide a satisfactory explication of it.

Next explanation, which Achinstein explains in terms of conveying understanding, which in turn he understands in terms of knowing answers (to the question whose *oratio obliqua* form has called for explanation) that are more correct, accurate, deep, complete and unifying (p. 72). The connections of explanation with these latter notions are urged with Achinstein's usual deft use of examples, but again I find the explicata no less in need of explication than the explicandum. And there are a couple of arguments in the chapter I could not follow at all. He discusses for example two conditions for a person *not* understanding something, which Bromberger has proposed as singly sufficient and disjunctively necessary. He cites a case (where someone *knows* whether Concord is the capital of New Hampshire) which satisfies neither condition and yet seems not to

be a case of *understanding* (whether Concord is etc.). So far, so good; what I could not fathom is the rationale of his following remark (p. 70): 'What Bromberger says appears to disallow speaking of understanding in such a case, *since* he claims that we cannot say that someone does not understand this' (my italics). Now this obviously *is not* a case of understanding, but how that is supposed to *follow* from it not being a case of not understanding escapes me.

However, Achinstein's main use of his definition of explanation is in his subsequent enquiry into the part laws can play in it. He gives a couple of very well laid out examples and shows how various features of laws can help to provide the more correct, accurate, deep (etc.) answers required of good explanations. But 'regarding the *need* (my italics) for laws in explanations, the most that can be said', he thinks (p. 97), 'is this. Given an explanation which does not explicitly invoke a law, if all the assumptions were themselves to be explained . . . at some point it is very likely that laws would be invoked.' This of course is in sharp contrast with the deductive-nomological (D-N) model of explanation, which Achinstein here attacks. He makes *inter alia* the common mistake of reading proposed necessary conditions of explanation as if they were supposed to be sufficient, hence producing as counter-examples cases that are completely irrelevant (p. 104). Of his other arguments I have space merely to indicate some of the points on which I should take them up; there is nothing I can see that is not met in Hempel's further discussion in his *Aspects of Scientific Explanation*. On page 101 Achinstein tacitly equates D-N with causal explanation, which is merely a special case of it. On page 105, to say that Hempel's deductive completion of a weak statistical 'explanation' would be of something else does not begin (even were it true) to show that the original really *is* an explanation. The event referred to in it may really be the *cause* of the event it purports to explain, if suitable deductive completion merely characterises the same event more fully (as opposed, e.g. to requiring the presence also of other events), but that is quite a different matter. My thumping the radio may well be the cause of its starting again, but that description of what I have done is not thereby made an explanation. And the argument on page 107, against reference to laws being implied by the tacit universalisability of explanations, rests essentially on treating typical causal explanation (e.g.) as stating necessary rather than sufficient conditions.

But the main point, underlying indeed the method of the whole book, is that Achinstein is not as prepared as the D-N theorists certainly are to deviate sharply from scientists' use of 'law'. On their, and Achinstein's, usage it is certainly true that explanation need not involve laws. But so what? 'Law' is to my mind essentially a term of philosophical art. It is applied to, not within, the working vocabulary of science. To vary its scientific use for philosophical purposes is not to misrepresent science as it would so to vary the use of 'electron' or 'cell'. Scientists rightly have only a spasmodic interest in the philosophy of science, nor are they the ultimate authority on it. In conceptual as well as ecological matters it is reasonable to hold that science is too important to be left entirely to scientists. Achinstein has not satisfied me that much more than historical

significance attaches to what they have and have not called 'law' down the centuries. Hempel and company deliberately extend the use of the term in the interests of stating a theory of explanation, prediction and confirmation. Their right, as philosophers, to do this seems to me the same as that of chemists to extend the use of the term 'element'. Some continuity with previous use is naturally called for; but the justification for the extension is the success of the theory which ascribes an essentially relevant similarity to objects of the old and new kinds. And so it is with laws. On the D-N theory, many more general statements share the crucial role in explanation that makes what scientists have on the whole called 'law' important. Their essential similarity in this respect warrants a common classification, and the title 'law' is very apt for the class. Now since Achinstein does not accept the theory, he has of course no call to accept the usage that goes with it. But that scientists' usage is not that of the theory in itself no more counts against it than chlorine being unlike earth, air, fire and water counts against the chemical theory that directs us to call chlorine an 'element'.

I am relieved to be able to agree with more of Achinstein's views in the last two chapters, on various modes of reasoning and their use in arriving at laws. He distinguishes explanatory, inductive, analogical, analogical-explanatory and deductive inferences; and uses examples that show clearly how each has been used both in arriving at and in justifying laws. He argues against an emphatic distinction between the contexts of justification and discovery, and the related Popperian claim that all reputable inference in science is deductive. I have no doubt that Achinstein is right on both counts, and that it is a legitimate task for philosophy to explicate the rules of major kinds of non-deductive inference that crop up in scientific practice. My only complaint here is that Achinstein seems unaware of some of the main sources of data for this task. For example, the discussion from page 119 of 'inference to the best explanation' would have gained greatly in realism by reference to statistical techniques for doing just that. And the same reference would almost certainly have prevented the very implausible conflation on page 122 of 'explanatory power' with 'probability in the light of the evidence'. Similarly, the reference on page 126, to what alternatives it is 'reasonable' to consider when inferring that all *F*'s and *G* from all examined *F*'s being *G*, would be a good deal more convincing if it were backed up with a solution of Goodman's notorious 'grue' problems. But, incredibly, neither Goodman himself nor any of the many others who have tackled this topic are even mentioned in this connection.

I regret having had to cavil at so much of this book. It must be said that it is a cut above the many sub-Hempel texts that have appeared on these topics in the last few years. I shall refer to it constantly for its sensitive use of a wide and well chosen range of examples. And any adherent of a more or less Hempelian position should satisfy himself that he has answers to the theses here presented. Others are in any case likely to find the work more congenial.