Some Problems about Solving Problems

D.H. Mellor

Cambridge University

The two other commentators, being, like Professor Laudan, scholars, have commented on the whole of his book. As a mere philosopher, I may perhaps be forgiven if I get little further than the first chapter of *Progress and Its Problems.*

We know that the practice of science increases our knowledge and understanding of things and events, and consequently our capacity to predict and control them. That is why the practice of science interests philosophers who are concerned to say how such knowledge and understanding, such capacity for prediction and control, can be acquired. It challenges them to show why scientific practice should deliver these cognitive goods. Some who decline the challenge, or fail to meet it, may deny that it does deliver the goods; but their conscious practice, of preferring aircraft to broomsticks for flight and telephones to telepathy for communication, belies the sincerity of their denials.

Laudan does not deny science's cognitive achievements. He does, however, think that scientific practice makes more sense if seen as solving problems. About scientific theories, Laudan says, "Theories matter, they are *cognitively* important, insofar as - and only insofar as - they provide adequate solutions to problems." (p. 13).[1] That science solves problems no one disputes. So do cooks and Zen masters, historians, mechanics, artists and mathematicians. Every action, mental or physical, may be seen as solving some problem, if only how to pass the time. Laudan himself claims that his approach "...can be applied, with only a few qualifications, to *all* intellectual disciplines." (p. 13).

Laudan starts with science, however, because of its notable successes in solving problems of a certain sort. He needs, therefore, without necessarily developing a detailed demarcation criterion, to say something about what makes a problem a scientific one. We can then ask

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what makes scientific methods especially good at solving problems of that scientific sort. Laudan says it is theories that provide solutions to science's problems (p. 13); but given today's liberal use of the term 'theory' that hardly suffices to mark off scientific from other problems. So Laudan further marks off the species of problem which scientific theories solve by calling them 'cognitive'; and in this species he recognizes two sub-species, the empirical and the conceptual. I shall concentrate on what Laudan says about solving empirical problems. The question now becomes: how does a theory provide a solution to an empirical problem (or at least a better solution than there used to be, or than its rivals provide)?

Before we can assess this reformulation of the traditional question ("How does science increase our knowledge and understanding of things and events?"), we must learn more about the nature of empirical problems. Laudan admits that they "are easier to illustrate than to define." (p. 14). Here are two of his three illustrations: (1) "We observe that heavy bodies fall toward the earth with amazing regularity. To ask how and why they so fall is to pose such a problem.", and (2) "We may observe that the offspring of plants and animals bear striking resemblances to their parents. To inquire into the mechanism of trait transmission is also to raise an empirical problem." (pp. 14-15). To these illustrations he adds the remark that "anything about the natural world which strikes us as odd, or otherwise in need of explanation, constitutes an empirical problem." (p. 15).

I confess that I inferred from this that to solve an empirical problem was just to give an explanation; to say, for example, how and why heavy bodies fall, or what the mechanism is that transmits striking traits from parents to offspring. That would indeed be to increase our knowledge and understanding of these things and events. But then the examples failed to persuade me that it helps us to formulate them as cases of solving problems rather than as cases of explaining facts.

Laudan admits "an apparent functional similarity between talk of problems and problem solving and the more familiar rhetoric about facts and the explanation of facts." (p. 15). Despite his own explanation of what constitutes an empirical problem, however, he goes on to deny that solving them reduces to explaining facts. He then gives four _prima facie_ reasons for his denial.

First, Laudan observes that not all supposed facts are facts. (p. 16). Hot goat's blood does not, it seems, split diamonds; but those who, like Oresme ([4], p. 244), thought it did had an empirical problem despite there being no fact to explain. What can we say to this? Well, we can say that Oresme and his colleagues only thought they had an empirical problem, because they thought (wrongly) that they had this fact to explain. That way we can say more easily than Laudan can why we lack Oresme's supposed problem: we lack it because we know that only facts need explaining and, unlike Oresme, now know that here there is no fact to explain.

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Secondly, Laudan notes that unknown facts pose us no problems. (p.16). Why should they? Since only facts need explaining, we see no problem where we see no fact. It is Laudan, not I, who has a problem here, namely to explain why nothing seems problematic until it seems factual.

Thirdly, as Laudan says "many known facts do not necessarily constitute empirical problems." (p. 17). Now, of course, our changing interests, theoretical and practical, affect what facts we set out to explain. Our interest in prehistoric man is what makes us want to explain the distribution of his remaining artifacts; our interest in curing cancer is what makes us look for the cause of that disease. Without such interests we might well overlook these matters. I do not see that "contemporary philosophy of science" does or need "tend" as Laudan says, "to assign all problems equal weight." (p. 14). Speaking for myself, I do indeed tend to study the adequacy of solutions separately from the significance of problems. I do so because I believe them to be largely independent of each other. How well Newtonian gravity explains the falling of heavy bodies seems to me to depend very little, if at all, on why or how much it interests us to know how heavy bodies fall. Until Laudan convicts me of error in that opinion I shall decline his invitation to mix these matters up.

Fourthly, Laudan remarks that problems come and go and facts do not. (p. 17). Thus, he points out, geological theorists no longer have the problem "of explaining how the earth took its shape within the last 6,000 to 8,000 years." (p. 17) Nor indeed do they; and their supposed problem vanished, I suppose, just when they stopped thinking that a fact.

These points, however, are peripheral. They enable me to defend the "familiar rhetoric" of explanation by translating what Laudan says into truisms about it; but if that were all, our dispute would be a mere matter of rival idioms. Fortunately Laudan's account of empirical problem solving is not all truism; and the crux is his cavalier treatment of truth. "In determining if a theory solves a problem," he says, "it is irrelevant whether the theory is true or false, well or poorly confirmed." (pp. 22-23). That certainly does not translate into a truism about explanation. Truth, as Laudan says, has long been thought a virtue in explanations. Even those daunted by the difficulty of securing truth itself have asked at least for verisimilitude, probability, corroboration or some other more attainable next best thing. Laudan imposes none of these requirements.

But do we not look for truth even in Laudan's own examples? Take his problem about how and why heavy bodies fall with such regularity. At one time we saw a solution in the Newtonian attraction the earth has for heavy bodies. Is that solution not impaired by the falsity of Newton's theory? Given our continued interest in the matter, would the problem not have revived had a more believable theory not given us a new solution? And if falsehood were no bar, why should appeal to, say, connecting springs not have solved the problem well enough?

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Laudan says "We can all agree...that Ptolemy's theory of epicycles solved the problem of retrograde motion of the planets, regardless of whether we accept the truth of epicyclic astronomy." (my emphasis). (p. 24). But his past tense here gives the game away, as it does in his other examples. Why not say it has solved the problem, solves it even now, if truth and confirmation are neither here nor there? No doubt we have better solutions now, but there is more to Ptolemy's downfall than that. The fact is that now we do not believe planets move as Ptolemy's theory said. We would not offer that theory as a solution to any empirical problem.

I am, moreover, puzzled why Laudan, discarding truth, still takes the virtues of entailment so much for granted. He denies that theories need entail "an exact statement of fact to be explained" (p.22); but it is central to his account that a theory entail "an approximate statement of the problem" (p.22) it solves. Yet all entailment does is pass on a theory's truth. Why call for that when there need be no truth to pass on? Yet theories do entail approximately what they explain. Entailment has no serious rival as a candidate for the link between a theory and a problem it solves. The problem may have to be restated in "theory-laden" terms (hence, often, the approximation); but that is done precisely to secure the requisite entailment. Laudan is right enough to recognize that entailment is required; my complaint is that without truth he cannot explain why it is required.

Even apart from explanation, one might want to know what a theory entails in order to find out what it asserts, or in order to test it. But these interests also depend on assessing theories in terms of truth. To say a theory's truth is irrelevant to its problem solving role is to say in effect that it needn't be asserted, in which case it does not matter what its assertion would entail. Likewise, if theories needn't be true, why should they ever be tested for truth and have it count against them if they fail the test?

Truth remains the crux. If theories are not meant to be true, it is hard to make sense either of their use in explanation, or of their assertion, or of their testing. Laudan, however, does not remove the truth requirement because he is averse to it; even if, as I believe, he fails to appreciate how little of his own account survives its removal. He removes it because, like many others, he thinks we can't know whether or when it is met. He says "we apparently do not have any way of knowing for sure (or even with some confidence) that science is true, or probable, or that it is getting closer to the truth." (p.127). To set up such aims "as goals for scientific inquiry," he infers, is "...not very helpful if our object is to explain how scientific theories are (or should be) evaluated." (p. 127). I take this inference to be ad hominem, challenging those of us who demand truth in explanations to meet their own demand when explaining how theories are evaluated. Since, however, truth doesn't figure in his own criteria, Laudan himself can hardly reject our theory as unhelpful, when it solves the evaluation problem in other respects, just because it isn't true.

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Let me, however, instead of swapping *ad hominem* arguments, try to meet Laudan's challenge. What we have to explain is how we evaluate theories as solutions to empirical problems, the question being in particular what role truth does or should play in our evaluation. Now truth is not a goal I and other philosophers have gratuitously imposed on scientific enquiry. Rather it is a goal we detect there; and without it, as I have illustrated, we find the evaluation of scientific theories hard to explain. And truth is not really as formidably unrealisitic a goal as Laudan suggests. We do not have to say that people must be right in evaluating theories as true, merely that they must think they are, i.e., that they must believe the theories to be true. That is all our goal of truth here amounts to: that people do (and should) believe theories to be true which they believe solve empirical problems. And is that not so? We must, of course, make an exception for theories that are still in the early stages of construction, still the object of little more than test and speculation, especially at the frontiers of physics. I dare say only a few people yet believe for sure in quarks; and by the same token I suppose that only those few are sure that quarks solve the problem of the plethora of weakly interacting particles. But mature theories, on which we base technologies to which we trust our lives, are another matter. Can we really believe that something like gravity explains the falling of heavy bodies without believing in something like gravity? Or that something like genes transmit traits without believing in something like genes?

By beliefs, of course, I do not mean merely introspectible feelings of conviction. I mean those mental states that, along with our desires and aversions, determine our activities, both physical and mental. And I deny that scientists and engineers can be generally so frivolous or so schizophrenic as to separate the activity of explanation from their other serious practical concerns. Neither they nor we in real life accept for purposes of explanation what we would reject or seriously doubt for the purpose of determining our other activities. If we ceased to believe in anything like gravity, we should in particular cease to believe in its power to solve empirical problems.

Not so, says Laudan, drawing in support an inductive inference that is surprisingly fashionable given its crudeness and the weakness of its premises: "Most of the past theories of science are already suspected of being false; there is presumably every reason to anticipate that current theories of science will suffer a similar fate." (p. 126). To anticipate that fate, presumably, we should now stop believing current theories while continuing *faute de mieux* to accept the solutions they provide to our empirical problems. But surely what makes a theory current with us is just our now believing it. To believe past theorists mistaken is just not to share their beliefs. We cannot set out in like manner not to share our own current beliefs. Beliefs are not like baggage: we cannot impartially compare ours with our grandparents' and conclude that ours are no better than theirs were. To think that my beliefs are as bad as ones I do not possess is *ipso facto* to lose possession of them. Those who say we do not or should not believe our theories, because we are not justified in doing so, must say what else

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besides belief, then, makes these theories ours. Laudan does not say; and until he does I shall continue to deny that we can believe we have a solution to an empirical problem without believing the solution.

I do not deny the problems facing accounts of belief: problems of distinguishing beliefs from other mental states and one belief from another; problems of relating the content of belief to its linguistic expression. But I do deny that the problems are insoluble or that theoretical beliefs are especially problematic. Their most notably problematic features are that they are somewhat indeterminate (especially in the case of what Laudan calls "research traditions", such as atomic theory and the theory of evolution (pp. 71-72), general, and conditional. None of these features is peculiar to theoretical beliefs; any account of belief must cope with them, and in so doing may well suffice to explain the peculiarities of theories.

The indeterminacy of theories, for example, is what gives them their notorious power to survive "anomalies". We believe, as I said, in "something like" gravity, "something like" genes. That does not mean our attitude to these theories is not one of belief; merely that the detailed linguistic expression of a theory overstates what we believe. There is nothing peculiar to theories in that. None of us fully understand, let alone believe, all the consequences that may be drawn from what we rightly and sincerely say. That, of course, creates a problem for theories of meaning in relating what our words mean to what we believe, but some recently developed approaches to it seem to me to show enough promise of solving the problem (see, e.g., [2]).

Similarly with the generality that theories possess. We might start with Ramsey's observation that a general belief such as the belief that all _F's_ are _G's_ is less a judgment than a disposition to believe _F's_ to be _G_ ([6], pp. 133-138). To be so disposed need not be to believe a possibly infinite conjunction of instances. The disposition may be applied to explaining the known _G-_ ness of some _F's_ while others remain unknown; it may even survive quite a lot of evidence for some _F's_ not having been _G_. I expect accounts of belief as high degrees of belief to show how and how far this is possible, especially recent accounts of conditional beliefs (such as the belief that if this is _F_ then it is _G_) in terms of related subjective probabilities (see, e.g, [1]). Again I see more hope, as well as more need, than Laudan does of reconciling belief in theories with the history of science as he presents it.

Since science is supposed to give us knowledge, however, another stock objection still remains: if knowledge needs belief it also needs truth and justification, and belief in the generalities of science cannot be justified or known to be true. So, as Popper says, scientific knowledge does not need belief or, as Laudan says, science can solve our empirical problems without giving us knowledge. Popper ([5], chs. 3,4) does not seem to me to have given any account of how knowledge can exist without believers; his "Third World" seems to me no more than a label for objective knowledge, not a credible alternative location for it. If it is not itself merely our shared belief, at least it needs a shared disposition that we have to believe scientifically certified literature. To contain knowledge, libraries need people disposed to believe what they read there. Knowledge, however objective, still depends for its effects on being believed. But what then of truth and justification?

I have followed Ramsey ([6], pp. 44-5) in contending the concept of truth to be redundant, given the concept of belief. To believe others' beliefs to be true is just consciously to share those beliefs; to believe one's own beliefs to be true is just to be aware of having them. As for justification, I again follow Ramsey ([6], pp. 126-7) and others in thinking justification to be mistaken as a requirement for knowledge. Knowledge needs not to be justified but to be obtained by reliable means; in the case of science, that is by causal means which, as a part of the world, it is also the business of science to inform us of. So our scientific theories of ourselves and of the rest of the world need also to say how it is that applying our scientific methods generally causes us to believe the world (and ourselves) to be as our scientific theories say. Then in believing our science, we will believe in particular that its methods are reliable and the beliefs they give rise to mostly knowledge. Stronger tests of science's claims to increase our knowledge are neither possible nor necessary.

By affecting not to share the scientific beliefs of our day, Laudan is naturally led to deny that they amount to knowledge. In so doing he deprives himself of the means amongst other things of saying how scientific knowledge grows; and in effect he consoles himself for that by saying that it does not. That is what his discarding the truth requirement amounts to, and that is what I jib at. A "science" that we take to solve our empirical problems without telling us what we take to be the truth is not credible, and it would be of no philosophical interest if it were. Yet that, for all its other virtues, is what Laudan's book purports to offer us. I have tried to indicate briefly how Laudan could concede truth more easily than he supposes. If he did so, his book would be fine - but his problem-solving terminology would be redundant. His book would then appear to be what in reality it is: a significant addition to the literature that attempts to explain the fact of growth in our scientific knowledge.

Notes

[1] All quotations from Laudan are taken from [3]. All references to Laudan will be made by page number only.

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