LAKATOS'S EPISTEMIC ASPIRATIONS

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ABSTRACT: Imre Lakatos argued that a theory of scientific method must be empirical, and therefore self-applicable; the standards it imposes on scientific theories must be ones it satisfies itself. But in relying on this standard of self-referential consistency to protect his theory from criticism, Lakatos becomes vulnerable to relativism. He escapes by hypothesizing that scientific changes which are methodologically progressive according to his theory are also progressive epistemically. The question is whether his theory of method has the resources to warrant this hypothesis. I construct a line of argument logically open to him, and use its inevitable failure to show that his epistemic aspirations depend on precepts of method that he has wrongly rejected.

Keywords: self-referential consistency, methodology, relativism, epistemic progress, epistemic aspirations.

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1. A Self-referentially Consistent Methodology

Imre Lakatos conceived his central contribution to the philosophy of science to be the first empirical theory of scientific progress, grounded in and responsible to the lessons of history. These lessons, he argued, discredited all extant accounts of the methods of theory appraisal but his own. In particular, scientific theories are not epistemically justified by the success of their empirical predictions, for highly successful theories come to be rejected. This is the Newtonian experience, from which naively justificationist methodologies cannot recover. Nor are theories falsified by the failure of their predictions. Every theory has such failures, and some theories, like Newton's, turn them into striking confirmations. The epistemic significance of predictive success or failure can only be reckoned by incorporating into the analysis complex additional factors accessible only with hindsight, and it is these factors rather than any confrontation of theory and experiment as such, on which the verdict principally depends.

The additional factors that Lakatos emphasized pertain to a theory's historical position in a larger theoretical context, or "research program", governed by its own metaphysical assumptions, developed in accordance with its own heuristic guidelines, and insulated from criticism by a strategy for deflecting negative empirical results away from its central commitments and onto dispensable or revisable auxiliary assumptions used to apply it to particular problems (Lakatos 1970). It is the relative progressiveness of research programs to which evaluative method is ultimately directed. A program's progressiveness depends only on the delivery of fresh empirical consequences by the successive theories in it, and the conformity of these consequences to empirical results uncontroversially endorsed by the relevant scientific community. So long as a research program fares better on these measures than rival programs, the continuing presence of empirical failure within it does not count. Predictive profligacy is the principal desideratum; a program that yields lots of new predictions most of which fail but some of which succeed rates higher than one with fewer predictive successes, however lower its failure rate.

Lakatos claimed that this theory of method, the "Methodology of Scientific Research Programs" (MSRP), captures the actual evaluative practice of scientists better than rival theories of method. Successful applications to scientific practice constitute predictive successes for MSRP. Lakatos inferred that only the development of a new theory of method that captured this practice better still could jeopardize (MSRP). Only a theory of method with more excess corroborated empirical content than MSRP could challenge it. MSRP is not jeopardized simply by inapplicability to or incompatibility with particular scientific episodes, however important, for by its own lights simple falsifications do not count. "*There is no falsification before the emergence of a better theory*" (1970, p. 35; italics in original).

Lakatos thus applied his methodology to itself, and pronounced it self-referentially consistent (Lakatos 1971). It is, he maintained, the first methodology to possess this virtue. The theory that theories are justified by predictive success must, if correct, be unjustified, for it boasts no predictive success. The theory that theories are to be rejected if faced with predictive failure must, if correct, be rejected, because it faces predictive failure. Any theory, he might have added, whose correctness thus precludes its own acceptability is unacceptable.

Lakatos might also have advanced this argument against Karl Popper's theory that to be acceptable methodologically the proposal of a theory must be accompanied by a specification of potentially obtainable empirical conditions under which the theory would be withdrawn: Popper's theory must be withdrawn, for its proposal was accompanied by no such specification. Why didn't he?

The reason is that Lakatos thought there actually are empirical conditions that Popper could have cited as a test of his theory. Popper could have said that if the consensus of scientific judgment in the appraisal of theories does not conform to his requirement that potentially falsifying conditions be specified, then his theory, rather than scientific judgment, is to be faulted. He could, in other words, have given his methodology an empirical rather than an a priori status. But, significantly, it is not Popper's failure to do this with which Lakatos took issue. Lakatos did not reject Popper's theory for violation of the principle of self-referential consistency. Instead, Lakatos relied on the fact that methodologically acceptable theories are proposed without specification of falsifying conditions, and that their advocates are characteristically unwilling to enter into such commitments once their theories gain momentum, Newtonian theory being the prime example (1971, p. 125; 1974, p. 147)[1]. He relied, that is, on the observation that had Popper been willing to submit his theory to empirical test, which he quite explicitly was not, the facts would not have borne him out. Lakatos thereby applied a naive falsificationist standard of appraisal to a more sophisticated falsificationist theory of method, in contravention of his own standard of self-referential consistency. This misstep is telling. He was, I believe, right to take it, although, if so, much of his philosophy is wrong.

There are any number of apparent counterexamples to Lakatos's methodology. The actual history of science features crucial experiments, for example. According to Lakatos, any experimental decision between competing theories depends on subsequent theoretical developments whereby one program sustains greater progress than another. No experiment can be "crucial", except with long hindsight[2]. But in the 1960's measurements by radio telescopes of the reduction in the density of the distribution of galaxies with distance provided a crucial test between big-bang and steady-state cosmologies. It did not require hindsight to interpret the decrease in density as definitive against the steady-state theory.

Again, the fact that the fracture patterns in quartz deposits found with concentrations of iridium, a metal common in extraterrestrial objects, are characteristic of impact rather than heat is a crucial test between Alvarez's theory and the volcanic theory of the extinction of dinosaurs. This discovery of the mid 1980's quickly refuted the hypothesis that widespread volcanic eruptions, rather than a single impact, produced the climatic changes responsible for the extinction of many life forms some 65 million years ago.

Lakatos can of course cite cases in which an experiment's status as crucial required the perspective of a future, successful program -the Michelson-Morley experiment, famously- but there are any number of examples in which the theoretical landscape is sufficiently prepared in advance to allow a single new result to be decisive.

Contrary to MSRP, theories sometimes are rejected for empirical failure despite the unavailability of a theory that does any better. The Bohr-Kramers-Slater theory was rejected well before a theory implementing strict conservation of energy in beta-decay was available, as Lakatos himself acknowledged (1970, p. 82). Instead of adhering to an empirically discredited theory for want of an alternative, scientists often say that they have *no* theory to explain a recalcitrant phenomenon. There is "no" theory today to explain the "great wall of galaxies" that disrupts the expected large-scale homogeneity of a universe originating in a big bang. Nor is there a theory to explain the high orbital velocity of stars at the outer edges of the Milky Way. But of course there are theoretical mechanisms capable of producing these effects. It is just that these mechanisms are discounted for failure to conform to well established background information. It is simply false that scientists will always prefer a bad theory to none.

Let us consider further the move by which Lakatos dismisses such problems. Research programs are not abandoned for empirical failure, but only in favor of other programs offering greater excess corroborated empirical content. Since MSRP is such a program, only another theory of method with greater content can overthrow it. Empirical objections like those I have given are beside the point.

But perhaps they are not entirely beside the point. Officially, empirical objections are not supposed to affect the evaluation of a program, but since admirable programs in the history of science have been replete with them it is only to be expected that MSRP, as an admirable program in the science of historiography, will face empirical objections itself. MSRP predicts that all programs, itself included, will perennially face empirical problems. So perhaps its own problems constitute predictive successes. Perhaps what were advanced as objections to MSRP instead deserve to be regarded as confirmation.

There are intimations to this effect in Lakatos; the historian, he says, ought not to expect all science to conform to a developing theory of method anymore than the scientist expects all of nature to conform to a developing scientific theory. For this reason, the boundary between internal science, which a theory of method is responsible for getting right, and external science, which, as the product of the irrational side of man, will violate the best theory of method, keeps changing. Empirical problems facing MSRP may simply be relegated to external science until such time as MSRP comes up with the resources to handle them, perhaps forever (1971, p. 134).

But this analogy is difficult to take seriously. The difference between internal and external history hardly corresponds to any difference between corroborative and anomalous empirical results. The predictive failures that methodologically pristine scientific theories are supposed perpetually to face are not fairly represented as illegitimate or unscientific intrusions upon scientific practice; they are merely disenfranchised in the calculus of appraisal. Were a theory to predict everything correctly, this would surely be to its credit, and were the entire history of science subsumable unproblematically under MSRP, this would surely be taken as vindication of MSRP's promise to be the first empirical theory of method. Lakatos cannot have it both ways.

Whatever the status of empirical anomalies, MSRP claims license to persevere in the face of them, so long as no more progressive program is on offer. This presumptively unmet condition for the overthrow of MSRP presupposes, of course, that MSRP is true. Any application of the test of self- referential consistency operates by supposing that the theory is true in its account of what a verdict as to its truth should be based upon. But the prospective rival methodology for which Lakatos is hypothetically prepared to abandon his own will presumably have *its own* idea of what the verdict should be based upon, an idea presumably different from Lakatos's. The conditions under which Lakatos will abandon his theory for it may not be conditions that favor it by its own lights. And conditions that do favor it by its own lights may be ones that Lakatos feels free to dismiss. If a new theory is to be preferred to MSRP, are not its standards rather than MSRP's the better ones to use in judging theories? But if so, perhaps the new theory is not preferable after all. How can the shift from one methodology to another be pronounced progressive if each methodology is free to presuppose its own standard of progress?

Lakatos repeatedly advertised his methodology as the only one ultimately capable of resisting relativism (1970, 1971, 1974). The conventionalism in Popper's theory invites relativism, for example. But Lakatos's only answer to the obviously relativistic implications of the test of self-referential consistency was to plead that in the ascent up the levels of standards "one must always stop somewhere" (1974, p. 153). In granting this, one wonders why it is not best to stop right at the beginning and insist that the predictions of an acceptable theory conform to experience, as Lakatos himself did, albeit inadvertently, in rejecting the Popperian standard that the theorist specify conditions under which he would abandon his theory.

2. *The Epistemic Conjecture*

I shall try to defend this suggestion indirectly, by discussing the problem of induction, as Lakatos reconceives it. For Lakatos, what this problem comes down to is how to infuse epistemological significance into a scientific practice that conforms to MSRP. Supposing that science proceeds as Lakatos says it should, what reason is there to think that the epistemic goals of science are thereby achieved or advanced?

The epistemic goal of science is, of course, knowledge; that is, the point of doing science is to learn more about the nature of reality, to uncover the "blueprint of the universe" (1974, p. 155). But MSRP was not constructed on the basis of information as to how such knowledge is best obtained. Lakatos does not claim to have such information, and the Popperian tradition with which he identifies, which he seeks to extend and improve rather than supplant, denies the very possibility of such information. Instead, MSRP was developed with a view to capturing scientific practice at its best; that is, at its most successful by the appraisal of the scientific community. MSRP was developed so as to maximize the amount of this practice that comes out rational under historical scrutiny, so that, for example, what turns out to be false science does not automatically turn out to have been bad science. Now supposing, for the sake of argument, that MSRP succeeds in this regard, why expect that science practiced to its specifications progresses toward truth? What makes practice that is good methodologically good epistemologically?

Lakatos could have answered "nothing". He could have remained this Popperian and still claimed to have improved significantly on Popper with his more sophisticated, self-referentially coherent account of method. But he didn't. He opted for a "whiff of inductivism" (1974, p. 159), that would endow MSRP with epistemic importance. Without inductivism, he lamented, science even as he would have it practiced is but a "game" (1974, pp. 160-165); "'progress' or 'degeneration' (...) remain mere honorific titles awarded in a pure game" (1974, p. 165). Rationality is assessable within the game, by reference to its rules, but there is no basis for pronouncing as to the rationality of the game itself; there is nothing but taste to recommend playing it. And this is to reestablish with a vengeance the relativism that it was supposed to be the special achievement of MSRP to defeat.

So Lakatos instead proposed an epistemic angle. He offered the "conjecture" that science practiced as per MSRP delivers theoretical truth. He admitted that this conjecture is metaphysical and fallible, selflessly stressing these unfortunate properties with the alacrity of one who hopes to recommend his position through modesty, and the self-righteousness of one whose official position conveniently legitimates them. In support of the conjecture he has to say only that it is no worse off than the unavoidably conventional elements of Popper's position, such as the basic statements that arbitrate only in virtue of happening to be accepted by a self-identifying scientific elite.

This is not much of an argument, and it is not difficult to see Lakatos's problem. A methodology based on scientific decision-making serves epistemic ends only if scientific decisions are themselves epistemic, and correctly so. If scientists go wrong in attempting to pick true theories, or if they pick theories for nonepistemic reasons, of which there are surely many that matter in science, then there is no reason to suppose that a methodology capturing scientific rationality hooks up to epistemic goals. But not only is Lakatos unable to demonstrate that theory choice is even *intended* to be epistemic, let alone that it is epistemically successful; the actual scientific judgments that MSRP is recommended for rendering rational are expressly contrary to his purpose. It is such facts as that, e.g., phlogiston theory, *although false*, comes out progressive, rational science on MSRP, that recommend MSRP over earlier methodologies whose conditions of acceptability could not distinguish rejected science from pseudoscience (1974, p. 152). And manifestly, the attribute of securing rationality under conditions of acknowledged falsehood discredits the conjecture that the methodology possessing it advances to truth.

There was, however, an obvious line of argument open to Lakatos. Why did he not attempt to defend his epistemic conjecture systematically, by applying his own standard of self-referential consistency? The natural question is whether the addition of the epistemic conjecture to MSRP constitutes a progressive shift. Does the addition offer excess empirical content over MSRP alone? To the extent that he thought of this at all, it was to conclude in the negative, with the implication that if he were to pursue the point he would be bound by his own standard to reject the conjecture as a "degenerating problemshift". And to the extent that the positive heuristic of MSRP rationalizes the introduction of the conjecture through its promise to explicate progress, this degeneration faults MSRP itself. So he suppressed the natural line of defense for his conjecture, which, according to MSRP, is a perfectly acceptable move and prudence itself, provided no one notices[3].

3. Lakatos Reconstructed

Having noticed, we might be able to salvage something from this line of argument, if we may presume to amend MSRP a bit. The first step is to be more exacting than Lakatos was about the nature of the excess corroborated empirical content that betokens a research program's progressiveness. Lakatos required that the program have novel success, that it correctly predict novel results. In this, Lakatos is distinctive among philosophers of science of his period. He attached greater importance to novelty and gave it a more prominent position in his theory of science than anyone else. He valued it, for example, over unifying explanations of previously predicted results, a preference that is persistently controversial[4]. In light of this emphasis, it is remarkable how loose and inconsistent he was in his characterization of novelty.

He meant by it that each theory in the research program yield predictions not obtainable from its predecessor, and, if empirical as well as theoretical progress is to be assessed, that these predictions prove correct[5]. He reasoned that if the predecessor did not predict the result, then its success would have to redound to the credit of the latest theory, so that the addition of this theory to the program would constitute progress. But he did not consider what would happen if some theory of a rival program predicted the result. Why then should the result be probative for the former theory?

Of course, if a novel result is entirely *new* -unknown to the scientific world prior to its prediction by the program for which it is novel- then it cannot also be (known to be) predicted by a rival program, at least not immediately. But this is a completely implausible restriction on the range of probative results, and Lakatos himself violated it. He gave mixed messages as to whether a novel result need be new in the temporal sense, sometimes citing previously known results and results obtainable from different programs as novel and sometimes relying on the presumed temporal newness of novelty to obviate questions about any formative role a result might have in generating a theory that predicts it. For example, he contrasts newly predicted "old facts" with "genuinely novel facts" (1970, p. 70), but he cites the precession of Mercury's perihelion and the Balmer series as genuinely novel facts for the later theories that first predicted them successfully (1970, pp. 32-34; see Leplin 1997, pp. 41-43 for interpretation of Lakatos's conception of novelty). He was silent altogether as to *how* a novel result comes to be predicted, as to its relationship to the theory that predicts it. He spoke only to the relationship of a novel result to previous theories of the program that had *failed* to predict it. These, in Popperian fashion, should render it unlikely or surprising. Thus he had no machinery to distinguish results that would not be expected to be corroborated unless the theory predicting them were on the right track epistemically, from results that would be expected in any case because of their relationship to - their role in the provenance of- the theory predicting them.

Rather than address such issues, Lakatos was happy simply to let any result newly predicted by a research program be novel, and to let novel results be probative for several research programs at once, contributing to the progressiveness of each program but canceling out one another's effects in the comparison of programs. The resulting calculus of appraisal is cumbersome, unintuitive, and, judged against Lakatos's epistemic ends, counterproductive. For it presumes that judgments of progressiveness are relativized to rival programs, and this invites the worry that the rivals available for comparison might not include the one or ones that actually promote our epistemic ends. Epistemically speaking, the best of the lot might not be any good. The epistemic benefit of merely relative progressiveness is difficult to sustain, as a legion of anti-realist advocates of the underdetermination of theories will not cease to remind us (See Leplin 1997, chapter 6).

To address these concerns, I propose to let a result predicted by a theory be novel for the theory in the probative sense Lakatos wants only if *no* alternative theory, belonging to the same research program or any other, predicts it; indeed, only if there is no basis for predicting it other than the particular theory for which it is novel. This will ensure that any probative weight the result achieves is a distinctive advantage of the theory that predicts it, or of the program to which this theory belongs. This restriction has nothing to do with temporal newness, however. A novel result may be one that has posed an explanatory challenge since long before the advent of the program that first predicts it. Secondly, I will require that the result not be presupposed in the development of the theory for which it is to be novel. This is not a matter of whether the result is know to the theorist or whether an interest in explaining it motivates the theory. Rather, the provenance of the theory must not have relied on the result, or on any more general assumption from which the result could have been obtained, in such a way that the theory would be expected to yield the result however it fared under testing.

These two constraints, respectively a *uniqueness* condition and an *independence* condition, in making novelty much more difficult to achieve than Lakatos envisioned, enhance the prospect for its epistemic importance. For they disqualify the evident explanations of how a theory or research program can come to attain a sustained record of successful novel prediction if it does not advance us epistemically. (This is argued at length in Leplin 1997).

Now consider a research program that is progressive in that its subsequent theories yield results that are novel for them, in the sense of the uniqueness and independence conditions, and are borne out by observation and experiment. Let us entertain the conjecture that this program is epistemically progressive. Our question is what further empirical content this conjecture imparts. Let us be clear that the question is not whether the program's novel success argues for its truth, approximate truth, partial truth, progress toward truth, or whatever the epistemic goal becomes under analysis. This question Lakatos, having rejected justificationist theories of method, must answer in the negative. Our question is the second-order question of whether a contribution to an analysis of progress (a Lakatosian analysis, let us presume it still to be) constitutes an improvement in this analysis by the standards that the analysis itself imposes for a diagnosis of progress. Does the conjecture that the program is epistemically progressive as well as empirically progressive yield novel predictions unobtainable without it, and, if so, are they successful?

It does and, we may stipulate, they are. The conjecture carries the expectation -it permits the prediction- that the program will continue to be empirically successful in any further novel predictions that it produces. To the extent that the program captures the blueprint of nature, correctly identifies the mechanisms responsible for the phenomena by which it is tested, we predict that its further predictions will be borne out. Assume they are, so that the prediction that future predictions succeed succeeds. Without the epistemic conjecture we have no basis for this successful prediction. Therefore the prediction satisfies the uniqueness condition for novelty. The prediction is not in any way assumed in developing the program that produces it. It is only a conjecture about the program invited by its empirical success, not any facts about the content of the program, that produces the prediction. Of course the content of the program produces its empirical predictions, but it does not predict their success. This is a second-order prediction that depends on the epistemic conjecture that the program's content represents the world accurately. Therefore the independence condition is satisfied.

Thus the prediction that our program's further novel predictions will succeed itself qualifies as novel with respect to the theory -the hypothesis- that predicts it; with respect, that is, to the conjecture that our program is advancing epistemically. The new conditions for novelty have enabled us to achieve self-referential consistency: by MSRP's account of what it takes to be progressive, the addition to it of the epistemic conjecture is progressive. Novel success constitutes progress, and the epistemic addition carries its own novel success. Therefore the conjecture is justified.

Crucial to this argument is the premise that absent the epistemic conjecture there is no basis for predicting that the program's track record of successful novel prediction will continue. In particular, this track record does not itself forecast its own continuation. That is, the fact that the program has met with continued novel success to date is not evidence that there will be further success. But this premise is surely available to MSRP. To suppose otherwise is to accept the naive inductivism that Lakatos joins Popper is rejecting. In fact, the premise was already implicit in the assumption that the uniqueness condition permitted previously known results to count as novel. That a result has occurred is not in itself a basis for predicting its recurrence, except by an impermissible straight induction.

4. The Argument Collapses

Have I supplied the missing argument that Lakatos needs to extract himself from the relativist rut and connect MSRP to the epistemic purpose of doing science? I fear not, for the argument I have constructed on his behalf is at least incomplete and possibly question-begging. What has been argued is that the addition of the epistemic conjecture to MSRP is progressive.

This is the respect in which we are justified in adding it. But, we must ask, is it epistemically progressive or merely empirically and prudentially so? That is, does assuming it just make the program work better and yield more successful predictions, or does it advance the program toward truth? Is the assumption that the program achieves truth truthful, or merely useful? A demonstration that the epistemic conjecture is progressive is reason to think it true only if progressiveness betokens truth; but this is precisely what is conjectured.

We can put the problem this way: The addition of the epistemic conjecture to MSRP is, under the hypothesized conditions, progressive by MSRP's standard of progressiveness. But is this the right standard if our goals are epistemic? To presuppose that it is the right standard is to render the defense of the epistemic conjecture circular.

The problem at root is, of course, that self-referential consistency is at most a constraint, a condition of adequacy, on theories; it is not epistemically probative. It cannot be, for incompatible rival theories could satisfy it equally. It happens that extant rivals to MSRP all violate it, or so Lakatos argued. But this is incidental; the weakness of the competition does not make MSRP the right methodology. If the condition were probative, then the theory that the best theory is the one that faces the most problems would be the best theory, because it faces the most problems. To obtain a probative condition requires, evidently, that we add to self-referential consistency that old-fashioned inference from the evidence that Lakatos decried as failed methodology. We must, that is, endorse some form of justificationism.

MSRP was supposed to be a better methodology, a more sophisticated analysis of scientific practice at its progressive best. But how could it be, if progress is understood epistemically? If theories cannot be justified by their predictive success nor refuted by their predictive failure, then what good is all the additional sophistication? In the end, it simply creates further conditions for itself to fulfill, without adducing the least epistemic significance to doing so. Lakatos, or our reconstruction of him, may justly be admired for the rare philosophical achievement of proving able to solve a problem of his own creation, but this does not help him with the original epistemic problem that faced him all along.

5. *Maligned Methodology to the Rescue*

The solution to this problem, if there is one, must be that predictive success sometimes does justify theoretical beliefs, in the epistemic sense of justification, and that predictive failure sometimes does justify disbelief. Of course the caveat, 'sometimes,' is crucial; Lakatos was no doubt right to reject the bare bones of the hypothetico-deductive method. The needed qualification is, I suggest, obtainable from the conditions I have proposed for novelty. Because of these conditions, only a theory's truth, or some semblance or progress thereto, can explain its achievement of a sustained record of successful novel prediction unmarred by error. This is the argument I develop for realism in my (1997).

What needs to said here is that the argument is unavailable to Lakatos because of his dismissal of justificationist and falsificationist methodologies. So long as one is entitled to ignore empirical anomalies in appraising a research program, so long as predictive failure is impotent to check the bandwagon of growth of a program supplying the scientific community with profitable employment, so long as logical inconsistency of theoretical foundations is not recognized as a liability that must eventually be overcome even as one tolerates it in the short term for pragmatic and heuristic benefits, it cannot be epistemic progress that one is appraising. So long as one is forbidden to attribute predictive success to the epistemic merits of one's theory, so long as whatever predictive success one's theory achieves, however impressive, is supposed less that what would have been achieved by a degenerating rival if only the efforts of the right people had happened to be invested in the rival instead of in the progressing theory, it cannot be epistemic progress that one is diagnosing.

In his effort to find some source of epistemic justification for a methodologically progressive research program, Lakatos failed to respect basic tenets of the very concept of epistemic justification. In particular, it must be closed under conjunction and entailment. Otherwise reasoning cannot extend a justified system of beliefs, and scientists certainly extend their theoretical commitments through reasoning. It does little good to identify an empirical source of epistemic justification for theoretical hypotheses, whether in novel predictive success, as I would have it, or anywhere else, unless justification is transmitted across inferential links to other hypotheses that do not enjoy it directly. For the more fundamental theoretical commitments of science are typically the more remote from empirical test. And transmission through inference requires deductive closure at a minimum.

Conjunctive closure means that any conjunction of justified conjuncts is justified. Without this condition, no deductively valid inference rule that delivers a conclusion not entailed by a single premise in isolation transmits justification. P and $P \rightarrow Q$ may each be justified, but as neither entails Q their justifications do not extend to Q unless their conjunction is also justified. Q may be regarded as a logical consequence of the *set* $\{P, P \rightarrow Q\}$, but sets of statements are not proper objects of justification; only their elements are. That inference rules in formal logic are typically designed to avoid the conjunctive step may be misleading. It is present in the form of the requirement of a fixed multiplicity of preceding steps. The basis for the rule is always that the conjunction of these steps entails the inferred step. But for this, the metatheoretic completeness results without which formal inference is valueless are not provable.

Since contradictions cannot be epistemically justified, it follows from the closure of justification under conjunction that the individual hypotheses of an inconsistent theory or research program cannot be justified either. They can of course be "adopted" for pragmatic purposes and can function heuristically, but their collective inconsistency is intolerable epistemically. Moreover, in combination with the closure of justification under entailment, conjunctive closure carries the consequence that every theory is justified if the members of an inconsistent set of hypotheses are justified. By ignoring inconsistency in the context of appraisal, MSRP preempts its aspiration to serve as a theory of epistemic progress.

MSRP's failure to respect failed predictions is a worse problem, as predictive failure, "empirical anomaly," is, by Lakatos's own testimony, more pervasive than inconsistency. To the (very great) extent that prediction has a deductive structure, the closure of justification under entailment ensures that no theory yielding predictions that fail is epistemically justified. According to Lakatos, the typical research program -indeed, every research program- faces massive and perpetual predictive failure. If he is right, the typical research program, however progressive MSRP diagnoses it to be, is unjustified[6].

But of course he is not right. It is only by assimilating all empirical problems facing theories into a single category of predictive failures that he was able to make plausible his contention that every theory is born, lives, and dies refuted (1973, p. 5). But it is through neither accident nor intellectual stubbornness that scientists regard some empirical problems as anomalies to be accommodated by the revision of auxiliaries or to be dismissed as the evil of another day, while regarding other problems as decisive refutations. This is a real, albeit elusive, distinction, based not self-interestedly on the degree of scientists' investment in a program but disinterestedly on judgments about the potential resources of the program for handling specific empirical problems.

Consider again the anomalous motion of stars at the periphery of the Milky Way. What makes this motion anomalous is that it has no evident basis in prevailing ideas about the formation of galaxies. It does not refute these ideas, however, because it might have some other basis compatible with them, such as a gravitational influence of uncharted dark matter. Consider the problem of the great chain of galaxies, a structure large enough to be anomalous for the big bang theory, which predicts large-scale uniformity of the present universe. This problem does not refute the big bang theory, for several reasons. With less than one per cent of the universe surveyed for such structures, the degree of departure from large-scale uniformity which they represent cannot yet be estimated. More importantly, there must obviously be mechanisms subsumable within big bang cosmology capable of generating great local concentrations of mass-energy. These mechanisms are not well enough understood to impose limits on the scale of such concentrations, and some involve quantum effects, such a spontaneous symmetry breaking, whose consequences are probabilistic. It is for reasons such as these, not because of any general ideology of adherence to theories in the face of anomaly, or general indifference to predictive failure, that big bang cosmology continues to be viable. It is perfectly easy, contrary to Lakatos, to specify purely empirical conditions under which big bang cosmology would not be viable even if no better alternative theory were available. The constancy of the density of matter with distance, together with the failure to identify any cosmic background radiation, would surely do it.

Even if Lakatos is right that all theories constantly face empirical problems, some of these problems are appropriately discounted or bracketed for specific scientific reasons while others are immediately serious and could only be obviated by some general Lakatosian license to ignore them. As it is not the case that all theories face empirical problems of the latter sort, no such license is needed. Neither general relativity nor contemporary quantum mechanics predicts anything known to be untrue, despite immense, sustained critical effort. To protect the epistemic aspirations of science by decreeing that methodology respect predictive failure, to deny Lakatos his license, is not to threaten the epistemic status of science.

What does threaten this status is Lakatos's refusal, following Popper, to respect ampliative reasoning -his conversion of the "problem of induction" into the problem of finding a special, additional epistemic justification for a research program beyond its empirical progressiveness. There is no such extra justification. If theories are to be epistemically justified, it will have to be on the basis of their predictive success. That is, the *original* problem of induction cannot be evaded. My proposal is to restrict probative weight to novel predictive successes, in the sense of my uniqueness and independence conditions, and to invoke explanatory reasoning, along side ordinary induction, in attributing such success to the truth of the theory that achieves it. Without abduction, science is merely instrumental.

Perhaps it was his mission to save philosophy of science from the *a priori* and give it an empirical grounding that prevented Lakatos from appreciating the dilemma he created for himself in dismissing justificationist and falsificationist methodologies. An empirical grounding meant fidelity to scientific practice, or at least, since this was plainly impossible, the rational reconstruction of scientific practice. But scientific practice is not primarily, and certainly not exclusively, about epistemic appraisal. Within scientific practice it is virtually impossible to separate out purely epistemic judgments from judgments of progressiveness along any number of other dimensions of appraisal. The judgment that a theory is on the right track epistemically and the judgment that it offers the best way to make progress in solving important problems are judgments that need not differ at all with respect to the behavior they engender. These judgments may not even be distinguished in the mind of the practicing scientist, who has, after all, been sadly used by the many reductive analyses of truth and knowledge forthcoming from philosophy. The distinctions that have to be made to judge an epistemic theory's conformity to evaluative practice in science are philosophical distinctions that cannot be read off of practice. The very phrase "theory choice" by which evaluative practice is commonly labeled plainly identifies the obstacle to founding an epistemic theory on the judgments of scientists: chosen for what? Required to be an empirical theory, MSRP becomes a theory of pursuit, not acceptance, because pursuit, not acceptance, is what we can see scientists doing. In the context of pursuit, refutations may be ignored, appraisals may be relative to available rivals, and progress can be achieved on inconsistent foundations. But you do not get epistemology short of acceptance. And the judgment that a theory is epistemically acceptable is unavoidably a philosophical overlay to the reasoning that propels practice.

Notes

1 Note that this observation is unaffected by Lakatos's admonition (1974, p. 148) that the meta-criterion he employs in criticizing Popper is not the one he will eventually accept himself. For the one he accepts himself he applies only to MSRP, not to Popper's theory. Popper's theory is supposed to be refuted by counterexample. But why should it be, if naive falsificationism is wrong?

2 "*Crucial experiments are seen to be crucial only decades later*" (1970, p. 72; italics in original).

3 Lakatos comes this close to applying the standard of MSRP to his epistemic conjecture: "(...) a solution (...) [to the problem of induction] (...) is interesting only if it is embedded in, or leads to, a major research program; if it creates new problems -and solutions- in turn" (1974, p. 164). But he immediately abandons this line as unavailing, and in place of defending the conjecture argues that Popper's theory descends into relativism for want of it. It is hard to see this move as any better than subterfuge. Instead of benefitting by comparison to Popper, MSRP is in fact subject to same objection.

4 The historian Stephen Brush has attempted to adjudicate the controversy with sociological evidence of the disinclination of scientists to attach extra evidential weight to novelty (1989). Brush's uncritical identification of novelty with temporal newness renders the attempt inconclusive.

5 In identifying a theory's excess empirical content with its prediction of novel facts (1970, pp. 32-33), Lakatos remarks that possession of excess content is decidable "instantly by a priori logical analysis"; only the verification of this content takes time. This is surprising in view of his sensitivity, in the context of falsification, to the complex role of shifting auxiliary information in determining a theory's empirical commitments (1970, pp. 17-19).

6 Admittedly, the closure of epistemic justification under conjunction and entailment are controversial in epistemology, in large measure because of paradoxes like the lottery and the preface that they appear to generate. It is unfortunately beyond the scope of a paper on Lakatos to undertake a general defense of these principles. My discussion of their role in reasoning indicates the line of defense I favor. The paradoxes do not arise if justification is severed from probability, as it must be if justification is to be closed under conjunction.

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