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Laudan and the Problem-Solving Approach to Scientific Progress and Rationality

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I

Laudan's object in *Progress and Its Problems*[1] is 'to trace out the consequences of the view that science fundamentally aims at the solution of problems' (pp. 4-5). He does not deny that science can be legitimately considered as something other than a problem-solving enterprise; his view is rather that considering it as 'a problem-solving system holds out more hope of our capturing what is most characteristic about science than any alternative framework' (p. 12). More specifically, Laudan's position is that scientific progress consists in developing theories of increasing problem-solving effectiveness and 'rationality... in making the most progressive theory choices' (p. 6).

As Laudan emphasizes at the beginning of his discussion, the idea that science is essentially a problem-solving activity is not new: it has been discussed in some detail, most notably by Kuhn and Popper (cf. p. 11 and footnote 1, p. 228).[2] It is, therefore, natural to ask whether Laudan's view is similar to the view considered (and rejected) by Kuhn—that a theory is acceptable just in case it has more problem-solving effectiveness than any of its competitors—or to the view urged by Popper—that a theory is acceptable just in case it effectively solves pre-existing problems (or more effectively solves pre-existing problems if there is more than one theory in the offing).[3] In other words, does Laudan hold the view that theory appraisal involves comparing specified theories in terms of their total problem-solving effectiveness, or the view that it involves evaluating how effectively a specified theory or specified theories solve certain specified problems?

It might be thought that this question should be easy to answer. However this is not so, because Laudan appears to hold both views. On the one hand, he frequently suggests that all we have to take into account when evaluating

1 L. Laudan, *Progress and Its Problems*, Berkeley 1977. All page references in the text are to this book.

2 It is not obvious that all the claims that Laudan makes for the novelty of his approach can be sustained. In particular, the distance Laudan attempts to put between his view that science fundamentally aims to solve problems and views according to which it aims to explain facts (cf. pp. 22-26) appears to rest on an implausible view of what explanation is. Moreover, it is not clear that Popper's 'overtures to problems are only rhetorical' (p. 228, footnote 1).

3 For references to Kuhn's and Popper's discussions of problem-solving see below. Although Laudan does not provide us with a precise account of theory acceptance, what he seems to have in mind is the idea that acceptable theories are theories which are reliable, credible, trustworthy, etc., i.e. Lakatos' 'acceptability$_3$'. Cf. I. Lakatos, 'Changes in the Problem of Inductive Logic' in I. Lakatos (ed.), *The Problem of Inductive Logic*, Amsterdam 1968, pp. 390-405.

theories is their relative problem-solving effectiveness. Thus, for instance, we find him saying that 'of any and every theory, we must ask how many problems it has solved and how many anomalies confront it' (p. 18); that 'the problem-solving effectiveness of a theory depends on the balance it strikes between its solved problems and its unresolved problems' (p. 67); and that 'what is crucial in any cognitive assessment of a theory is how it fares with respect to its competitors' (p. 71). On the other hand, Laudan also discusses theory evaluation in a way that strongly suggests that it must make essential reference to a set of pre-existing problems. For instance, he remarks that 'scientific theories are usually attempts to solve specific empirical problems about the world' (p. 11); that 'problems of all sorts... arise within a certain context of inquiry' (p. 15); and that 'one of the hallmarks of scientific progress is the transformation of anomalous and unsolved problems into solved ones' (p. 18).

Although we can find a place within either approach for much of what Laudan has to say about the nature of problems, their sources, their relative importance, etc., it is crucial that they be distinguished. For as we shall see, they are far from being equally plausible.

II

To open the discussion, let us look at the view that we should accept theories which have greater problem-solving effectiveness than any of their competitors, and ask how Laudan characterizes problem-solving effectiveness.

According to Laudan, we must certainly consider the relative number and the relative importance of the empirical problems that each theory solves. (Problems are said to be empirical if 'they are substantive questions about the objects which constitute the domain of any given science' [p. 15].) But not only this: we must also consider two other kinds of problems: anomalous problems, i.e. 'empirical problems which a *particular* theory has not solved but which one or more of its competitors have' (p. 17); and conceptual problems, i.e. problems which arise from 'conceptual ambiguity or circularity' within a theory, from internal inconsistencies, or as a result of conflict with other theories or doctrines believed to be well-founded by proponents of the theory in question (p. 49).[4] What we need not consider, Laudan argues, are those problems which none of the competing theories solve: this is because 'the only reliable guide to the problems relevant to a particular theory is an examination of the problems which predecessor—and competing—theories in that domain (including the theory itself) have already solved' (p. 21).

This view is more sophisticated than the one considered and rejected by Kuhn: it recognizes a role for anomalous and conceptual problems and it takes

4 Although many philosophers of science have recognized that conceptual considerations play a role in scientific theorizing—cf. Hempel's remarks about 'theoretical support' in his *Philosophy of Natural Science*, New York 1966, chapter 4; Popper's view in *Conjectures and Refutations*, New York 1962, p. 241, that new theories should, if possible, resolve 'theoretical difficulties'; and Kuhn's view in (e.g.) his 'Logic of Discovery or Psychology of Research' in I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, London 1970, p. 21, that 'connectedness with other theories' is an important scientific value—it is not implausible to claim, as Laudan does, that 'no major contemporary philosophy of science allows for the weighty role which conceptual problems have played in the history of science' (p. 66).

In his discussion of conceptual problems, Laudan does not exhaust the possibilities. Certainly, we should also take into account any ambiguity, circularity, etc. that arises when one theory is taken in conjunction with others.

into account that some problems are more important than others. Moreover, once these additional factors are recognized Kuhn's objection to the problem-solving approach appears far less convincing. We can agree that arguments 'based upon the competitor's comparative ability to solve problems (need be) neither individually nor collectively compelling' and that 'there are losses as well as gains in scientific revolutions' without giving up the problem-solving approach as Laudan conceives it.[5] Since, according to Laudan, what counts when theories are being compared is the balance they strike between the problems they solve and the problems they fail to solve, both weighted according to their relative importance, 'the growth of knowledge can be progressive even when we lose the capacity to solve certain problems' (p. 150).

This point notwithstanding, the idea that theories can be compared in terms of their problem-solving effectiveness is open to a number of serious objections:

(1) As Laudan recognizes, the theory as sketched above does not exclude the possibility of our 'trivially and mechanically generat(ing) conceptual problems for any theory simply by conjoining it arbitrarily with any "wild" belief we like' (p. 55). The difficulty here is that it is far from clear how we can solve this problem without substantially modifying the view about theory appraisal presently under consideration. Certainly, Laudan's own response—that the only sorts of beliefs that can generate 'external conceptual problems' are those associated with scientific theories, methodological theories, or prevalent world views' (ibid.)—is inadequate: there is, of course, no lack of 'wild' scientific and methodological theories to which we can appeal.

(2) More important is the objection that we cannot appraise theories in terms of their problem-solving capacity because what counts as a problem (to say nothing about what counts as an important problem) is theory-dependent.[6] For Newtonians, but not for Einsteinians, there is the problem of specifying the mechanism by means of which light is transmitted in the ether; for phlogiston theorists, but not for oxygen theorists, explaining why metals are so much alike was an important problem.[7] To avoid this difficulty it is not sufficient to argue that competing theories should be evaluated with reference to the problems they have in common: the crucial problems may be just those that some of the theories fail to address. Nor can we argue that theories can always be evaluated in their own terms: that a theory fares better than its competitors when evaluated with respect to what it takes to be important problems is no guarantee that it will also fare better when evaluated from the standpoint of theories with which it competes. Nor, finally, is it plausible to hold, e.g., that Einstein solved the

5 T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago 1970, second edition, p. 155 and p. 167. Cf. also ibid., p. 169. In this context it is worth noting that Kuhn's 'own impression... is that a scientific community will seldom or never embrace a new theory unless it solves all or almost all the quantitative numerical puzzles that have been treated by its predecessor'. Kuhn, 'Logic of Discovery or Psychology of Research' (note 4), p. 20.

6 In this and the following section I consider a truncated version of Laudan's view. As we shall see, Laudan treats research traditions as the fundamental unit of appraisal and handles theories derivatively. This need not concern us here, however, because similar objections can be raised against the complete version of the theory. See section IV. With respect to the relevance of the present difficulty for the complete theory, see *Progress and Its Problems*, p. 93.

7 For the first of these difficulties, see A. Grünbaum, 'Can a Theory Answer more Questions than One of its Rivals?', *British Journal for the Philosophy of Science*, **27**, 1976, 21; for the second difficulty, see Kuhn, *The Structure of Scientific Revolutions* (note 5), p. 148.

problem of how light is transmitted in the ether by showing it to be a pseudo-problem; if we allow problem solution by dissolution, it is difficult to see how we can avoid opening the way for the elimination of all embarrassments as pseudo-problems.[8]

(3) Finally, notice that comparative theory appraisal of the sort under consideration fails to recognize that there are occasions when it is unreasonable to accept any theory from among a group of competing theories. It is not true that the only thing that matters is 'how (a theory's) effectiveness or progressiveness compares with its competitors' (p. 120). Indeed, if we hold, as Laudan does, that to accept a theory is 'to treat it as true' (p. 108), acting in accordance with the present theory may lead to risky, even dangerous, behaviour.[9]

If correct, these observations show that we would be well advised to focus on the view that theories should always be evaluated with respect to a pre-existing set of problems. What we would now like to know is what Laudan's view looks like when reconstructed this way.

III

According to the view I now wish to examine, a theory is accepted into the body of science not because it is a better problem-solver than it competitors, but because it brings about an improvement in a pre-existing problem situation.[10] More specifically, the idea is that problems always arise against a background of relatively unproblematic belief, comprising well-established data, auxiliary hypotheses, theories, and methodology: a theory is acceptable just in case, given the background knowledge which constitutes the problem situation, the number of significant pre-existing empirical problems the theory solves more than compensates for the number of significant empirical and conceptual problems it generates.[11] (In the case of competing theories, each of which would alone be

8 The idea that theories should be compared with respect to the problems they have in common and the idea that they should be compared from their own standpoints are mentioned by Laudan in *Progress and Its Problems*, pp. 144-46 and in 'Two Dogmas of Methodology', *Philosophy of Science*, 43, 1976, 585-97. Here, however, Laudan's point is simply that non-cumulativity and incommensurability do not rule out the *possibility* of theory comparison. The third suggestion—that the difficulty raised can be handled by invoking the idea of a pseudo-problem—can be gleaned from *Progress and Its Problems*, pp. 35-36 and 'Two Dogmas of Methodology', p. 589.

9 Another difficulty is that it is by no means easy to evaluate the number and importance of the problems solved and generated (e.g.) by Newton's and Einstein's theories, let alone the problems solved and generated by science and Zen Buddhism. (Laudan suggests that such appraisals are possible on p. 2.)

10 For a clear statement of the view that theories should be appraised with respect to a pre-existing set of problems, see K. R. Popper, *Objective Knowledge*, Oxford 1972, p. 143.

11 Among the sorts of problems that might confront a scientist is the problem of explaining why a certain phenomenon occurs, the problem of developing auxiliary theories to link established theories with established data, the problem of developing a theory which reproduces the successes of a refuted theory without also reproducing its failures, the problem of unifying two areas of investigation, and the problem of replacing a theory beset by conceptual or methodological difficulties.

For a useful discussion of background knowledge see Popper, *Conjectures and Refutations* (note 4), p. 238. The notion of background knowledge being appealed to here differs from Lakatos' notion of a background or 'touchstone' theory. Cf. his 'Changes in the Problem of Inductive Logic' (note 3), p. 375, footnote 2. For further acceptable, we say that a scientist should accept the theory which strikes the most favourable balance between the problems it solves and those that it generates.)

The first thing to note concerning this version of the problem-solving approach is that it does not succumb to the objections just raised against the other version of the approach. (1) There is no question of generating conceptual problems for a theory by linking it with 'wild' beliefs, since the only beliefs that can generate external conceptual problems, according to the present view, are those occurring in what is taken to be unproblematic background knowledge. (2) What counts as a problem, as well as the importance of problems, is determined in advance by the problem situation. And (3) we are not always obliged to accept some theory or another, it being quite possible that none of the theories we are considering effects a favourable balance between the problems it solves and those it generates.

Second, it is important to notice that the present view is compatible with what Laudan calls 'non-cumulative progress' (p. 147). It is not denied that the solution of a problem may involve rejecting previously accepted data, auxiliary hypotheses, theories, and methodological principles. By solving the problem raised by the fact that 'Maxwell's electrodynamics ... when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena'[12] without generating excessively many new problems, Einstein rendered Newton's theory (theoretically, if not practically) obsolete. In a similar way, Kekulé's solution of the problem of determining the correct formula for benzene involved no less than the rejection of a certain view about chemistry, namely that it should concern itself with 'transformation' rather than 'constitutional' formulas.[13]

Third, I should perhaps point out that it is no part of the view under discussion that problem-solving is all that goes on in science—it is not denied that problem-finding, testing, the clarification and the development of theories, etc. are all important scientific activities. Nor is it part of the view that theorizing always occurs in response to problems—it is not denied that theories may result from curiosity, playfulness, and the like. Nor, finally, is it part of the view that the only solved problems which count when a theory is being appraised are those the theory was specifically designed to solve—it is not denied that a theory's solving a problem it was not specifically designed to solve may count heavily in its favour.

Finally, it should be noted that the present account makes contact with an important aspect of Laudan's discussion. Unlike the account just considered, it recognizes that 'the kinds of things which count as empirical problems, the sorts of objections that are recognized as conceptual problems, the criteria of intelligibility, the standards of experimental control, the importance or weight assigned to problems, are all a function of the methodological-normative beliefs of a particular community of thinkers' (p. 130).

These last observations do not, of course, constitute anything like a defence of the view that theory appraisal must always make reference to a pre-existing

considerations pertaining to the notion of background knowledge see A. Musgrave, 'Logical versus Historical Theories of Confirmation', *British Journal for the Philosophy of Science*, 25, 1974, 1-23. It is my view that Musgrave's criticism of the 'strictly temporal view of background knowledge' can be answered.

12 A. Einstein, 'On the Electrodynamics of Moving Bodies', in H. A. Lorenz et al., *The Principle of Relativity*, New York 1952, p. 37.

13 For details and references see my 'Overdetermined Problems in Science', *Studies in History and Philosophy of Science*, 9, 1978, 14-16.

problem situation. That would require a careful account of problem situations and background knowledge, and a detailed study of what Popper and others have proposed along these lines. My point is rather that problem-solving accounts of theory appraisal that make reference to pre-existing problems are more promising than those that do not.

IV

In the third chapter of *Progress and Its Problems*, 'From Theories to Research Traditions', Laudan reworks the problem-solving approach to incorporate a view which has been strongly urged by Kuhn and Lakatos, namely that 'global theories' (paradigms, research programmes, etc.) are 'the primary tool for understanding and appraising scientific progress' (p. 71). Laudan does this, he says, in the interest of 'fidelity to scientific practice and usage' and because failure to do so would mean that 'many of the epistemic features . . . most characteristic of science' would elude us (ibid.).

This shift in emphasis finds expression in a new account of 'global theories' (which Laudan calls research traditions) and in a new account of theory acceptance. Specifically, Laudan suggests that we view a research tradition as 'a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain' (p. 81); that we take 'the acceptability of a research tradition (to be) determined by the problem-solving effectiveness of its latest theories' (p. 119); and that when determining the acceptability of a theory we take into account both its problem-solving 'effectiveness and . . . the acceptability of (its) related research tradition' (ibid.).

When the problem-solving approach considered in section II is supplemented with this view about research traditions, the criticisms I raised against it appear to be readily answerable. We can appeal to the constraining role of research traditions (p. 89) to rule out 'wild' scientific and methodological theories; we can appeal to their problem-determining role (p. 86) to define what counts as a significant problem for a given theory; and we can appeal to the fact that a theory need not be associated with an acceptable research tradition to make room for the possibility of its being unreasonable to accept any theory from among a group of competing theories.[14]

Nevertheless, we would be ill-advised to adopt this approach, since virtually the same problems arise for research traditions themselves. (1) Since the theories which constitute a research tradition may be related to it historically as well as conceptually (p. 85), the possibility arises of a research tradition being rejected as a result of its being associated with 'wild' component theories. (2) We cannot appraise research traditions in terms of the problem-solving effectiveness of their latest theories in the way Laudan suggests, since research traditions 'influence the recognition and weighting of empirical and conceptual problems for their component theories' (p. 93). And (3) since research traditions are always appraised 'within a comparative context' (p. 120), we are always obliged to accept one or other of the traditions under discussion.

The situation changes, but does not improve, when we couple Laudan's views about research traditions with the problem-solving approach considered in section III. The problem here is not so much that research traditions give rise to as many problems as they solve, but rather that we have no need of them once we avail ourselves of the notion of background knowledge. This is clear in the case

14 Laudan does not mention this possibility; indeed, given his insistence on the comparative nature of all appraisals, it is likely he would resist proceeding this way.

of questions concerning what we should count as an empirical problem, the importance of problems, the sorts of objections which should be recognized as conceptual problems, and so on. As noted above, all these can be seen as being determined by background knowledge. Somewhat less clear is the question of whether there are any well-established phenomena which can only be accounted for by appealing to research traditions. In this paper, I shall confine myself to a consideration of the phenomenon that is most frequently cited as signalling the presence of 'global theories', namely the phenomenon of scientists' making progress by resisting theories with superior problem-solving effectiveness in favour of theories with long and spectacular histories; I shall argue that 'tenacity' of this sort can be accounted for without appealing to research traditions; and I shall conclude, pending further investigation, that Laudan's shift from theories to research traditions is uncalled for.[15]

There can be no doubt that scientists frequently, and apparently reasonably, refrain from accepting theories which yield a closer fit with the available data than the theories they actually accept. For instance, in the nineteenth century, many scientists rejected Airy's suggestion that Newton's law of gravitation 'falls off' at great distances, the suggestion of Tisserand and others that the same law should be supplemented with terms involving the velocity of propagation of gravitation, and Hall's suggestion that the exponent of the distance in this law should be changed from 2 to $2 + 1.612 \times 10^{-7}$, even though they recognized that Newton's theory so modified better covered the data.[16] This is not in dispute; the crucial question is not whether scientists reject theories which better cover the data but whether they reject theories which have more empirical *and* conceptual problem-solving capacity. What needs to be shown, and what has not been shown, is that suggestions such as those of Airy, Tisserand, and Hall better cover the data without generating significant conceptual problems. (That this is far from obvious in the cases mentioned is easily shown: Airy's and Hall's suggestions ran foul of the fact that the propagation of anything from a point source in three dimensions is a function of $1/r^2$, while Tisserand's suggestion rested on a questionable theory of electricity and assumed without argument that the velocity of gravitation is equal to that of light.[17])[18]

15 Besides the problem-determining role and the constraining role of research traditions already mentioned, Laudan also suggests that they have a justificatory role (p. 92) and a heuristic role (p. 89). It is my view, however, that background knowledge can fulfill these roles as well as, if not better than, research traditions. The only other place where research traditions figure significantly is in Laudan's account of rational pursuit (pp. 109-14). However, since this account is unacceptably restrictive, I do not regard it as constituting a serious threat to the view I am urging. (The difficulty here is that Laudan's account fails to allow for the possibility that it may be rational to investigate or pursue—as opposed to accept—a radically inferior problem-solver with a long record of failures.)

16 For Airy's suggestion, see W. M Smart, *Occasional Notes of the Royal Astonomical Society*, 2, 1947, 33-88; for Tisserand's suggestion, see E. Whittaker, *A History of Theories of Aether and Electricity*, New York 1960, vol. 1, pp. 207-208; and for Hall, see S. Newcomb, 'Gravitation', *Encyclopedia Britannica*, 11th Edition, vol. 12, 1910, p. 384.

17 Cf. J. D. North, *The Measure of the Universe*, Oxford 1965, p. 48.

18 The plausibility of the approach to tenacity just sketched depends on our taking theories to comprise law statements and specific claims concerning ontology and methodology, which is how Laudan himself views them (cf. p. 84). The approach would not be successful if theories were merely sets of statements expressing empirical

At this juncture, a proponent of research traditions might interject that even if background knowledge renders research traditions superfluous, we should nonetheless reject the former in favour of the latter. A theory is acceptable, it might be argued, with respect to a research tradition, just in case the number of significant empirical problems (as determined by the research tradition) that the theory solves more than compensates for the number of significant empirical and conceptual problems (again as determined by the research tradition) that the theory generates. What can be said in response to this move?

The first thing to notice in this regard is that the distinction between theories and research traditions is not as sharp as it might seem to be. On the one hand, Laudan argues—I believe quite correctly—that theories comprise ontological and methodological prescriptions as well as 'specific and testable laws about nature' (p. 84), i.e. that they incorporate some of the features we might initially be inclined to associate exclusively with research traditions.[19] On the other hand, Laudan—also not unreasonably—further identifies research traditions with sets of specific individual theories (p. 71); indeed, he explicitly refers to them as 'global theories' (p. 72). Thus, it would appear that the difference between Laudan's two kinds of 'propositional network' (p. 71) is a difference of degree, and that there is nothing above and beyond specific individual theories of varying generality.

A second difficulty concerns Laudan's view that research traditions can evolve: although certain elements of a research tradition are sacrosanct, he tells us, 'the set of elements falling in this (unrejectable) class changes with time' (p. 99). This view may avoid some of the difficulties that bedevil Kuhn's idea of paradigm and Lakatos' idea of a hard core. However, it also introduces other problems: in particular, in what way do the classes of sacrosanct elements change and in what way can evolving traditions be marshalled to play the epistemological role of paradigms and hard cores?

Finally, there is a problem mentioned by Laudan: not only do some traditions fail to involve a common set of ontological prescriptions and some fail to involve a common set of methodological prescriptions, some cut 'across almost every conceivable metaphysical and methodological tradition' (p. 105). Laudan's response to these 'non-standard' traditions is to call for more detailed research; it seems to me, however, that the existence of such 'traditions' indicates that the idea of a research tradition is problematic and that we should avoid appealing to it if we can.

V

To conclude this discussion, I wish to comment briefly on Laudan's view that problem-solving has no direct connection with truth and that it is a mistake to

regularities. That theories should be seen in the way Laudan and I see them is strongly suggested by Kuhn's important observation that what usually goes by the name of laws are in fact 'law-sketches' which require filling out before they can be applied in particular situations. Cf. Kuhn, *The Structure of Scientific Revolutions* (note 5), p. 188. According to this view, a statement of (e.g.) Newton's laws is by no means a complete statement of the theory Newton presents in *Principia*, and Newton's laws have virtually no application in the absence of the Newtonian ontology and methodology.

Of course, I am not claiming that there is nothing in science that looks like a research tradition: as I see it, such appearances are 'surface effects' which arise as a result of the relative stability of the specific ontological and methodological assumptions of individual theories.

19 Cf. footnote 18.

think of science as a truth-seeking enterprise (cf. pp. 123-25). This is a particularly important issue since the plausibility of the problem-solving approach appears to rest on the conjecture that the more problem-solving capacity a theory has, the more likely it is to be true. If, as Laudan suggests, science is merely a problem-solving enterprise, it is difficult to see why we value it as highly as we do.[20]

Laudan's argument that science does not aim at the truth is remarkably simple: if this were the aim of science, he tells us, we should be able to demonstrate what has never been demonstrated, namely that 'science, with the methods it has at its disposal, can be guaranteed to reach the "Truth", either in the short or long run' (p. 125). This, however is unconvincing: that our employing a certain method cannot be shown to lead to a certain goal does not mean that our employing that method to obtain that goal is irrational.[21] For instance, although it cannot be demonstrated that my calling the police will result in my retrieving a stolen object, not only would it not be irrational of me to call the police, in the normal course of events it would be irrational of me not to call them. I conclude, therefore, that Laudan has not shown that our holding the problem-solving approach to scientific theory appraisal does not preclude our also holding the view that science fundamentally aims at the truth.[22]

20 In this regard, recall that Laudan also holds that to accept a theory is 'to treat it as if it were true' (p. 108).

21 This was pointed out to me by Mark Kaplan, to whom I am also indebted for the example that follows.

22 I have not discussed the material in the last three chapters of Laudan's book. These chapters contain an excellent discussion of some of the more important interrelationships between philosophy, history and sociology of science. In writing this paper I have benefited from the comments of Naomi Scheman.