

6. C. Hempel and P. Oppenheim, "Studies in the Logic of Explanation," in *Aspects of Scientific Explanation*.
7. Cf. R. Eberle, D. Kaplan, and R. Montague, "Hempel and Oppenheim on Explanation," *Philosophy of Science* 28 (1961): 418-428.
8. D. Kaplan, "Explanation Revisited," *Philosophy of Science* 28 (1961): 429-436.
9. Cf. W. Salmon, *Statistical Explanation and Statistical Relevance* (Pittsburgh: University of Pittsburgh Press, 1971).
10. M. Friedman, "Explanation and Scientific Understanding," *Journal of Philosophy* 72 (1974): 5-19.
11. R. Causey, *Unity of Science* (Dordrecht: D. Reidel, 1979).
12. B. Brody, *Identity and Essence* (Princeton: Princeton University Press, 1980).
13. I am indebted to my student, Jeanne Kim, for suggesting this example to me.

## 10

### Truth and Scientific Progress

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Philosophers of science are increasingly taken with the following apparent paradox: theories that excelled under criteria now employed in evaluating theories ultimately proved unacceptable; therefore, it is likely that the best current theories, and even better ones that we might imagine overcoming what defects we now recognize in current theories, will prove unacceptable. Thus, we have a strong inductive argument against the ultimate acceptability of theories that we have strong inductive grounds to accept.<sup>1</sup> It is common to summarize this situation by some such equally paradoxical remark as: "We really know that all of our scientific beliefs are false."

Of course the "paradox" is not genuine. Eschewing induction blocks inference to the fate of current theories from the record of past theories. But then, acceptance of any particular theory on the basis of evidence bearing individually on it is equally blocked, and the negative, general implication as to the prospects for scientific knowledge is sustained. Philosophers of science on many fronts are understandably in retreat from the traditional view that the growth of science delivers ever closer approximations to fundamental truths about the physical world.

Part I of the present paper examines some examples of this movement so as to establish a general perspective on the problem of connecting truth with progressive theory change. Part II defends the traditional view that progress attests to truth by developing the thesis that science exhibits certain forms of progress for which realism with respect to scientific theories

is the required explanation. In Part III the argument of Part II makes a troubled peace with the skeptical induction just indicated. The scope and limits of the realism that emerges are then delineated.

## I

A number of diverse philosophical views associated with Thomas Kuhn may be brought to bear on the problem of connecting truth with scientific progress. Here is a perspective on Kuhn. He begins as a scientist struck by the superficiality and outright ignorance of scientists as to the historical background of their current ideas. He invents an interpretation of science which explains such failings. The explanans is incommensurability: intellectual traditions are sufficiently circumscribable and independent that understanding one's intellectual heritage has no part in the mastery of current knowledge, but is solely the task of the historian. Thus, incommensurability emerges as the cardinal thesis of Kuhn's philosophy of science. Primarily on the basis of this thesis, Kuhn is widely read as denying any connection between truth and progress. In fact, he denies explicitly that any sense can be found in which a theory better approximates the truth than does the theory it replaces.<sup>2</sup> Whether Kuhn merely despairs of finding such a sense, or contends that incommensurability precludes there being one, is unclear. But certainly, on Kuhn's view, the increases of predictive accuracy, scope, and fertility of theories in which progress consists do not attest to the truth or approach to truth of hypotheses. The consequences of hypotheses may be judged true when sustained by experiment; but such judgment is intratheoretic, inaccessible to proponents of alternative theories.

Imre Lakatos, for all his outrage at this last bit of relativism, is equally explicit in divorcing progress from truth. Progress is a matter of increasing Popperian verisimilitude—the excess of corroborated over falsified consequences of theories. But, whereas Popper is prepared, on the strength of Tarski's definition of truth for formal languages, to interpret increasing verisimilitude as approximation to truth in the classical sense of correspondence with objective facts, Lakatos rejects this interpretation as a “dangerously vague and metaphysical idea.”<sup>3</sup> The danger, evidently, is that we are led by Popper's view to suppose the ultimate constituents of the world to be more like the items purportedly referred to by current theory than like those purportedly referred to by past theory; more like fields, say, than like matter and force. And there is no intuitive basis for this assessment. But Lakatos seems simply to have confused the view that the classical correspondence theory of truth is the intuitive or preanalytic

idea of truth and of the goal of science, with the view that scientific advance toward truth in this sense delivers ever more intuitively plausible theories.<sup>4</sup> Popper was certainly not endorsing the latter view in supposing increases of verisimilitude in his technical sense to indicate increases of classical verisimilitude. Popper, as everyone knows, believes that scientific progress decreases the likelihood or plausibility of theories by decreasing their probabilities so interpreted.

Lakatos accepts a Platonized version of Popper's third world; the growth of scientific knowledge is an unarguable datum for Lakatos.<sup>5</sup> But this position fails to connect scientific knowledge with truth. A connection between knowledge and truth is characteristic of analyses of second world knowledge, where the truth of a belief is required for attribution of knowledge to the believer. World three exhibits no such discrimination. To suppose that it should is to confuse objective knowledge with certain knowledge which, if it exists in world three at all, is confined to purely deductive relations within mathematical systems. All theories, at least all developed, articulated theoretical systems to emerge in the course of human history, are admitted undistinguished as to truth value. Indeed, the hallmark of citizenship in world three is autonomy, meaning roughly that these are things that can initiate influences on world two, that we can learn from. And what we learn from primarily, according to Popper, are mistakes.

Crucial to Popper's treatment of rational theory replacement as increasing approximation to truth is the thesis that rationally replaced theories are falsified. Lakatos rejects this thesis because of the underdetermination by experience of singular statements formulating predictions by which theories are tested. The selection of research programs must inherit the conventional freedom attaching to the selection of such statements. Perhaps this result underlies Lakatos's ironically Kuhnian view that the progressiveness of a research program reflects more on the talents of its practitioners than on its scientific merits. If, as Lakatos evidently believes,<sup>6</sup> the more absurd of competing research programs may, with imaginative and skillful patronage, emerge the more successful in anticipating novel facts, then Lakatosian progress is clearly weaker than Popperian. Scientists concerned to advance knowledge are deterred from *ad hominem* rejection of rival research programs only by vanity. Assessments of the rationality of theory choice are not merely irremediably retrospective for Lakatos; they are irremediably fallible. For such assessments commit us to undecidable counterfactuals to the effect that degenerating programs would not have proven more progressive than their preferred rivals if pursued. Lakatosian progress cannot be said to approach truth because it cannot be said to replace error. Rejection is no

more falsification for research programs than for the individual theories replaced in their construction.

Larry Laudan's *Progress and Its Problems* is probably the most explicit recent rejection of the relevance of truth to the analysis of scientific progress.<sup>7</sup> For even knowledge, which might not, after all, prove so readily dissociated from truth as Lakatos imagines, finds no role in Laudan's approach. Laudan neither denies that scientific statements have truth values nor rejects the possibility of a scientific theory being true. But he infers from past philosophical failures that no sense can be made of the notion of relative approximation to truth, and that in the inductively unlikely event that any theory is true this fact about it would be in principle unknowable. To suppose truth to be a goal of science, then, portrays the activity of science as irrational, presumably on the principle that rational behavior has an end whose achievement is at least recognizable. But it is a condition of adequacy for any theory of rationality that our preanalytic intuitions as to the rationality of paradigm cases of progressive theory change be sustained. Therefore, truth is not a goal of science, and individual scientists whose motivations Laudan acknowledges to include a quest for truth are presumably confused in some way.<sup>8</sup> The task of Laudan's work is to give an analysis of progress, in terms of which rationality can in turn be defined, which makes no use of the concepts of truth and knowledge.

This task, if achievable, would certainly constitute an interesting and worthwhile alternative to traditional attempts to understand the rationality of scientific change. There are, however, a number of reasons for skepticism. One is that the record of philosophical failure, which so impresses Laudan as to induce him to discount the very possibility of understanding progress in terms of truth, includes the career of instrumentalism from which Laudan's own problem-solving model of progress is insufficiently distinguished. The claim that scientific statements possess determinate truth values amounts to little if coupled with the claim that such truth values are in principle indeterminable nor even subject to probabilistic estimation.<sup>9</sup> Laudan does maintain, in apparent opposition to instrumentalism, that research traditions impose certain "ontological commitments" on their constituent theories. But the nature of these commitments is unclear, as none of Laudan's criteria for the appraisal of theories are criteria for the acceptability of their ontological claims. Another reason is that Laudan's own problem-oriented approach to his task creates its own philosophical problems which it has yet to show progress in solving. On many crucial points, including the proper understanding of what constitutes a problem or a solution and, in particular, the grounds for evaluation of the latter, we have only promissory notes.

But I shall not pursue such criticisms here. What I stress instead is the counterintuitiveness of an account of scientific progress that finds no role for truth or knowledge. That science does progress and that scientific change is, in large measure, rational, are presuppositions of Laudan's work. By what reasoning does he hold the claim of science to deliver knowledge of the physical world any less entitled to presuppositional status? Philosophers prepared to assume the rationality of scientific change, and to reject interpretations of science that depict theory choice as irrational or arational on this account alone, have invariably credited science with the status of a knowledge-acquiring enterprise. Laudan's dissociation of these assumptions is a radical step requiring some compelling motivation. Until he provides one, or salvages scientific knowledge through some new, equally radical analysis of knowledge requiring neither the truth nor the likelihood of successful knowledge claims, his work is seriously incomplete.

An apparent exception to the movement away from understanding scientific progress as truth approximation is Dudley Shapere.<sup>10</sup> Shapere holds that truth can be salvaged as an achievement of science if truth as a metaphysical ideal is reduced to epistemology. The truth of a scientific statement amounts, for Shapere, to the absence, after a reasonable period of critical inquiry, of specific reasons for doubt with regard to the statement. Thus if a statement thrives according to the evaluative standards of science, it qualifies as true despite the possibility, admittedly ever present, that specific reasons for doubt will yet emerge, and despite the strict consistency of its denial with the corpus of scientific evidence. A statement is to qualify as true if research appropriate by the standards and methods of current science fails to reveal specific grounds for doubting it. But, of course, such grounds might subsequently emerge. Moreover, standards and methods might subsequently so change that the evidential basis of the statement comes to be regarded as inadequate or inappropriate.

According to Shapere, however, it is consistent simultaneously to maintain that the statement is true and to acknowledge these possibilities, to acknowledge even the possibility that such changes will be rational and progressive so that the original endorsement of the statement will subsequently be regarded justifiably as the unfortunate error of a weak or misguided methodology. We might go so far as to imagine the statement falling into such disfavor as to impugn the scientific standards employed in endorsing it. In Shapere's view, the statement qualifies as true despite all such possibilities.

Clearly, various metaphysical features of the concept of truth—for example, that if a statement is true it remains so whatever the vicissi-

tudes of belief, and that a statement might be false even if there are good reasons to believe it and no good reasons to disbelieve it—are being rejected here. What is the justification for this? Does this not amount simply to redefining 'truth' so as to get the result that science achieves truth? Is Shapere's approach any less a retreat from truth than the approaches of philosophers who dissociate truth from knowledge?

Such cogency as Shapere's position has emerges from its response to these challenges. What Shapere ultimately is arguing for is the abandonment of an abstract metaphysical ideal of truth which has no connection with the intellectual processes by which knowledge is acquired and which is, accordingly, unrealizable in principle. Shapere has long insisted that such concepts as "observation" and "evidence" be so analyzed as to reflect their actual use in scientific reasoning, rather than imposed as the abstract philosophical tools of a presuppositionist philosophy of science. By this he means that situations actually regarded as observational or evidential by scientists in the articulation of successful theories are to be accorded such status by philosophical theories of observation and evidence. His move now is to treat truth and knowledge in the same way. A criterion of acceptability for theories of truth and knowledge is that they admit paradigm cases of successful science. A truly empiricist philosophy of science must reject presuppositionism altogether, deferring to science itself the identification of what is true and what is known.

But there is a conceptual difficulty in so sweeping a rejection of presuppositionism. There must be presuppositions at some level if there are to be standards against which the legitimacy of any particular presupposition is assessed. Shapere apparently contests this, maintaining that standards at any level may be rationally violated as science changes. Yet Shapere himself is not offering a presupposition-free philosophy of science in place of the tradition of measuring science against presupposed metaphysical ideals. He is offering a different set of presuppositions on which a particular view of the status of scientific results constrains what we are allowed to mean by 'knowledge' and 'truth'. Successful science explicates the concepts of knowledge and truth, and for criteria of success we simply defer to consensus within science itself. The disadvantage of this substitution of presuppositions is that it abandons the very idea that the scope and limits of the scientific enterprise as a whole are subject to philosophical assessment.

An interesting novelty of Shapere's approach is its emphasis on the continuity and stability of scientific results. While recognizing the instability of scientific methods and standards, and rejecting, on that account, presuppositionist philosophies of science, Shapere requires considerable survival power of those statements that are to qualify as true. For the fact that specific reasons for doubt do not readily appear upon the introduc-

tion of a scientific hypothesis will hardly be enough to render it true. Shapere no more specifies the amount of critical inquiry that a hypothesis must survive to be true than Lakatos imposes a time limit for the rejection of degenerating research programs or than Kuhn specifies the number of anomalies required for revolution. But Shapere invites us to imagine ourselves in possession of a comprehensive theory to which no compelling objections arise over a period of hundreds or thousands of years, and he intimates that reluctance to acknowledge the truth of such a theory could only be philosophical perverseness.

Polemics aside, the interesting aspect of this picture is its utter disregard of the inductive argument against any such foreseeable stability of scientific theory, whose inducement to skepticism with respect to truth I began by noting. If Shapere's approach is to yield its desired result that science achieves truth, there must be some historical counterargument suggesting that science does or will produce theories whose rejection ought not to be anticipated. We will need some account of continuity in science at the level of theory, whereas any account of continuity consistent with the skeptical inductive argument would appear limited to the empirical level at which theories are tested. Laudan, for example, holds that it is only the base of empirical problems that exhibits continuity in science; their solutions are short-lived.

Just such a historical counterargument as Shapere requires has coincidentally been attempted by Levin.<sup>11</sup> It depends, of course, on assuming what Laudan is at pains to deny, namely, that theory change is cumulative. Levin claims that each successive theory is obliged to explain all the facts that its predecessor explained, and more. It must also explain some facts that its predecessor failed to explain, in particular, those responsible for the rejection of its predecessor, and it must explain why its predecessor, although false, succeeded where it did. Given these conditions of adequacy for a new theory, Levin maintains that theory replacement is an ever more arduous activity. The greater the number of facts to be explained, the more difficult it is to explain them. So the chances of coming up with a new theory are reduced in proportion to the length of the historical succession it is to join. Every scientific revolution, suggests Levin, makes it less likely that another will occur.

This turns the skeptical inductive argument on its head; instead of inferring that present theory will be rejected from the fact that past theories were rejected, we are to infer that present theory will be retained from the fact that past theories were rejected! Presumably, were history to reveal few rejections and great longevity for current theory, then revolution should be anticipated because it would be so much easier to accomplish!

One reply to this remarkable argument which I wish to reject is a

denial of cumulativity. It is well established that successive theories typically address different problems or seek to answer different questions, in part because some of the questions a theory is responsible for answering receive their first formulation in the context of the theory itself. Perhaps no theory explains all the facts thought to be explained successfully by its predecessor. But this does not refute cumulativity. Cumulativity I take to be the thesis that facts established by theories continue to be recognized as facts from the vantage point of future science, so that once a fact has been established, its status as a fact will continue to be inferable from future theories. This thesis is unimpaired by the finding that what were formerly considered established facts are no longer so regarded, provided current theory rationalizes their rejection. Nor does appropriation of established facts by theories other than those which replace theories first establishing them impair cumulativity. Science as a whole may exhibit cumulativity if individual theory transitions do not. What would refute cumulativity are the possibilities that in the course of scientific growth, no facts are established at all, for then there is nothing to accumulate, and that purportedly established facts are neither inferable nor deniable on the basis of future theory. Examples of change in the problem situations confronting successive theories in themselves realize neither possibility.<sup>12</sup>

There are, however, more telling replies. Levin's argument suffers from an exclusive focus on the process of theorizing. As the scope and depth of theories increase, so does the sophistication of experimental procedures, both as to the precision and range of measurements and as to the ability to produce technologically new types of phenomena requiring theoretical understanding. As theories become more comprehensive, the ability to test them, and with it the likelihood of producing disconfirmations, increases. So although the conditions of adequacy for a replacement theory are more severe than those for current theory, the ultimate acceptability of current theory is less likely than was that of past theory. Certainly, Levin's argument will fail to impress a Popperian who believes that the more a theory explains, the greater its content, the easier it is to falsify.

The real issue here is the connection between disconfirmation and rejection or, more clearly but less precisely, between falsification and theory replacement. Levin presumes, as do many philosophers, that no theory, however unsatisfactory, can rationally be rejected unless a superior theory is available to replace it. If it is possible to make an adequate case against a theory in the absence of an alternative, then Levin's argument surely fails. For the difficulties of constructing a new theory may then be irrelevant to the survival of present theory. I have never under-

stood the arguments which are supposed to show that the absence of a better theory in itself guarantees the continuing acceptability of whatever theory is available. Most such arguments are nothing more than complications of historical examples which are equally well interpreted as evidence of the ability of scientists to come up with a new theory when one is wanting. Perhaps the clearest attempt at such an argument is Lakatos's, which depends on assuming that no amount of empirical or methodological difficulty (nor, for that matter, inadequacy relative to alternatives) ever legitimates the conclusion that any theory is false. On this view, so much mainstream scientific judgment becomes illegitimate that one wonders how Lakatos can make maximization of the scope of internal history a desideratum for historiography of science.

It is worth adding that if we allow that theory rejection requires theory replacement, Levin's argument is then vulnerable to the conjecture that the ability of scientists to construct new theories improves with experience. Levin's example of the cavemen with five facts to explain suggests that we are to regard the faculty for theory invention as a diachronically fixed, innate endowment incapable of evolution. But we are not simply cavemen with more than five facts to explain, who, therefore, face a tougher task. We have learned how to construct theories and how to distinguish promising from unpromising directions of scientific creativity.<sup>13</sup> Analysis of the epistemic basis of these abilities reveals the role of experience. Levin seems to have been taken in by the old view that the creative aspects of science are not a fit subject for comprehension.

Although flawed, Levin's optimistic reversal of the skeptical inductive argument is instructive. For its irony mirrors an irony of the original argument by which the core issue for a theory connecting truth with scientific progress may be grasped. I intimated that Levin is committed to the converse of his position, to the view that were history to reveal great longevity for theories and little in the way of theory replacement, then future replacement would become more likely. The reason is that were Levin to maintain that under such hypothesized conditions replacement remains unlikely, we would get the result that history is simply irrelevant to the prospects of current science. I consider this result inadmissible, for while the fate of a theory might not depend on the availability of a successor, it surely does depend on the conditions of its provenance.

As many philosophers have argued, there are dimensions of theory appraisal to which the projection of a theory against its background of methodological and metaphysical commitments and their evolution is crucial. Now, if we consider the converse of the original skeptical argument, we get the result that were past theory to remain viable and not require rejection, then current theory also should be expected to survive.



But this inference is unacceptable as we are dealing with exclusive theories among which a decision is ultimately necessary. The original argument therefore suggests that current theory is in trouble whatever the fate of past theories. *What turns out to cause epistemic problems for current theory is not so much the fact that past theories were rejected as the fact that there were past theories at all, which are inconsistent with current theory.* Current theory is unreliable because history has produced alternatives.

To put the point another way, consider the fact that a necessary condition for theory succession to approach truth is that replaced theories be wrong. For surely, if they are not wrong, then we do not approach truth by replacing them. It is partly his conviction that this condition cannot be known to be satisfied that leads Lakatos to reject any connection between progress and classical verisimilitude. Now, it is ironic that the *satisfaction* of this condition hypothesized by the original inductive argument leads to a skeptical conclusion. The argument in effect maintains that theory change does not approach truth whether or not this condition is satisfied. The real thesis appears to be that *truth is not something that can be approached*. It does not come in degrees. It must be achieved all at once in a theory that never gets abandoned, or not at all. Only such an immortal theory which obviates history can have any connection with classical truth. Why is Shapere so interested in a theory that lasts thousands of years if it is not that for such a theory history no longer counts? The vicissitudes of attempts, failures, and new attempts which prepared its introduction become prehistory or protoscience of no possible relevance to its epistemic credentials. The argument from actual history to the unreliability of current theory really argues that no theory is acceptable so long as it has a history to be reckoned with.

The only way I know to counter this view, the only way to salvage a role for truth in the analysis of scientific progress, is to take the position that there are degrees of truth, that truth can be approached. Even if no theory ever to be produced is completely true, later theories are, for the most part and on balance, truer than earlier ones. This is the view I shall seek to defend.

## II

My approach will have much in common with views advocated or once advocated by Hilary Putnam. Putnam has espoused realism with respect to scientific theories and claims to have shown that only the classical conception of truth offers an adequate account of the nature of science. I shall begin by discussing Putnam's argument.<sup>14</sup>

Putnam maintains that scientific progress, at the level of increasing predictive success, is an established fact. He offers realism as an explanation of this fact. In particular, that the purportedly referential terms of mature science refer and that the laws of mature science are approximately true are *hypotheses* which render this fact comprehensible. If we reject these realist hypotheses, the success of science becomes incomprehensible—a "miracle," says Putnam.

There is an embellishment to the effect that the acceptance of these hypotheses by scientists renders their behavior comprehensible. Scientists typically seek new theories whose laws reduce to those of established theories "in the limit" and whose terms are coreferential with those of established theories. It would be perverse of them to accept such constraints on theorizing were they not realists in the sense of these hypotheses. This embellishment is problematic, because if one asks why this would be perverse, the answer is that violation of these constraints makes it easier to meet the empirical requirements for new theories. But if this is so, why does empirical progress require realism for an explanation? Empirically successful methods free of realist constraints would themselves provide the explanation. The point of the embellishment must really be that realist constraints are, rather than a hindrance to theorizing, a help in selecting among possible theories those most likely to be empirically successful. That they have this effect is again an explanandum for the realist hypotheses.

One might attempt to counter Putnam's argument with a version of the skeptical inductive argument. We cannot explain the success of past theories by invoking the truth or reference of past theories, as we do not believe that past theories are true or referential. Therefore, it is unlikely that the success of current theory can be explained by invoking these attributes. That the realist hypotheses do explain current success is a crucial premise for Putnam, however. For the claim that nothing other than realism explains scientific success is by itself an insufficient case for realism. Whatever force this argument has against Putnam depends on its suppressed inference from the failure of truth and reference in past theories to their failure in current theory. Only this inference provides the argument's challenge to the current explanatory status of realism. So it is really the original skeptical argument that must, once again, be confronted. Putnam acknowledges this by allowing that his reasoning depends on blocking the skeptical induction. But he provides no means of doing this.<sup>15</sup> One might, therefore, simply rest the case against Putnam.

This, in effect, is what M. Hesse does.<sup>16</sup> But the matter is not quite so straightforward. Hesse appears to allow that the realist hypotheses do explain the empirical success of science but contends that the skeptical induction, which she formulates as a principle of "no privilege" for cur-

rent science, requires that some other explanation short of realism be found. Thus, the target is actually Putnam's second premise, the claim that there is no nonrealist explanation. Unfortunately, Hesse fails to produce one. What she produces instead is a "reconciliation" of her "no privilege" principle with the "principle of growth," that "science does exhibit apparent cumulative predictive success." The reconciliation consists in showing that sentences at an observational level, including some formerly theoretical sentences which made no purported references to hypothetical entities and were therefore able to shift over to the observational level, survive scientific revolutions. But what was to be explained was not simply how it is possible for science, at periods separated by revolution, to agree on some basic empirical facts—there are nearly as many ways of doing this as there are critics of incommensurability. Rather, the explanandum is the dramatically successful predictive power of theories. The explanandum, one might say, is the achievements themselves, not the conditions of their accumulation. If theorizing does not approximate truth, if the entities it postulates do not exist and their descriptions are not approximately satisfied by anything that does exist, why are its predictions so successful?

Laudan's reply to Putnam also provides no explanation. The version of the skeptical inductive argument on which Laudan relies is the claim that the inference to the truth or reference of theories from their predictive success is invalidated by history. Thus, Putnam is wrong to cite predictive success in support of realism. If the realist hypotheses really are empirical, as Putnam says they are, then they must be regarded as falsified by historical evidence. The explanatory status of the hypotheses has no role in this criticism. This is appropriate enough, as Laudan is hardly in a position to deprive any hypothesis of explanatory status on grounds of epistemic liability. But the omission results in a misrepresentation of Putnam's reasoning. Putnam does not infer the truth of realism from the predictive success of science. Rather, he infers the truth of realism from its alleged, unique ability to explain the predictive success of science. Once again, the telling criticism seems to be one that attacks realism directly on the basis of a skeptical reading of history, and then concludes indirectly either that realism fails to explain predictive success because it is false, or that the explanation it provides is unacceptable. The explanatory problem Putnam has raised remains outstanding.

The general result seems to be this: the denial of realism is compatible with a certain form of progress, perhaps even with growth of knowledge in some as yet inadequately articulated third world sense. Moreover, the form of progress that science exhibits seems an insufficient evidential basis for realism, as such progress has been achieved by discredited

theories. Nevertheless, progress seems to require realism for an explanation. Specifically, the fact that not only does science exhibit an accumulation of knowledge at the pragmatic, empirical level but theory transitions produce marked *increase* of predictive success, seems explainable only by the superiority, according to realist standards, of theories over their predecessors.<sup>17</sup>

The superiority required is not primarily referential. New theories may purport to refer to more, to fewer, or to different entities than their predecessors, but that is not a condition of their superiority. Moreover, if their replacement in turn required referential failure, an initial judgment of their referential superiority could not be sustained. What is required referentially is stability; new theories at least sometimes preserve the references of their predecessors.<sup>18</sup> Their superiority must primarily be a matter of truth; a new theory of greater predictive success, if it does not introduce new references which in turn are preserved, must more closely approximate truths about the nature of the referents of the replaced theory. Short of recognizing superiority of this sort, we must either regard a major form of scientific progress as unexplainable or we must deny that progress of this form actually occurs. The latter position leads directly to historical relativism; it is, in effect, Kuhn's view that each major theory determines its own domain of application, so that Aristotelian scientists can be credited with having learned as much or more about their world as Newtonian scientists learned about theirs.

The apparent relationship between increasing predictive success and the hypothesis of truth approximation, whereby the latter is needed to explain but is inadequately supported by the former, is typical of science itself. Galileo was unable to explain the tides because his aversion to astrology prevented him from recognizing celestial influences. Newton explained the tides by celestial influences. Of course, the phenomenon of tidal motion in itself is insufficient reason to accept Newtonian gravitation even if no other explanation seems possible. But its combination with additional otherwise unexplained and unrelated motions is compelling. When an explanans is underdetermined by but uniquely explanatory of its explanandum, one investigates its explanatory potential in other areas, particularly areas that have also proven resistant to explanation and that differ significantly from the explanandum.

The hypothesis of truth approximation has such potential, for there are established forms of scientific progress distinct from increasing predictive success. There is, most obviously, increasing predictive power, the fact that a greater number and diversity of predictions are generated by theory change, which there is certainly no a priori reason to expect to accompany or to be accompanied by increasing success (unless, with

Lakatos, one reckons success without regard to failure). More generally, there is increasing explanatory, problem-solving, and question-answering power as science develops. And there is a form of progress that consists in extension of the scope of observation, in the ability to observe newly postulated entities. Especially important is the ability to observe such entities directly, where 'direct observation' has not its philosophic sense of observation free of theoretical presuppositions, but its scientific sense of achieving the best possible access or evidential situation with respect to the entities which the theory of them allows. I shall argue that truth approximation is vital to the explanation of these phenomena as well as to the explanation of increasing predictive success, so that combination of explananda will increase the credibility of realism.

Of course, it is possible to question these alleged explananda, just as the presumption of increasing predictive success has been challenged. Moreover, while increasing predictive success may seem too manifest an accomplishment to be denied, the other forms of progress claimed for science are sufficiently abstract and complex to issue in such technical difficulties as invite skepticism as to the very claim that such progress occurs. It is important to recognize that rejection of this claim, as much as that of increasing predictive success, introduces relativism. It asserts, in effect, the symmetry of judgments as to the capacities of successive theories when such judgments are relativized to the theories themselves. Each theory fares better by its own standards, and there are no transtheoretic standards to which to appeal. Thus, A. Grünbaum contests Popper's contention that general relativity (*E*) answers more questions than the Newtonian theory of gravitation (*N*) by denying, implicitly, that there are transtheoretic criteria for the legitimacy of questions. *E* answers more Einsteinian questions; *N*, more Newtonian ones.<sup>19</sup>

Discussion of this example will serve both to defend the claim that scientific change increases problem-solving and explanatory power, and to develop the role of truth approximation in explaining these phenomena. *N* and *E* are incompatible theories. Therefore, a nonmetrical (qualitative) comparison of their Tarskian logical content or Popperian information or empirical content under the set inclusion relation will not yield the result that the transition from *N* to *E* increases verisimilitude. Nevertheless, Popper maintains that a nonmetrical comparison can sustain the intuition that *E* exceeds *N* in explanatory power, by focusing initially on questions which *E* and *N* can answer. For *E* answers, with at least equal precision, all the questions *N* answers, and more. Grünbaum produces as counterexamples a series of questions which *N* answers with precision but which cannot be formulated in *E*. Each question has a presupposition, either permissible or realized according to *N*, which contradicts *E*. And it

is clear that there is no end to the generation of such questions, precisely because *E* and *N* are incompatible. As an initial example, consider the question:

- (1) Why is the orbit of a planet of negligible mass subject to the gravitational attraction of the sun alone Keplerian?

*N* answers (1) by straightforward mathematical deduction from basic laws. *E* denies the presupposition that the orbit is Keplerian, implying to the contrary that the planet precesses.

Now despite Grünbaum's argument, one is inclined to suppose that there is an intuitive sense in which *E* does answer all the questions *N* answers and more. This is the sense in which 'question' is taken to mean "question whose presuppositions are consistent with current theory." The intuitive reply to Grünbaum, then, is that *E* answers all the questions *N* answers which satisfy this restriction, and that it is legitimate to impose this restriction. But how can the latter claim be defended? If the issue to be decided were which of the theories *E* and *N* is the better theory, and the question-answering power of *E* and *N* were being compared with a view to informing this decision, then there could be no defense. The imposition of Einsteinian standards in assessing the legitimacy of questions would be circular. But this is not the issue, at least not here. We start with the fact that *E* is better, a fact established by *E*'s comparative predictive success. The issue is how this fact is to be explained. And we find that this fact enables us to recognize a second fact also requiring explanation, that *E* exceeds *N* in question-answering power.

This reply, as it stands, will not do. From the fact that *E* exceeds *N* in predictive success, it by no means follows that *E* is superior to *N* in such respects as would justify restricting legitimate questions to those whose presuppositions conform to *E*. The very point at issue is whether *E* exhibits additional relative merits that can serve as explananda for its relative truth. This issue is prejudged by assuming *E*-presuppositions superior to *N*-presuppositions. Nevertheless, the independence of the assumption of *E*'s superiority with respect to predictive success from the circularity of the reply is relevant to the legitimacy of the intuition that *E* exceeds *N* in question-answering power. Consider the question:

- (2) Why does the perihelion of Mercury precess?

Although *N* answers (1), it can also answer (2). For *N* is not committed to the satisfaction by Mercury of the conditions hypothesized in (1). *E* also answers (2), differently of course. Thus, the presupposition of (2) would seem compatible with both *N* and *E*. Does this indicate a transtheoretic standard of legitimacy for questions? It depends on how (2) arises. Its presupposition might be deduced from each theory independently as the idealizing conditions hypothesized in (1) are corrected, so that each



theory judges (2) legitimate by its own standards. But what if the deduction is not performed or the idealizing conditions are not yet correctable, and (2) arises *empirically*? Must we then await these theoretical developments for a decision as to (2)'s legitimacy? Consider

(3) Why does the perihelion of Mercury precess by 5,650 seconds of arc per century?

Presumably the presupposition of (3) is not obtainable deductively from *N*. Yet neither does it contradict *N* in the way that the presupposition of (1) contradicts *E*. Is (3) legitimate for *N*? If so, can *N* answer (3)?

It seems to me that the answers to these questions are "yes" and "no," respectively. (3)'s legitimacy for *N* was acknowledged by proponents of *N* who therefore tried to answer (3), but failed. Their failure indicates that (3)'s presupposition is incompatible with *N*. But this does not render (3) dismissible as illegitimate, for the simple reason that (3) arose as an empirical question and is, therefore, legitimate transtheoretically. None of Grünbaum's examples are empirical questions, but if we consider such questions we destroy at once the apparent symmetry of question-answering power of competing theories. For *E* answers all the questions legitimate by its own or by transtheoretic standards that *N* answers, and more. But *N* fails to answer all the questions legitimate by its own or by transtheoretic standards that *E* answers, for it fails to answer (3). If we can assume that *N* does not exceed *E* in scope of application, so that *E*'s superiority in question-answering power is not purchased at the expense of legitimating fewer questions, then we can infer from the destruction of this symmetry that the *E* to *N* transition increases the number of questions answered.<sup>20</sup>

Now consider the following reply. *N* can answer (3). There are auxiliary hypotheses consistent with *N* that enable *N* to yield precisely the empirically determined amount of Mercury's precession. The problem is not that *N* cannot answer (3); the problem is that *N*'s answer is incorrect or unsubstantiated. What we are comparing is not predictive success but predictive power, and *N* achieves predictive power comparable with *E* at the expense of predictively unsuccessful auxiliary hypotheses. The argument for the question-answering superiority of *E* confuses the ability of a theory to answer a question with its ability to answer correctly. Very likely *E*'s answer is also incorrect.

It is to the informativeness of the failure of this reply that I have been leading. If the argument confuses the ability to answer with the ability to answer correctly, it is because that distinction is in fact confused. In a case such as

(4) What is the amount of Mercury's precession?

which both *E* and *N* can answer with precision, the distinction is clear. But in a case such as (3), which asks for an explanation of an empirical finding, the distinction is unclear. What makes it unclear is that the epistemic credibility of a purported answer influences its status as an answer. *N*'s purported answer to (3) consists in citing some auxiliary hypothesis, *A*, asserting, for example, the existence of heretofore undisclosed intra-Mercurial matter. If *A* is empirically falsified or discredited, it simply is not available to *N* for citation. The mere fact that *A* is logically consistent with the basic laws of *N* is insufficient. The ability of a theory to answer a question of type (3) amounts to its ability to explain a natural phenomenon or to solve an empirical problem. And the standards for explanation or solution preclude recourse to an independently discredited *A*. No theory is credited with achieving an explanation simply by virtue of complying with the logical canons of deduction. *N* attempted to answer (3) and some such attempts did produce deductions of the observed perturbations, but *N* was never credited with the ability to answer (3).

Questions of (3)'s type invariably require auxiliaries. The epistemic credentials of such auxiliaries can shift rapidly. Therefore, the question whether a theory can answer such a question is incomplete except as a retrospective question about a historical theory whose full career is available for review. Otherwise the question must specify a time, and the answer depends on the auxiliary information available at that time. Often the question of whether or not the presupposition of a question is compatible with a theory is only answered in the course of attempting to answer the question on the basis of that theory.

Grünbaum's discussion evades this historical dimension by encapsulating all auxiliary hypotheses involved in providing answers into the theory itself. But it is not a part of *N*, in the sense of a presupposition to which *N* is committed, that, to adapt an example of Grünbaum's, the only planets of nonnegligible gravitational influence on Uranus are Saturn and Jupiter. Otherwise *N* would be unable to answer the question of why Uranus exhibits certain perturbations, a question *N* did answer. In general, the question whether a theory can answer an empirical question remains unanswerable over significant periods in the theory's career. The important distinction is not between answers as such and correct answers, but between actual and potential answers. *N* had potential answers to (3), but none of them was realized.

For clarification of this point consider the following question:

(5) Why do Mercury's perturbations exceed the amount predicted by *N*?

Although (5) is so formulated that its presupposition explicitly conflicts with *N*, (5) is clearly legitimate for *N*. For (5) bears the same relation to *N* as

(6) Why do Uranus's perturbations exceed the amount predicted by *N*? which *N* did answer. The difference between (5) and (6) with respect to *N*'s question-answering capabilities is a difference in the confirmation of needed auxiliary hypotheses, a matter independent of *N* itself. *N* produced potential answers to both (5) and (6) but only the latter was realized.

Grünbaum generalizes his criticism of Popper by arguing that whenever the addition of an auxiliary hypothesis to a theory results in the correct postdiction of a phenomenon anomalous for the theory, no Popperian content increase can be said to occur. Let  $T_1$  and  $T_2$  be theories such that  $T_1$  predicts a phenomenon  $e_1$ , a phenomenon  $e_2$  incompatible with  $e_1$  actually occurs, and  $T_2$ , which results from the addition of an auxiliary  $A_2$  to  $T_1$ , postdicts  $e_2$ . Then the transition from  $T_1$  to  $T_2$  is not content-increasing by any Popperian nonmetrical comparison for the simple reason that  $T_1$  and  $T_2$  are mutually inconsistent. The inconsistency results from the occurrence in  $T_1$  of a hypothesis  $A_1$  inconsistent with  $A_2$ .

Now of course in its prediction of  $e_1$ ,  $T_1$  does invoke some such  $A_1$ . But this does not make  $A_1$  an ingredient of  $T_1$  such that any theory denying  $A_1$  is a distinct theory inconsistent with  $T_1$ . A theory does not include, in the sense of part whose rejection implies rejection of the theory, any and all auxiliaries it uses in deducing empirical results. In fact,  $T_2$  would not be considered a rival of  $T_1$  at all, but a development of it. The original  $T_1$  is not rejected in the transition to  $T_2$ , but salvaged by the introduction of  $A_2$ .  $A_1$  was assumed implicitly by  $T_1$ , and this assumption was shown to be both incorrect and unnecessary for  $T_1$ . Thus,  $T_1$  and  $T_2$ , insofar as they are distinguishable as theories at all, are certainly not mutually inconsistent. Grünbaum chides Popper for the naiveté of imagining the introduction of  $A_2$  to be a "mere conjunctive appending" of  $A_2$  to  $T_1$ , suggestive of increase in content. In fact, this operation alone would render  $T_2$  inconsistent if  $A_1$  were regarded as an ingredient of  $T_1$ . Undoubtedly, Popper did not so regard  $A_1$ , and in this he was surely justified. The status of  $A_1$  in  $T_2$  is different;  $A_2$  does become an essential ingredient of  $T_2$ , given the empirical requirement that  $e_2$  be postdicted. Thus, the contraction hypothesis did become an essential ingredient of the Lorentz electron theory, while its implicit denial in early versions of the theory was shown never to have been essential. Had it been, the theory could never have been reconciled with Michelson's result on pain of inconsistency. A content-increasing conjunctive appending seems to be exactly

what occurred. Grünbaum's example of the introduction of Neptune is subject to a similar analysis, except that the Neptune hypothesis is more empirical and less theory-dependent than contraction, and so retains an auxiliary status.

Clarification of the nature of the operation by which auxiliaries are introduced to account for anomalies is of further importance in assessing Popper's use of truth and falsity content comparisons in his theory of verisimilitude. For that assessment has produced technical results whose significance is marred by the implausible rigidity of portraying theories as Tarskian deductive systems which set theoretically include all empirical predictions by which they are tested. For example, certain predictions of *E* do not, as is well known, accord exactly with observation. Corresponding predictions of *N* are in far greater disparity with observation. Thus, *N*'s false predictions differ from *E*'s false predictions. From this fact alone it immediately appears, without appeal to the usual technical apparatus, that the falsity content of *E* cannot be set-theoretically contained, properly or improperly, in that of *N*, violating one of Popper's conditions for attributing greater verisimilitude to *E* over *N*.<sup>21</sup> And, indeed, any incompatible theories yielding different predictions for a given observation will be incomparable as to (qualitative) Popperian verisimilitude unless one of them is confirmed. The difficulty with this apparently destructive result is that the appropriateness of including *E*'s incorrect prediction in its falsity content class is a very complicated matter. For that prediction might well be corrected through changes of auxiliary information, leaving *E* itself unimpaired. The above reasoning precludes this possibility on pain of rendering *E* inconsistent.

Increasing explanatory and problem-solving power, as well as increasing predictive success, are properly regarded as forms of progress in science. Increase in relative truth of scientific hypotheses is the required explanans for all. This is not to say that relative truth is explicated by Popperian verisimilitude. If a theory itself is false, as opposed to yielding correctable, false predictions, then a trivial version of the above argument applies unambiguously to preclude its exceeding any other theory at least in qualitative Popperian verisimilitude. Content increase simply cannot be restricted to set-theoretic comparisons. Nor is it to say that all intuitive content increases exhibit these attributes. The Lorentz case is a counterexample, because increasing predictive success is a necessary condition for increases in the other attributes. A theory cannot take credit for an explanation or for a problem's solution, excepting problems that are purely pragmatic, unless the hypotheses in terms of which it purports to offer an explanation or solution are corroborated.

Scientists simply do not regard a problem as solved or a phenomenon

as explained unless they believe that the epistemic situation entitles them to be confident that there is some truth to such hypotheses, that they are at least "partly right" or are "headed in the right direction." Insofar as the hypotheses are tentative, the task they address remains open. Insofar as conceptual problems are taken to require that they be treated instrumentally, only "systematization of experience" or solution of pragmatic problems, which amounts to predictive success alone, will be claimed as an achievement. The philosophical significance of this attitude is its reflection of the reluctance to accept the reality of phenomena that cannot be explained.

The further form of progress which I have claimed for science, its extension of the scope of observation, also requires the truthlikeness of theoretical hypotheses for its explanation. For theoretical entities are "directly observed" only if an approximately true theory precludes better evidence for their existence than that acquired in observing them. There is no sense to current talk of observations of elementary particles, for example, if no truth is imputed to those of their attributes that preclude less inferential access to them. The claim is not that there is no sense to talk of observing entities that are treated instrumentally; such talk could have an instrumental sense. The claim is that there would be no sense to the distinction drawn in science between direct and indirect observation if the entities said to be observed are treated instrumentally. But advance in the ability to observe directly, or advance from mere detection to observation, is an established form of progress in which the boundary of the observable shifts to encompass more phenomena. If there is no truth to theories postulating new entities, the progress apparent in the satisfaction of the criteria such theories fix for the observation of these entities is illusory.<sup>22</sup>

### III

The case for increasing truth of scientific theories comes to this. There are these choices: (1) We can deny that science does exhibit what are normally taken to be its most manifest forms of progress; (2) we can deny the ultimate intelligibility of the world; (3) we can offer a realist explanation of progressive scientific change. (1) has the disadvantage of being at least as much in conflict with our experience as is realism with the historical record of theory change. (2) is always available. Skepticism as such remains a tenable epistemological position. But we must see the rejection of realism in this light. If we are prepared to deny that there is any truth to our most successful theories on the grounds that it is possible for a false theory to be successful, we might as well deny that there is an exter-

nal world on the grounds that it is possible for a brain in a vat to have external-worldish experiences.

This is not to admit that the case for realism founders on skepticism. The argument I have given is of a form that may be parlayed into an attack on skepticism. It does not directly claim that predictive success of theoretical hypotheses attests to their truth. Rather, it claims that increases of predictive and explanatory success and of the scope of observation constitute explananda for a realist interpretation of science. Thus, the argument is indirect and hypothetical, as transcendental arguments must be. These features are double-edged. They enable the argument to meet the challenge to realism which the possibility of predictively successful, false theories poses. But they subject it to the inconclusiveness that plagues transcendental arguments generally.

The inclusiveness of the argument results partly from the continuing viability of (1) and (2), which some will find no more problematic than (3). (1) may be defended, for example, by arguing that extension of the scope of observation presents an explanans which itself is described in realist language, so that it is unsurprising that a realist explanans is forced upon us. It is possible to maintain that while the boundaries of the observable shift, theories retain "deep structures" which observation never penetrates, and which, accordingly, resist at least this motivation for a realist interpretation. It is possible to dismiss scientific talk of observation of theoretical entities altogether as a *façon de parler*, to reject the scientific distinction between experimental procedures that support hypotheses introducing theoretical entities and experimental procedures that constitute modes of observing such entities as a distinction without a difference. One explanandum for (3) then collapses.

I hope to have shown, however, that the others are free of the potential circularity of a realist explanation of phenomena described in realist terms. For if my argument against Grünbaum is right, realism is not required for *recognition* of advances in predictive, explanatory, or problem-solving power through scientific change. And if, as I have further argued, these explananda are persuasive on behalf of (3), it is at the very least an attractive fringe benefit of (3) that the scientific concept of observation is underwritten.

A further inconclusiveness results from the inability of a transcendental argument to establish its conclusion uniquely. I have sought to forestall this objection by offering an analysis of certain forms of scientific progress that requires the imputation of truth to hypotheses effecting such progress. Inasmuch as alternative analyses are possible,<sup>23</sup> it may be said that the impossibility of a nonrealist explanation of scientific progress has not been established.

There are, however, further advantages to (3) which any alternative

short of (1) or (2) would be hard pressed to match. My argument for (3) may be applied, derivatively, at the level of individual theories. Realism with respect to individual theories is confirmed by its production of expectations beyond the explanatory function it is designed to serve, which in turn are satisfied. And this it does, for a successful theory characteristically provides explanations of phenomena and solutions of problems which emerge only as the theory is developed and applied in new areas, which are unanticipated in its initial formulation. How, short of (3), do we account for the ability of theories to explain and predict successfully phenomena outside the scope of the empirical laws they were designed to yield? This query answers Laudan's as to why we should denigrate ad hoc problem-solving devices.<sup>24</sup> The fact that 'ad hocness' is unfailingly pejorative is symptomatic of a realist attitude toward theories. Without realism there would be no rationalizing this attitude. We should welcome the introduction of new hypotheses, however contrived, which accommodate unanticipated, adverse evidence, rather than insist that solutions issue in a natural way from the original theory. Insofar as we need to add special hypotheses for emergent problems, a realist interpretation of the original theory is disconfirmed.

Still, it will be argued, it is not just the possibility of successful false theories with which realism must contend, but the fact of them. Is this any worse? Presumably so, for it shows that realism as a philosophical interpretation of science is not simply underdetermined but is refuted by the history of science. I wish to suggest, however, that history is not univocally opposed to realism any more than our experience of ordinary objects is univocally veridical. The difference in our epistemic stances with respect to scientific realism and other explanatory metaphysical doctrines is one of degree. For, apart from the moot issue of conceptual continuity through revolutions, one historical pattern that has remained stable throughout scientific change is the tenacity of preferential judgments about theories. Although a theory that replaces another is in turn replaced, its superiority over its predecessor continues to be recognized. As much as history records sustained judgments of the ultimate unacceptability of theories, it records sustained judgments of their relative merits.

Such judgments are not restricted to the pragmatic dimension of predictive success, but include explanatory comparisons. Newton provided a better explanation of free-fall than did Galileo although both explanations have been superseded. If we retain such judgments beyond the tenure of the theories themselves, we must regard one theory as having got more of the relevant facts right or as having described those facts more accurately, even if both theories are false. If the explanations proposed by both theories were rejected as utterly devoid of truth, such comparisons would be impossible. There seem to be as good inductive

grounds for concluding that scientific theories increase in truth as for concluding that all theories are false.<sup>25</sup>

These conclusions are, moreover, consistent, if 'false' is taken to mean 'incompletely true'. Bivalence then requires distribution of truth values among logically independent components of theories. The above considerations regarding the status of auxiliary hypotheses and the technical difficulty of explicating truth approximation by set-theoretic comparison of truth and falsity contents of theories lead me to prefer a multivalued logic. In any case, multivalued logic is appropriate if the comparisons we wish to sustain pertain to degrees of accuracy. This seems a natural interpretation for statements of empirical science whose success in describing objective features of the world is not, intuitively, an all-or-nothing affair. It is, after all, quite common to compare descriptions, either verbal or pictorial, of a physical object as to accuracy without imagining that there is or could be such a thing as a maximally accurate description. Such a view does not require that there be an ultimate, absolute truth about any given aspect of the world, relative proximity to which explicates comparative truth assessments. In ethics and practical reasoning, we make comparative assessments of value without reference to an ideal state. It does not even require that truth be a relation between bearers of truth and the world, although this is the natural view if bearers of truth are taken to be statements. But it does require the independence of relative truth from the vicissitudes of justification and belief. For if the progressiveness of one theory over another is to be explained by its exceeding the other in truth value, then truth-value comparisons cannot be analyzed as epistemic comparisons.

Perhaps, however, the best way to block the skeptical induction is not to develop such a view but to resort to the philosophical ploy of self-reference which applies convincingly against verificationism and historical relativism and should, inductively, work again. For example: all philosophical theses of the past have turned out unacceptable (for who would claim as much progress for philosophy as for science at even its most theoretical level?). The claim that present theory is discredited by past theory is a philosophical thesis. Therefore...

## NOTES

1. This is a strong version of the argument. If we suppose only that past theories have excelled by the standards of the time, we get a weaker version which invites recourse to a metalevel appraisal of the standards of different historical periods. Then we need metalevel standards and a meta-metalevel appraisal, and so forth; so we might as well stop with the formulation given.

2. I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), 265.

3. *Ibid.*, 189.

4. Lakatos distinguishes two senses of 'verisimilitude', Popper's technical sense and "intuitive truthlikeness," assimilating "classical verisimilitude" to the latter. The reader unwilling to believe that Lakatos could be guilty of so elementary a misunderstanding is encouraged to seek an alternative interpretation of *ibid.*, 189. See also *ibid.*, 265.

5. Cf. I. Hacking's review of Lakatos's *Collected Papers*, *British Journal for the Philosophy of Science*, vol. 30, no. 4.

6. *Criticism and the Growth of Knowledge*, 187.

7. L. Laudan, *Progress and its Problems* (Berkeley, Los Angeles, London: University of California Press, 1977).

8. *Ibid.*, 12.

9. More precisely, Professor Laudan's view is that while neither truth nor falsity is determinable in principle, falsity but not truth can be estimated. A theory for which severe anomalies resist great effort at resolution is likely to be false, but no likelihood of truth can be imputed to a theory however impressive its achievements. Thus, Laudan is in a position to endorse a form of the skeptical, historical induction.

The claim underlying this view may be that the approximate falsity of a theory can be expected to issue in problem-solving failure, but approximate truth is no reason to expect problem-solving success. The grounds for such asymmetry, however, are unclear. It is insufficient to argue that extant philosophical analyses of approximate truth fail to support an inference to empirical success; neither do accounts of approximate falsity support an inference to empirical failure. Moreover, Laudan has shown in detail that false theories may be empirically successful (see his contribution to this volume).

On the other hand, the claim may be only the converse, that failure betokens falsehood. But according to Laudan, any degree of verisimilitude short of rigorous truth cannot be expected to issue in empirical success. Thus, a theory that fails may well be (very nearly) true.

10. D. Shapere, "The Character of Scientific Change," in *Scientific Discovery, Logic, and Rationality*, ed. T. Nickles, BPS no. 56 (Dordrecht: D. Reidel, 1980), 61-102.

11. M. Levin, "On Theory-Change and Meaning-Change," *Philosophy of Science* 46, 3: 407-425.

12. There is, widespread among scientists at least, a notion of cumulativeness very different from mine according to which theories retain their predecessors as limiting cases. This view, on every coherent version I know of, is refuted by the history of science.

13. See J. Leplin, "The Role of Models in Theory Construction," in *Scientific Discovery, Logic, and Rationality*, 267-283.

14. See Putnam's essay in this volume.

15. The "Principle of Charity" offered to secure the reference of past theories does nothing for their truth.

16. M. Hesse, "Truth and the Growth of Scientific Knowledge," *PSA* 1976, vol. 2, ed. F. Suppe and P. D. Asquith, pp. 261-281.

17. This is not to suppose as Putnam seems to, that just any form of empirical success requires realism for an explanation. There is no mystery, for example, about the ability of hypotheses proposed with the express purpose of providing a theoretical basis for antecedently established empirical laws to yield those laws, whether or not the hypotheses are interpreted realistically. For discussion of the forms of success requiring realist explanations, see J. Leplin, "The Historical Objection to Scientific Realism," *PSA* 1982, ed. T. Nichols and P. Asquith, pp. 88-98, and "Novel Prediction," in manuscript.

18. That this requirement is satisfied is argued in J. Leplin, "Reference and Scientific Realism," *Studies in History and Philosophy of Science*, 10, 4: 265-285, which however, presupposes in opposition to positivism that evidence for a theory is evidence for the theory as a whole, not just for its observational portions.

19. A. Grünbaum, "Can A Theory Answer More Questions than One of Its Rivals?" *British Journal for the Philosophy of Science*, 27: 1-23.

20. Professor Laudan points out that in the general case such an assumption will be very problematic. Changes of ontology, for example, will shift radically the ranges of questions to be addressed. And a shift that reduces the number of questions answered may well be progressive if achieved through ontological economy.

21. This elementary observation is amply illustrated by using Popper's familiar example of comparing false estimates of clock-time. Let the clock read 9:48 and let the interval estimates exclude upper bounds. Then the estimate "it is between 9:45 and 9:48" fails to exceed the estimate "it is between 9:40 and 9:48" in Popperian verisimilitude since, for example, it implies the falsehood "it is between 9:44 and 9:48" which is not implied by the latter.

22. For further argument, see J. Leplin, "The Assessment of Auxiliary Hypotheses," *British Journal for the Philosophy of Science* 33 (1982): 235-249, and "Methodological Realism and Scientific Rationality," forthcoming in *Philosophy of Science*.

The truthlikeness of theoretical hypotheses, has, arguably, further explananda which I neglect as more controversial and less obviously forms of progress, for example, conceptual continuity through revolutionary changes in mature science.

23. It is noteworthy that such a nonrealist as Laudan, who does recognize explanatory or problem-solving advances through theory change, offers no analysis of what constitutes a solution to a scientific problem beyond requiring a deductive relationship between a theory and a statement of the problem. Rather, he defers to what scientists regard as solutions. Cf. *Progress and its Problems*, 22.

24. *Ibid.*, 115.

25. There are cases of theories that continue to be regarded as explanatory without apparent continuing imputation of truth to the explanans. In such cases, the explanandum also is rejected. The problem is of the theory's own making (or is the creation of a predecessor) and subsequent theory dissolves rather than solves it. Explanatory comparisons among such cases are to be excluded from the indicated induction.