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LAUDAN'S PROBLEMS

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Rationality consists in making the most progressive choices. This is the startling claim of Laudan's *Progress and its Problems.*[1] "*The chief way of being scientifically reasonable or rational is to do whatever we can to maximize the progress of scientific research traditions* (p. 124)." And "to make rational choices is, on this view, to make choices which are progressive. . . . By thus linking rationality to progressiveness, I am suggesting that we can have a theory of rationality *without presupposing anything about the veracity or verisimilitude of the theories* we judge to be rational or irrational" (p. 125).

If Laudan were found going up an escalator which was coming down, and we observed that it was not getting him anywhere, he might well point to the progress in the number of steps he was taking. Where is there to go? We need only to march ahead!

In the Prologue, we are informed that the kind of progress he is concerned with is *cognitive progress*, which is "nothing more or less than *progress with respect to the intellectual aspirations of science*" (p. 7). And cognitive progress, it is claimed, neither entails nor is entailed by, material, social or spiritual progress. What, then, are these purely intellectual aspirations, which have nothing to do with truth, veracity, or verisimilitude?

The view we are offered is that an intellectual endeavour is directed to the solution of problems. The criterion for choosing between theories and between competing "research traditions" (more on these ahead) is what he calls "problem-solving effectiveness". The importance that is attached to problem-solving and to problem-solving effectiveness marks this book out as both unusual and a fresh approach to understanding science. All the traditional issues of epistemology and methodology re-emerge in this work in a new light. For the reader to appreciate why problems loom so large in *Progress and its Problems*, however, it is necessary to see that Laudan is a "research programme" philosopher.

The traditional unit of analysis in philosophy of science has been the theory, or the hypothesis. Laudan, however, belongs to the small group of philosophers who regard "research programmes" as the primary unit of analysis. What Laudan shares with these philosophers is their disavowal of empirical or intellectual foundations of science. In this they

[1] Larry Laudan, *Progress and its Problems*, Berkeley, 1977, pp. 257 + x.

follow Popper, since the notion of a programme of research dates back to Popper's theory of science.[2]

Popper's view, in brief, is that the mark of the scientific attitude is to take fasification seriously, rather than verification, because the former is what scientific theories risk, but which pseudo-scientific systems of thought dare not. Scientific theories are highly falsifiable, or have great "empirical content".

Popper and Agassi, however, discovered that the history of science reveals long periods dominated by hypotheses of a certain type.[3] Theories of electricity, for example, in the eighteenth century may be called Newtonian to the extent that they presuppose a theory of matter, force, and the relationship of acceleration to force which was first suggested by Newton. If we describe the presupposed theory of Newtonian physics as "Newtonian metaphysics", we may ask "How scientific is the scientist in his choice of his metaphysics?" The interesting fact is that during the eighteenth century there were many hypotheses about electricity, and many refutations of these hypotheses. From the point of view of fallibilism, the hypotheses considered refuted were not the only ones which need to have been so considered. The scientists might have given up their "Newtonian metaphysics", and set about looking for an alternative type of hypothesis about electricity. They did not.

Popper and Agassi came to the conclusion that metaphysics is an ingredient in science, but not empirically refutable. It is rather like a rich suggestive idea as to how to construct hypotheses about the facts of the matter. Research conducted under this umbrella is part of a "metaphysical research programme". This does not clarify the status of the metaphysical component in science, since there appears to be no method for evaluating this component in a series of theories of a certain type.[4]

In *The Structure of Scientific Revolutions*, T. S. Kuhn gave a radical turn to this issue.[5] He states that the metaphysics in a research programme (which is part of a "paradigm" in Kuhn's view) is not held explicitly by scientists, nor is it critically evaluated. It is rather a presupposition of social behavior in a scientific group. From Popper's point of view, this renders the attitude of many scientists to metaphysics for long

[2] Cf. Karl R. Popper, *Conjectures and Refutations*, New York, 1963, ch. 1, pp. 33-65.

[3] See Popper's *Unended Quest*, Glasgow, 1974, pp. 150-1, 227 (fn. 242), and J. Agassi's doctoral dissertation, "The Function of Interpretation in Physics", London University, 1956, especially the introduction.

[4] Several solutions have been offered to this problem, for example, Popper's "The Nature of Philosophical Problems and their Roots in Science", *Conjectures and Refutations*, ch. 2, pp. 66-98, which asserts that metaphysic is criticizable, just as science is falsifiable, and J. O. Wisdom's "The Refutability of Irrefutable Laws", *British Journal for the Philosophy of Science*, 1963.

[5] T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago, 1962.

stretches of time quite unscientific. Nevertheless, Kuhn's descriptions of the normal scientist as a "puzzler solver" have great persuasive force.

Lakatos sought to rescue Popper's conception of rational science by showing that the apparently dogmatic attitude to metaphysics was actually a rational choice prior to conducting research.[6] Lakatos argued that just as great falsifiability (empirical content) is a mark of good theories, so increasing empirical content is a mark of a succession of good theories. Scientists choose research programmes for their increasing corroborated content, and therefore their choice of a certain strategy is not a dogmatic allegiance to a metaphysical view, but is simply a necessary scientific strategy. It is perhaps for this reason that Lakatos refers to such traditions as "scientific research programmes", rather than "metaphysical research programmes".

The background for Laudan's work lies in the difficulties of Lakatos' theory of scientific research programmes. While Lakatos wrote about progressive and degenerating problem shifts to refer to content increasing and decreasing scientific strategies, problems and their analysis have no particular function in Lakatos' scheme. Laudan's central task is the investigation of problems, for the purpose of providing the necessary understanding of scientific research programmes.

The difficulties of Lakatos' theory lie in its exclusive attention to empirical content, and in the rigidity of its conception of research programmes.[7] But on this basis Lakatos' theory does provide an account of how scientists may be rational, even though they are apparently "dogmatic" about some elements of the research programme. Laudan notices, correctly, that *contra* Lakatos there are significant conceptual or nonempirical factors in debates between scientists. Furthermore, the rigidity of research programmes is not found in the history of ideas. This sets the task of providing a new reconstruction of the rationality of the scientist who chooses between research programmes (or "research traditions", since they evolve). The new candidate for the criterion of choice between research traditions is "problem-solving effectiveness", which includes an empirical element and a conceptual element.

In more detail, Laudan's view is that "*the central cognitive test of any theory involves assessing its adequacy as a solution of certain empirical and conceptual problems*" (p. 70). "Theories . . . are *cognitively* important, insofar as—and only insofar as—they provide adequate solutions to problems (p. 13)." This constitutes a remarkable turn from the analysis one finds in Popper's writings. Popper is primarily concerned with theory rejection; Laudan, on the other hand, interprets the central

[6] I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes", *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. E. Musgrave, Cambridge, 1970.

[7] Another difficulty in Lakatos' view which Laudan takes up is that Lakatos' rational reconstructions often are at great variance with the history of science. Cf. Lakatos' "History of science and its Rational Reconstructions", *Boston Studies*, 1970.

problem of epistemology as one concerning theory selection. In short, the focal point for evaluation is not a theory's *failure* to resolve problems, but its *successes* (*pace* Lakatos).

Laudan advocates a calculus for counting problems. A successful theory will *'maximize the scope of solved empirical problems, while minimizing the scope of anomalous and conceptual problems''* (p. 66). This calculus, moreover, is not to be employed *in vacuo*. But what we must preserve, "if we are to understand either the logic or the history of the natural sciences, is the notion of the integrity of a research tradition, for it is precisely that integrity which stimulates, defines and delimits what can count as a solution to many of the most important scientific problems" (p. 80).

So involved is Laudan in this approach to science that he neglects to ask "If this is what science is, what value is it?" The central difficulty of this book is that having given up pragmatic value and verisimiltude as possible aims of science, it has only the internal criterion of "problem-solving effectiveness" for its aim. Almost any activity we engage in would seem to be preferable to this, unless the solving of problems has an independent value. If so, what is it?

Problems

The success or failure of this approach turns, however, on something else. It depends on whether his analysis of problems and solutions does give us an objective and realistic understanding of science as a rational enterprise. As we shall see, it fails in this, though in a way that is not without interest. Two types of problems are distinguished—empirical and conceptual problems. Unfortunately, it is not clear whether this distinction is one of kind or degree. The notion of problem-solving effectiveness requires that they differ in kind, since we are told that the problem-solving effectiveness of a theory is determined "*by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates*" (p. 68). The proposed calculus, then, requires adding one sort of problem, and subtracting the other sort. But "in point of fact, there is a continuous shading of problems intermediate between straightforward empirical and conceptual problems" (p. 48). If this is the case, the notion of problem-solving effectiveness is sterile: where any problems falls between the extremes of the purely empirical and conceptual, the calculus offers no provisions as to whether or not solving this problem adds to the worth of a theory. More importantly, *Progress and its Problems* offers no criteria for determining where a problem falls, except for those that fall at the distant ends of the spectrum.

Empirical problems are defined as "anything about the natural world which strikes us as odd, or otherwise in need of explanation" (p. 15). Again they are "substantive questions about the objects which constitute the domain of any given science" (p. 15). What this means is that em-

pirical problems are *not systemic*—they are not about our system of theories, but about the natural world. And even though they may arise within a context of inquiry, it is a mistake to regard them as properties of this or that theory. They are not a far cry from Lakatos' "empirical content".

Conceptual problems, on the other hand, refer to the well-foundedness of the conceptual structure of a theory which is originally devised to answer empirical or first order problems. Conceptual problems arise as a result of internal inconsistency, lack of clarity, vagueness, and/or conflict with other theories. This discovery of conceptual problems is said to be significant; so much so, that "a conceptual problem will, in general, be a more serious one than an empirical anomaly" (p. 64). This constitutes his main advance beyond Lakatos. What he emphasises, however, is the notion of *inconsistency with an external theory*. Internal conceptual problems, he claims, play a less decisive historical role.

The discovery of "conceptual problems" does constitute one of the most significant advances beyond Lakatos. However, *the discovery that problems have a conceptual or intellectual aspect does not warrant rigorously distinguishing this aspect as a category unto itself*. Consider the following event: a billiard ball is at rest on a table, and is struck by a smaller ball moving at great velocity, thus moving the larger ball in the same direction as the motive force. This event is commonly observed, yet it constituted an important empirical problem for Cartesian science. More importantly, in terms of Laudan's theory, it is at the same time a conceptual problem. Descartes' theory of matter reduces space and matter to extension. Moreover, this extended matter is inert and indifferent to its state of rest or motion. Inertia is defined by Descartes as a resistance to force. Thus, Descartes has a difficulty, or an internal conceptual problem, in reconciling the inertness of matter with the resistance of inertia, since the latter both appears to imply a force and is a property of matter. Descartes' solution is to argue that the resistance is illusory, and a consequence of the size of a body. The result is a kinematic relationship between size and force, such that the larger a piece of matter, the more difficult it is to put into motion, regardless of the velocity of the acting body. The consequence of this *internal conceptual* problem is that Descartes' theory generated an *empirical* problem—*viz.* the phenomenon of impact. The same problem may be both empirical and conceptual at once.[8]

In fact, the admission that "the world is always perceived through the 'lenses' of some conceptual network . . ." (p. 15) would seem to deny the distinction between empirical and conceptual problems altogether. If experience is theory-laden, it would appear that all problems are *essentially conceptual*. Laudan asserts that theories prescribe an ontology and

[8] For a treatment of all problems as having a conceptual, or an intellectual aspect, see J. N. Hattiangadi, "The Structure of Problems", Parts I et II in *Philosophy of the Social Sciences*, Vol. 8, No. 4, Dec. 1978, and Vo. 9, No. 1, 1979.

methods for research—that standards are relative to particular frameworks—yet he wants to maintain that empirical problems are immune, at least in part, to the relativity of standards. He cannot have it both ways.

These difficulties stem from the very characterization of problems in this book, namely the view that empirical problems are questions—"if problems constitute the questions of science, it is theories which constitute the answers" (p. 13). While it is true that questions may arise when a theory runs into conceptual difficulties, they also arise when "someone decided it [an event] was sufficiently interesting and important to deserve explanation" (p. 17). In the latter case, inquiry is not directed by theoretical concerns, but by a natural sense of wonder about the world. "If a sound justification for most scientific activity is going to be found, 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 (p. 225)."

Man is a curious animal. So even when we are not in the possession of theories, we continue to ask questions—"in a new scientific domain, i.e., in a domain in which no adequate, systematic theories have yet been developed, almost all empirical problems are on a par" (p. 33). How do scientists distinguish between interesting and idle questions? At any time, scientists concentrate on some phenomena rather than others.[9] Laudan recognises this. The postulation of a "compelling" sense of curiosity would seem to run counter to this fact. But a careful consideration is given to such an objection, since it is recognised that "if the problem-solving approach is ever to become a useful took for appraisal, it must be able to show how, and why, certain problems are more significant than others" (p. 31).

To this end, empirical problems are divided into three classess—unsolved, solved, and anomalies. Unsolved problems are regarded as irrelevant, at least for the purposes of evaluation, since they do not indicate whether a theory is more successful than a rival. In other words, the only sorts of questions which function in the calculus are those which have solutions. For this reason, this theory is not so much about problems, as about solutions.[10] Solved problems weigh in a theory's favour, and indicate that scientists "believe they understand why the situation propounded by the problem is the way it is" (p. 22). And since the suggested

[9] Cf. J. Agassi, "The Nature of Scientific Problems and Their Roots in Metaphysics", *Science in Flux*, Boston, 1975, pp. 208-39.

[10] Laudan argues that dissolution deflates the importance of problems (pp. 35-6), since "*it is the solution which allows us to recognize* the *problem as a genuine problem at all*" (p. 33). This is an unlucky consequence of the success orientation of this theory; unlucky because it stands at odds with the fact that dissolutions—such as Galileo's dissolution of problems concerned with theories of impetus, Newton's dissolution of problems concerned with impact, and Einstein's dissolution of problems concerned with the composition of the ether—are of the greatest importance in the history of science.

notion of progressiveness is relative to a theory's successes, there are no prohibitions upon the way in which a solution is engineered, not even as concerns the employment of *ad hoc* manoeuvres. "Ad hoc modifications, *by their very definition*, are empirically progressive (p. 115)." 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. If we allow *ad hoc* manoeuvres, where is the difficulty in solving any problem? As Kuhn says, failure to achieve a solution discredits only the scientist and not the theory. It is not clear how Laudan can respond to this.

In any case, how does one count problems? How many questions does Newton's theory answer? Well, an infinite number of non-equivalent statements can be deduced from any theory. Corresponding to each consequence, one can formulate the question "Why is this so?" which the theory explains. Since the only sorts of questions relevant for evaluation are said to be those which have solutions, we can understand that we are to count only solutions. But how does one compare the number of problems solved on Newton's theory with those of Descartes'? They are both infinite in number. Laudan asserts that only a finite number of problems are solved by any theory, though it may have an infinite number of consequences.[11] This may be the case, but for all that he says about problems it is impossible to limit them to anything but a denumerable infinity.

Although there are several crucial respects in which this model of a solution is quite unlike the deductive-nomological model of an explanation, in this the most significant respect the two are identical. Laudan gives up the requirement that solutions be true, that they derive the statement of the problem exactly, and even suggests that standards for judging solutions may change in time. All this marks his theory of solutions as different from the explanatory account. But the central feature of his view, "problem-solving effectiveness", is ineffective; it is ineffective because the prefered account of problems and solutions leaves problems and their solutions denumerably infinite, like the class of consequences of an explanatory theory, or the number of possible sets of premises which can be used to derive any facts.

Research Traditions

The possibility that this theory of problems is not well developed enough to rescue Lakatos' conception of science is not necessarily discouraging. One might expect a few modifications to rescue this account of them. If Laudan's basic insight is right, then problems do somehow get counted, even though his remarks may not make it clear that they can be counted. But the trouble with the view goes deeper than this: there is good reason to suspect that the distinction between research traditions and theories is untenable. Research traditions simply are not

[11] Laudan, p. 228 (fn. 4).

things that scientists choose, rationally or otherwise. If this is so, then the suggested notion of problem-solving effectiveness, however interesting it may be, is fundamentally misdirected.

Laudan does not elaborate *why* this distinction is so essential. One can find the answer in Kuhn, perhaps, and in Lakatos' writings certainly, but not in Laudan's. He warns us, though, that "*until we become mindful of the cognitive and evaluational differences between these two types of theories, it will be impossible to have a theory of scientific progress which is historically sound or philosophically adequate*" (p. 72).

Even a casual study of the history of science would convince anyone that there are long periods where scientists seem to be dominated by one or other general theory—not a single theory, but "an entire family of doctrines, historically and conceptually related, all of which work from the assumption that organic species have common lines of descent" (p. 72). Sometimes we may discern two or even three global theories which share the stage. When Laudan refers to these as "primary units of analysis", however, he is suggesting that scientists cannot just do physics, for example, but must first decide whether it is Cartesian, Newtonian, or Aristotelean physics that they wish to do. Though this view allows for debates on fundamentals (unlike Kuhn's), and also allows for the evolution of fundamental metaphysics and methodological assumptions (unlike Lakatos'), it retains one feature of "paradigms" and "research programmes". As Laudan says, "put simplistically, *a research tradition is thus a set of ontological and methodological 'do's and 'don'ts'*. To attempt what is forbidden by the metaphysics and metodology of a research tradition is to put oneself outside that tradition and to repudiate it (p. 80)." A research tradition, one might say, then, is a class of possible theories that might solve problems, each member of that class being much like an explanatory hypothesis (with a few differences, as mentioned earlier).

On this model the individual scientist first chooses a role, and then acts out that role. He first becomes a Cartesian, for example, and then does science according to the dictates of that research tradition. The decision to don the Cartesian cloth is a rational choice, because it is based on an evaluation of the problem-solving effectiveness of Cartesian research compared with its alternatives. Once accepted into the Cartesian order, however, the scientist plays with theories to solve problems according to the rules of the Cartesian game.

The distinction between theories and research traditions is simply untenable. If one compares Huygens, Leibniz, and Newton as Cartesians, one sees immediately why the distinction does not work. First of all, each of their theories was proposed as a modification of Descartes' physics in order to solve some problem it generated. Secondly, each accepted some parts of Descartes' views and denied others, to arrive at three different modified Cartesian theories. Thus, each rejected Descartes' identification of matter with extension; both Huygens and

Leibniz accepted Descartes' thesis that gravity is the effect of an extraneous action, Newton did not; Leibniz adhered to Descartes' denunciation of the void, Newton and Huygens did not; Huygens retained kinematics as the model of impact, Newton and Leibniz did not; and finally, Newton retained Descartes' concept of the quantity of motion as a measure of force, Huygens and Leibniz did not.

If research traditions evolve, (as against Lakatos' concept of a single set of doctrines which characterize a research tradition throughout its history), it would seem to entail a loss of the integrity or identity of a research tradition with every modification of its assumptions. Laudan, however, suggests that only "certain elements . . . are sacrosanct, and thus cannot be rejected without repudiation of the tradition itself. . . . But unlike Lakatos, I want to suggest that *the set of elements falling in this (unrejectable) class changes through time* (p. 99)." If a research tradition has any sacrosanct "do's" and "dont's" at any given time, how can it possibly evolve? At the moment of change, someone has to do a "don't", and get away with it. In that case the "don't" is flexible, and the whole idea of a "research tradition" becomes gratuitous. What is a research tradition if it has all its elements questionable in principle, like a theory?

If, on the other hand, there are unproblematic assumptions of any research tradition, how can we possibly identify them? From the point of view of Cartesian science, there was not one assumption found not to be problematic at any given time. But then why is it reasonable to regard Newton and Leibniz as having opted out of the Cartesian tradition, and Huygens as having remained loyal? On the basis of certain sacrosanct core assumptions, no answer can be forthcoming, unless a consensus can be demonstrated. The above example indicates that this is not possible.

There is a further problem that the practice of identifying research traditions by their association with certain unproblematic axioms lends itself to any convenient historical reconstruction. From Leibniz's point of view, his denial of the void places him squarely within Cartesian science, whereas his denial of the identification of matter with extension places him outside the tradition. A historian, then, may select any single Cartesian axiom, and conclude that certain figures are Cartesians; or he may select others, and conclude the converse. If this is the case, the unavoidable logical consequence is that Descartes was the sole Cartesian, which is in effect a rejection of "research traditions" as relevant to science.

This leads to the final reason for rejecting the distinction between theories and research traditions. It is simply a matter of arbitrary choice whether we call the three views (of Newton, Huygens, and Leibniz) Cartesian theories, which are predictive (which they are), or research traditions (which they became in the light of subsequent science). Newton's theory appears to be a research tradition *only with hindsight*. To say that he put forward a theory *and* a research tradition is merely to

waffle. Newton did many different things, but he did not plan a tradition. It is much more plausible to regard Cartesian science as beset with difficulties, which are resolved in different ways by the three Cartesians. But to resolve the difficulties, each modifies the theory (or research tradition, as you will) differently. As soon as we contrast these three theories, one with the other, we find that each has its difficulties. (How can Leibniz's or Huygen's theories be reconciled with the observed motions of the heavens? How can Newton explain non-elastic collision?) And so each of these theories can be elaborated, developed, modified, into hosts of alternative theories, such as Euler's, for one. Whether we call Euler's theory Cartesian at two removes, or Newtonian at one remove, or a new Eulerian Research Tradition, is merely to play with words.

Laudan's insight that to accept the Newtonian point of view is to accept some "do's" and "don'ts" about theories is a valuable insight. It shows that Newtonian physics is not just an explanatory hypothesis, but rather *a class of possible explanatory hypotheses.* If the distinction between theories and research traditions is untenable, then *every* physical theory is a class of explanatory hypotheses. If it is proposed to solve some problems, then as a solution to *those* problems it is just a theory. But if it becomes a competitor with others, and engenders debate, then it will appear as a research tradition, or a class of theories.

If one studies history of science, one cannot but notice that there are research traditions. But these research traditions exist only for the historian. Long debated ideas constitute research traditions. Those which quickly succeed or quickly fail are theories. Laudan's entire study of problems presupposes a *difference in kind* between theories and research traditions. Consequently, his study of problems is fundamentally misdirected to the choice between entities with which the scientist is just not concerned. Insofar as a scientist makes choices, he makes a choice between theories. There is no allegiance in this to a research tradition, or the start of a new tradition.

A Paradox

One of the unresolved difficulties of Laudan's proposals is that his account of methodology is historical, even while he is proposing one which is not. Thus, Cartesian science has a set of "do's" and "don'ts" which are peculiar to the Cartesian tradition. Descartes' emphasis on clear and distinct ideas, for example, would be a case in point. If we were to suggest to Descartes that scientific theories should be chosen according to their problem-solving effectiveness, he would, naturally, deny it. According to Descartes, ideas are chosen because they can be demonstrated. If Laudan is right, and the choice between theories has in fact been made by scientists on the basis of problem-solving effectiveness, even while the scientists argued for their own methodologies, then the entire history of methodology must be double talk. Moreover, Descartes would get contradictory advice from Laudan.

Perhaps the best chapter in *Progress and its Problems* is the sixth entitled "The History of Ideas". It is a stirring call for a problem oriented history of ideas. This might be an excellent project, but historians, one must remember, are also intellectuals, who, if Laudan is right, belong to research traditions. Why should they accept Laudan's views? Why is their own research, given their tradition, not the way history should be practiced? Laudan's theory seems to recommend simultaneously that each historian (or scientist) must have his research tradition, but must nevertheless accept Laudan's strictures which are contrary to those inherent in that research tradition. If Laudan is right, then he must be mistaken. He must therefore be mistaken.

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