DISCUSSION

LAUDAN'S RADICAL ACCOUNT OF ADHOCNESS

Brian Baigrie
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In Progress and its Problems (1977), Larry Laudan offers a radical account of adhocness which he regards as one of the more important aspects of his book. It is curious that none of his reviewers has focused upon it, particularly in view of the extensive treatment adhocness has been given by Popper, Lakatos, and others.[1] This is an oversight I plan to rectify in this discussion note. As for Laudan, he makes two related claims: a) that his approach separates spurious from legitimate senses of adhocness; and b) that the concept of adhocness itself does not add anything to his analytic machinery for the appraisal of scientific theories—in other words, it is "just a special case of conceptual problem generation" (1977, p. 118). One wonders, then, why adhocness is not treated in chapter two of Laudan's book, titled "Conceptual Problems", and is included as part of the discussion in chapter four where he offers strategies for the evaluation of research traditions? The answer to this question is to be discerned in the relationship between these two claims. It is this: the concept of adhocness does not add to Laudan's analytic machinery if—and only if—he has managed to distinguish a pejorative sense of the term. However, if he has not, the concept detracts from his analytic machinery—i.e. the notion of problem-solving effectiveness—

in a way which is not immediately apparent, and explains why this subject is given special consideration by Laudan. In this note, I will propose an empirical test which discloses that his conceptual reading of adhocness is <u>without applications</u>—i.e. it cannot be applied to historical episodes. This diminishes the value of Laudan's theory of science for a number of reasons; notably, without a theory of adhocness, he cannot specify what is <u>novel</u> about conceptual change in science. My concern here, though, is Laudan's notion of problem-solving effectiveness: the consequence of his failure to specify a pejorative sense of adhocness is that <u>in principle</u> there are no limitations on the way a problem can be solved by the proponents of a research tradition, thus effectively emptying the class of anomalous problems. If this is so, his analytic machinery is sterile as a tool of theory appraisal.

"Science", claims Laudan, "is essentially a problem-solving activity" (1977, p. 11). "<u>The central cognitive test of any theory involves assessing its adequacy as a solution of certain empirical and conceptual problems</u>" (1977, p. 70). Empirical problems are not systemic—they are not about our system of theories, but about the natural world. "If a sound justification for most scientific activity is going to be found", says Laudan, "it will eventually come perhaps from a recognition that man's sense of curiosity about the world and himself is every bit as compelling as his need for clothing and food"

(1977, p. 225). Man is a curious animal, and so Laudan defines empirical problems as "substantive questions about the objects which constitute the domain of any given science" (1977, p. 15). Conceptual problems, on the other hand, refer to the well-foundedness of the conceptual structure of a theory which was originally devised in order to answer empirical questions. The progressiveness of scientific theories is determined by a relation between these two kinds of problems—viz. a successful theory will "maximize the scope of solved empirical problems, while minimizing the scope of anomalous and conceptual problems" (1977, p. 66). The notion of problem-solving effectiveness, then, is not unlike an accountant's ledger—theories or research traditions have assets and debits; their progressiveness is determined by the balance between the two.

In our (1981), J.N. Hattiangadi and myself argued that Laudan places "no prohibitions upon the way in which a solution is engineered, not even as concerns the employment of ad hoc manoeuvres....But if this is so, the class of anomalous problems—or those unsolved problems which are solved by a competitor—would seem to be empty" (1981, p. 115). Actually Laudan does place one prohibition upon the way problems are solved: his counter-intuitive thesis is that "ad hoc modifications, by their very definition, are empirically progressive" (1977, p. 115)—in short, that adhocness must increase rather than decrease the empirical problem-solving capacity of a theory—and

thus he rejects obvious and trivial manoeuvres which we might call ad hoc, such as arbitrarily restricting boundary conditions, redefining correspondence rules, and so forth (1977, p. 116). These aside, the upshot of his thesis is that there is nothing wrong with the desire to remove an anomaly, even when a change in theory does not conform to the expectations imposed by Popper and others (Cf. Popper 1965, Lakatos 1970). When we wrote our review, our thoughts were that a hypothesis which solves some new problems as well as the old ones is easier to arrive at than a hypothesis which does not. Put otherwise, we supported the conventional wisdom which has it that scientists can always come up with an ad hoc manoeuvre, and that this presumably is what makes them unacceptable. This would detract from Laudan's assertion that ad hoc modifications are progressive on two counts: 1) anomalies traditionally have been regarded as difficulties that repeatedly resist resolution. His assertion not only seems to trivialize this feature of problems, it also can be read as a recommendation to scientists to opt for the easy way out of a tight spot; and 2) if adhocness is permitted, scientists would experience little or no difficulty in solving problems, thus emptying the class of anomalous problems. This, of course, would diminish the value of Laudan's calculus because it would not apply to a number of similar cases. For example, an instance where we might want to employ a methodology like Laudan's occurs where two theories are competing in

the same scientific domain, say, quantum mechanics. But it is not unlikely that theories in the same domain will generate the same conceptual problems, especially those of an external kind. If the class of anomalous problems is empty, then one side of Laudan's ledger will be blank because in such instances the conceptual problems generated will cancel out one another. In this event, it would be extremely difficult to determine which of the rival theories is more progressive. In fact, the only unit of appraisal left to us would be the solved empirical problem, and this is a grey area which would be of little help. Not only would there be a debate between the proponents of both sides as to which solves more empirical problems, no doubt each would be encouraged to over-estimate their positive successes.

This criticism, however, stands or falls on a quibble about whether adhocness is easy or not. In any case, it does not address Laudan's central thesis which is that there is nothing objectionable about the desire to remove an anomaly. Let us assume for the sake of argument that his intuition is sound. Even so, there is something objectionable about his thesis. It is this: Laudan's claim is that conceptual problems have long been overlooked by philosophers of science, and his rule of thumb is that <u>they diminish a theory's value</u>. He does not tell us why this is so, despite historical examples to the contrary. Darwin's evolutionary theory is perhaps the most striking. Not

only was it conceptually vague, in addition it clashed with the accepted theories of the day, scientific and otherwise, thus occasioning more resistance than any other scientific view, save Copernicus'. Yet despite this—or, perhaps, because of this—we view it in retrospect as a giant step forward for mankind. Among philosophers of science, Feyerabend (1975) alone has grasped the significance of conflict as an ingredient in scientific growth, and advocated the proliferation of views which are inconsistent with the status quo.

This objection aside, Laudan's disavowal of conceptual problems is a consequence of his general thesis that science is a success-oriented activity. Let me explain. If the aim of science is to solve problems (and the more problems the better), then what we commonly regard as adhocness must be a cognitive virtue rather than a vice. Ad hoc manoeuvres, Laudan insists, do indeed solve problems. To maintain that every modification of a theory should immediately solve some new problems as well as the old ones is "to repudiate the doctrine that theories which solve more problems about the world are preferable to those which solve fewer" (1977, p. 115). By the same token, though, conceptual problems are undesirable because <u>they reduce a theory's overall problem-solving effectiveness</u>. On this basis, Laudan proposes his radical notion of adhocness: "<u>the only legitimately pejorative sense of 'adhocness' reduces to a situation in which a theory's overall problem-solving</u>

<u>decreases</u>, by virtue of its increasing conceptual difficulties" (1977, p. 117). This sort of thing, he contends, is common in science and frequently cited as grounds for rejecting a theory. Regrettably, Laudan does not provide us with a single illustration. My contention is that while conceptual problem generation may be cited as grounds for rejecting a theory (certainly Darwin's theory was rejected in many quarters for this reason), this should not be confused with Laudan's proposal. His thesis is that adhocness reduces to cases where a theory's overall problem-solving capacity decreases—i.e. where a theory generates more conceptual problems than solved empirical problems. It follows from this that if theories are rejected in accord with Laudan's proposal that scientists keep books in the way he does. The history of science indicates otherwise. More to the point, Laudan's very characterization of empirical problems makes such book-keeping impossible.

To demonstrate my claim, I propose the following test for Laudan's thesis: he should be able to furnish a case study which shows that a theory's conceptual losses outweigh its empirical gains—in other words, that its debits are greater than its assets. But how can he do this? To do so assumes that problems are countable. But how many empirical problems does a scientific theory solve? How many questions did Newton's theory answer? Well, for every statement that can be deduced from Newton's theory we can formulate the question "why is it

so?" which the theory explains.[2] Accordingly, Newton's theory can be construed as having answered an infinite number of questions. Laudan claims that scientific theories only solve a finite number of problems (1977, p. 228, fn. 4), but for all he says it is difficult to see how this is so. The upshot of this is that <u>no matter how many conceptual problems a theory generates</u>, <u>its proponents can always claim that it solves more empirical problems</u>. In other words, a change in theory can never be shown to produce more conceptual problems than solved empirical problems because its advocates can always assert that it solves any number of empirical problems. Consequently, we can never say that a change in theory is ad hoc.

The difficulty here concerns the tension between Laudan's uncritical acceptance of the view that problems are questions and his conceptual interpretation of adhocness. He cannot have it both ways: either he must relinquish his characterization of empirical problems as "substantive questions" or abandon his view of adhocness. If he relinquishes the latter, though, his theory of science will not be able to impose any restrictions upon the way in which a problem is solved. Therefore, it need not concern us whether or not ad hoc manoeuvres historically have been an easy matter. Without any limitations, scientists will be able to solve empirical problems any way they can, even if the solution they pursue invokes an entity as implausible as an evil demon. If this seems absurd, it simply reinforces the

need for a theory of adhocness, one which tells us why certain hypotheses are relevant to a given problem-situation and others are not. Lacking such a theory, Laudan's analytic machinery is useless for all practical purposes because the class of anomalous problems is empty. It need not concern us, though. While the class ~~of conceptual problems provides a glimmer of hope that we~~ can yet assign a number to the right-hand side of Laudan's ledger, so long as he characterizes empirical problems as questions, we cannot assign a value to the left-hand side. Of course, one option remains for Laudan—namely, he can give up his view of empirical problems. In so doing, however, he would relinquish the central tenet of Progress and its Problems. Whatever his response, therefore, his theory of science is problematic.

Footnotes

1 As a partial listing of the reviews of _Progress and its Problems_, see Baigrie and Hattiangadi (1981), E. McMullin (1979), and the symposium in _Philosophy of the Social Sciences_ (1980).

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