Falsification and the Methodology of Scientific Research Programs

Imre Lakatos · From *Criticism and the Growth of Knowledge* · In Schick: *Readings in the Philosophy of Science*

1. SCIENCE: REASON OR RELIGION?

For centuries knowledge meant proven knowledge—proven either by the power of the intellect or by the evidence of the senses. Wisdom and intellectual integrity demanded that one must desist from unproven utterances and minimize, even in thought, the gap between speculation and established knowledge. The proving power of the intellect or the senses was questioned by the skeptics more than two thousand years ago; but they were browbeaten into confusion by the glory of Newtonian physics. Einstein's results again turned the tables and now very few philosophers or scientists still think that scientific knowledge is, or can be, proven knowledge. But few realize that with this the whole classical structure of intellectual values falls in ruins and has to be replaced: one cannot simply water down the ideal of proven truth—as some logical empiricists do—to the ideal of 'probable truth'[1] or—as some sociologists of knowledge do—to 'truth by [changing] consensus'.[2]

Popper's distinction lies primarily in his having grasped the full implications of the collapse of the best-corroborated scientific theory of all times: Newtonian mechanics and the Newtonian theory of gravitation. In his view virtue lies not in caution in avoiding errors, but in ruthlessness in eliminating them. Boldness in conjectures on the one hand and austerity in refutations on the other: this is Popper's recipe. Intellectual honesty does not consist in trying to entrench, or establish one's position by proving (or 'probabilifying') it intellectual honesty consists rather in specifying precisely the conditions under which one is willing to give up one's position. Committed Marxists and Freudians refuse to specify such conditions: this is the hallmark of their intellectual dishonesty. *Belief* may be a regrettably unavoidable biological weakness to be kept under the control of criticism: but *commitment* is for Popper an outright crime.

Kuhn thinks otherwise. He too rejects the idea that science grows by accumulation of eternal truths.[3] He too takes his main inspiration from Einstein's overthrow of Newtonian physics. His main problem too is *scientific revolution*. But while according to Popper science is 'revolution in permanence', and criticism the heart of the scientific enterprise, according to Kuhn revolution is exceptional and, indeed, extra-scientific, and criticism is, in 'normal' times, anathema. Indeed for Kuhn the transition from criticism to commitment marks the point where progress—and 'normal' science—begins. For him the idea that on 'refutation' one can demand the rejection, the elimination of a theory, is 'naive' falsificationism. Criticism of the dominant theory and proposals of new theories are only allowed in the rare moments of 'crisis'. This last Kuhnian thesis has been widely criticized and I shall not discuss it. My concern is rather that Kuhn, having recognized the failure both of justificationism and falsificationism in providing rational accounts of scientific growth, seems now to fall back on irrationalism.

For Popper scientific change is rational or at least rationally reconstructible and falls in the realm ofthe *logic of discovery*. For Kuhn scientific change—from one 'paradigm' to another—is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the *(social) psychology of discovery*. Scientific change is a kind of religious change.

The clash between Popper and Kuhn is not about a mere technical point in epistemology. It concerns our central intellectual values, and has implications not only for theoretical physics but also for the underdeveloped social sciences and even for moral and political philosophy. If even in science there is no other way of judging a theory but by assessing the number, faith and vocal energy of its supporters, then this must be even more so in the social sciences: truth lies in power. Thus Kuhn's position would vindicate, no doubt, unintentionally, the basic political *credo* of contemporary religious maniacs ('student revolutionaries').

In this paper I shall first show that in Popper's logic of scientific discovery two different positions are conflated. Kuhn understands only one of these, 'naive falsificationism' (I prefer the term 'naive methodological falsificationism'); I think that his criticism of it is correct, and I shall even strengthen it. But Kuhn does not understand a more sophisticated position the rationality of which is not based on 'naive' falsificationism. I shall try to explain—and further strengthen—this stronger Popperian position which, I think, may escape Kuhn's strictures and present scientific revolutions as constituting rational progress rather than as religious conversions.

2. FALLIBILISM VERSUS FALSIFICATIONISM

Sophisticated versus naive methodological falsificationism. Progressive and degenerating problemshifts.

Sophisticated falsificationism differs from naive falsificationism both in its rules of acceptance (or 'demarcation

criterion') and its rules of *falsification* or elimination. For the naive falsificationist any theory which can be interpreted as experimentally falsifiable, is 'acceptable' or 'scientific'. For the sophisticated falsificationist a theory is 'acceptable' or 'scientific' only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts. This condition can be analyzed into two clauses: that the new theory has excess empirical content (*'acceptability'*$_1$) and that some of this excess content is verified (*'acceptability'*$_2$). The first clause can be checked instantly by a priori logical analysis; the second can be checked only empirically and this may take an indefinite time.

Again, for the naive falsificationist a theory is *falsified* by a '(fortified) observational' statement which conflicts with it (or rather, which he decides to interpret as conflicting with it). The sophisticated falsificationist regards a scientific theory T as falsified if and only if another theory T' has been proposed with the following characteristics: (1) T' has excess empirical content over T: that is, it predicts *novel* facts, that is, facts improbable in the light of, or even forbidden, by T;[4] (2) T' explains the previous success of T, that is, all the unrefuted content of T is contained (within the limits of observational error) in the content of T; and (3) some of the excess content of T' is corroborated.[5] […]

Let us take a series of theories, T_1, T_2, T_3, … where each subsequent theory results from adding auxiliary clauses to (or from semantical reinterpretations of) the previous theory in order to accommodate some anomaly, each theory having at least as much content as the unrefuted content of its predecessor. Let us say that such a series of theories is *theoretically progressive (or 'constitutes a theoretically progressive problemshift')* if each new theory has some excess empirical content over its predecessor, that is, if it predicts some novel, hitherto unexpected fact. Let us say that a theoretically progressive series of theories is also *empirically progressive (or 'constitutes an empirically progressive problemshift')* if some of this excess empirical content is also corroborated, that is, if each new theory leads us to the actual discovery of some *new fact*.[6] Finally, let us call a problemshift *progressive* if it is both theoretically and empirically progressive, and *degenerating* if it is not.[7] We *'accept'* problemshifts as 'scientific' only if they are at least theoretically progressive; if they are not, we *'reject'* them as 'pseudoscientific'. Progress is measured by the degree to which a problemshift is progressive, by the degree to which the series of theories leads us to the discovery of novel facts. We regard a theory in the series 'falsified' when it is superseded by a theory with higher corroborated content.

This demarcation between progressive and degenerating problemshifts sheds new light on the appraisal of *scientific—or, rather, progressive—explanations*. If we put forward a theory to resolve a contradiction between a previous theory and a counterexample in such a way that the new theory, instead of offering a content-increasing (scientific) *explanation*, only offers a content-decreasing (linguistic) *reinterpretation*, the contradiction is resolved in a merely semantical, unscientific way. *A given fact is explained scientifically only if a new fact is also explained with it.*[8]

Sophisticated falsificationism thus shifts the problem of how to appraise *theories* to the problem of how to appraise *series of theories*. Not an isolated *theory,* but only a series of theories can be said to be scientific or unscientific: to apply the term 'scientific' to one *single* theory is a category mistake.[9]

The time-honored empirical criterion for a satisfactory theory was agreement with the observed facts. Our empirical criterion for a series of theories is that it should produce new facts. *The idea of growth and the concept of empirical character are soldered into one.*

This revised form of methodological falsificationism has many new features. First, it denies that 'in the case of a scientific theory, our decision depends upon the results of experiments. If these confirm the theory, we may accept it until we find a better one. If they contradict the theory, we reject it.'[10] It denies that 'what ultimately decides the fate of a theory is the result of a test, *i.e.*, an agreement about basic statements.'[11] Contrary to naive falsificationism, *no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory.*[12] But then the distinctively negative character of naive falsificationism vanishes; criticism becomes more difficult, and also positive, constructive. But, of course, if falsification depends on the emergence of better theories, on the invention of theories which anticipate new facts, then falsification is *not* simply a relation between a theory and the empirical basis, but a multiple relation between competing theories, the original 'empirical basis', and the empirical growth resulting from the competition. Falsification can thus be said to have a *'historical character'*.[13] Moreover, some of the theories which bring about falsification are frequently proposed *after* the 'counter-evidence'. This may sound paradoxical for people indoctrinated with naive falsificationism. Indeed, this epistem-

ological theory of the relation between theory and experiment differs sharply from the epistemological theory of naive falsificationism. The very term 'counterevidence' has to be abandoned in the sense that no experimental result must be interpreted directly as 'counterevidence'. If we still want to retain this time-honored term, we have to redefine it like this: 'counterevidence to T_1' is a corroborating instance to T_2 which is either inconsistent with or independent of T_1 (with the *proviso* that T_2 is a theory which satisfactorily explains the empirical success of T_1). This shows that *'crucial counterevidence'*—or *'crucial experiments'*—can be recognized as such among the scores of anomalies only *with hindsight*, in the light of some superceding theory.[14]

Thus the crucial element in falsification is whether the *new theory* offers any novel, excess information compared with its predecessor and whether some of this excess information is corroborated. Justificationists valued 'confirming' instances of a theory; naive falsificationists stressed 'refuting' instances; for the methodological falsificationists it is the—rather rare—corroborating instances of the excess information which are the crucial ones; these receive all the attention. We are no longer interested in the thousands of trivial verifying instances nor in the hundreds of readily available anomalies: the few crucial *excess-verifying instances* are decisive.[15] This consideration rehabilitates—and reinterprets—the old proverb: *Exemplum docet, exempla obscurant.*

'Falsification' in the sense of naive falsificationism (corroborated counterevidence) is not a *sufficient* condition for eliminating a specific theory: in spite of hundreds of known anomalies we do not regard it as falsified (that is, eliminated) until we have a better one.[16] Nor is 'falsification' in the naive sense *necessary* for falsification in the sophisticated sense: a progressive problemshift does not have to be interspersed with 'refutations.' Science can grow without any 'refutations' leading the way. Naive falsificationists suggest a linear growth of science, in the sense that theories are followed by powerful refutations which eliminate them; these refutations in turn are followed by new theories.[17] It is perfectly *possible* that theories be put forward 'progressively' in such a rapid succession that the 'refutation' of the n-th appears only as the corroboration of the n+1-th. The problem fever of science is raised by proliferation of rival theories rather than counterexamples or anomalies.

This shows that the slogan of *proliferation of theories* is much more important for sophisticated than for naive falsificationism. For the naive falsificationist science grows through repeated experimental overthrow of theories; new rival theories proposed before such 'overthrows' may speed up growth but are not absolutely necessary;[18] constant proliferation of theories is optional but not mandatory. For the sophisticated falsificationist proliferation of theories cannot wait until the accepted theories are 'refuted' (or until their protagonists get into a Kuhnian crisis of confidence).[19] While naive falsificationism stresses 'the urgency of replacing a *falsified* hypothesis by a better one',[20] sophisticated falsificationism stresses the urgency of replacing *any* hypothesis by a better one. Falsification cannot 'compel the theorist to search for a better theory',[21] simply because falsification cannot precede the better theory.

THE POPPERIAN VERSUS THE KUHNIAN RESEARCH PROGRAM

Let us now sum up the Kuhn-Popper controversy.

We have shown that Kuhn is right in objecting to naive falsificationism, and also in stressing the *continuity* of scientific growth, the *tenacity* of some scientific theories. But Kuhn is wrong in thinking that by discarding naive falsificationism he has discarded thereby all brands of falsificationism. Kuhn objects to the entire Popperian research program, and he excludes *any* possibility of a rational reconstruction of the growth of science. In a succinct comparison of Hume, Carnap and Popper, Watkins points out that the growth of science is inductive and irrational according to Hume, inductive and irrational according to Carnap, non-inductive and rational according to Popper.[22] But Watkins's comparison can be extended by adding that it is non-inductive and irrational according to Kuhn. *In Kuhn's view there can be no logic, but only psychology of discovery.*[23] For instance, in Kuhn's conception, anomalies, inconsistencies always abound in science, but in 'normal' periods the dominant paradigm secures a pattern of growth which is eventually overthrown by a 'crisis'. 'Crisis' is a psychological concept; it is a contagious panic. Then a new 'paradigm' emerges, incommensurable with its predecessor. There are no rational standards for their comparison. Each paradigm contains its own standards. The crisis sweeps away not only the old theories and rules but also the standards which made us respect them. The new paradigm brings a totally new rationality. There are no superparadigmatic standards. The change is a bandwagon effect. Thus *in Kuhn's view scientific revolution is irrational, a natter for mob psychology.*

The reduction of philosophy of science to psychology of science did not start with Kuhn. An earlier wave of 'psychologism' followed the breakdown of justificationism. For many, justificationism represented the only

possible form of rationality: the end of justificationism meant the end of rationality. The collapse of the thesis that scientific theories are provable, that the progress of science is cumulative, made justificationists panic. If 'to discover is to prove', but nothing is provable, then there can be no discoveries, only discovery-claims. Thus disappointed justificationists—ex-justificationists—thought that the elaboration of rational standards was a hopeless enterprise and that all one can do is to study—and imitate—the Scientific Mind, as it is exemplified in famous scientists. After the collapse of Newtonian physics, Popper elaborated new, non-justificationist critical standards. Now some of those who had already learned of the collapse of justificationist rationality now learned, mostly by hearsay, of Popper's colorful slogans which suggested naive falsificationism. Finding them untenable, they identified the collapse of naive falsificationism with the end of rationality itself. The elaboration of rational standards was again regarded as a hopeless enterprise; the best one can do is to study, they thought once again, the Scientific Mind.[24] Critical philosophy was to be replaced by what Polanyi called a 'post-critical' philosophy. But the Kuhnian research program contains a new feature: we have to study not the mind of the individual scientist but the mind of the Scientific Community. Individual psychology is now replaced by social psychology; imitation of the great scientists by submission to the collective wisdom of the community.

But Kuhn overlooked Popper's sophisticated falsificationism and the research program he initiated. Popper replaced the central problem of classical rationality, *the old problem of foundations,* with *the new problem of fallible-critical growth,* and started to elaborate objective standards of this growth. In this paper I have tried to develop his program a step further. I think this small development is sufficient to escape Kuhn's strictures.[25]

NOTES

1. The main contemporary protagonist of the ideal of 'probable truth' is Rudolf Carnap. For the historical background and a criticism of this position, cf. Lakatos [1968a].
2. The main contemporary protagonists of the ideal of 'truth by consensus' are Polanyi and Kuhn. For the historical background and a criticism of this position, cf. Musgrave [1969a], Musgrave [1969b] and Lakatos [1970].
3. Indeed he introduces his [1962] by arguing against the 'development-by-accumulation' idea of scientific growth. But his intellectual debt is to Koyre rather than to Popper. Koyre showed that positivism gives bad guidance to the historian of science, for the history of physics can only be understood in the context of a succession of 'metaphysical' research programs. Thus scientific changes are connected with vast cataclysmic metaphysical revolutions. Kuhn develops this message of Burn and Koyre and the vast success of his book was partly due to his hard-hitting, direct criticism of justificationist historiography—which created a sensation among ordinary scientists and historians of science whom Burtt's, Koyre's (or Popper's) message has not yet reached. But, unfortunately, his message had some authoritarian and irrationalist overtones.
4. I use 'prediction' in a wide sense that includes 'postdiction'.
5. *For a detailed discussion of these acceptance and rejection rules and for references to Popper's work,* cf. my [1968a], pp. 375–90.
6. If I already know P_1: 'Swan A is white', P_ω: 'All swans are white' represents no progress, because it may only lead to the discovery of such further similar facts as P_2: 'Swan B is white'. So-called 'empirical generalizations' constitute no progress. A *new* fact must be improbable or even impossible in the light of previous knowledge.
7. The appropriateness-of the term 'problemshift' for a series of theories rather than of problems may be questioned. I chose it partly because I have not found a more appropriate alternative—'theoryshift' sounds dreadful—partly because theories are always problematical, they never solve all the problems they have set out to solve.
8. Indeed, in the original manuscript of my [1968a] I wrote: 'A theory without excess corroboration has no excess explanatory power; *therefore, according to Popper, it does not represent growth and therefore it is not "scientific"; therefore, we should say, it has no explanatory power*' (p. 386). I cut out the italicized half of the sentence under pressure from my colleagues who thought it sounded too eccentric. I regret it now.
9. Popper's conflation of 'theories' and 'series of theories' prevented him from getting the basic ideas of sophisticated falsificationism across more successfully. His ambiguous usage led to such confusing formulations as 'Marxism [as the core of a series of theories or of a "research program"] is irrefutable' and, at the same time, 'Marxism [as a particular conjunction of this core and some specified auxiliary hypotheses, initial conditions and a *ceteris paribus* clause] has been refuted.' (Cf. Popper [1963].)

Of course, there is nothing wrong in saying that an isolated, single theory is 'scientific' if it represents an advance on its predecessor, as long as one clearly realizes that in this formulation we appraise the theory as the outcome of—and in the context of—a certain historical development.

10. Popper [1945], vol. II, p. 233. Popper's more sophisticated attitude surfaces in the remark that 'concrete and practical consequences can be *more* directly tested by experiment' (*ibid.* my italics).
11. Popper [1934], section 30.
12. 'In most cases we have, before falsifying a hypothesis, another one up our sleeves' (Popper [1959a], p. 87, footnote 1). But, as our argument shows, we must have one. Or, as Feyerabend put it: 'The best criticism is provided by those theories which can replace the rivals they have removed' ([1965], p. 227). He notes that in *some* cases 'alternatives will be quite indispensable for the purpose of refutation' (*ibid.* p. 254). But according to our argument *refutation without an alternative shows nothing but the poverty of our imagination in providing a rescue hypothesis.*
13. Cf. my [1968a], pp. 387 ff.
14. In the distorting mirror of naive falsificationism, new theories which replace old refuted ones, are themselves born unrefuted. Therefore they do not believe that there is a relevant difference between anomalies and crucial counterevidence. For them, anomaly is a dishonest euphemism for counterevidence. But in actual history new theories are born refuted: they inherit many anomalies of the old theory. Moreover, frequently it is *only* the new theory which dramatically predicts that fact which will function as crucial counterevidence against its predecessor, while the 'old' anomalies may well stay on as 'new' anomalies.
15. *Sophisticated falsificationism adumbrates a new theory of learning.*
16. It is clear that the theory T may have excess corroborated empirical content over another theory T' even if both T and T' are refuted. Empirical content has nothing to do with truth or falsity. Corroborated contents can also be compared irrespective of the refuted content. Thus we may see the rationality of the elimination of Newton's theory in favor of Einstein's, even though Einstein's theory may be said to have been born—like Newton's—'refuted'. We have only to remember that 'qualitative confirmation' is a euphemism for 'quantitative disconfirmation'. (Cf. my [1968a], pp. 384–6.)
17. Cf. Popper [1934], section 85, p. 279 of the 1959 English translation.
18. It is true that a certain type of *proliferation of rival theories* is allowed to play an accidental heuristic role in falsification. In many cases falsification heuristically 'depends on [the condition] that sufficiently many and sufficiently different theories are offered' (Popper [1940]). For instance, we may have a theory T which is apparently unrefuted. But it may happen that a new theory T', inconsistent with T, is proposed which equally fits the available facts: the differences are smaller than the range of observational error. In such cases the inconsistency prods us into improving our 'experimental techniques', and thus refining the 'empirical basis' so that either T or T' (or, incidentally, both) can be falsified: 'We need [a] new theory in order to find out where the old theory was deficient' (Popper [1963], p. 246). But the role of this proliferation is *accidental* in the sense that, once the empirical basis is refined, the fight is between this refined empirical basis and the theory T under test; the rival theory T' acted only as a *catalyst*.
19. Also cf. Feyerabend [1965], pp. 254–5.
20. Popper [1959a], p. 87, footnote 1.
21. Popper [1934], section 30.
22. Watkins [1968], p. 281.
23. Kuhn [1965]. But this position is already implicit in his [1962].
24. Incidentally, just as some earlier ex-justificationists led the wave of skeptical irrationalism, so now some ex-falsificationists lead the *new* wave of skeptical irrationalism and anarchism. This is best exemplified in Feyerabend [1970].
25. Indeed, as I had already mentioned, *my concept of a 'research program' may be construed as an objective, 'third world' reconstruction of Kuhn's socio-psychological concept of paradigm':* thus the Kuhnian 'Gestalt-switch' can be performed without removing one's Popperian spectacles.

    (I have not dealt with Kuhn's and Feyerabend's claim that theories cannot be eliminated on any *objective* grounds because of the 'incommensurability' of rival theories. Incommensurable theories are neither inconsistent with each other, nor comparable for content. But we can *make* them, by a dictionary, inconsistent and their content comparable. If we want to eliminate a program, we need some methodological deter-

mination. This determination is the heart of methodological falsificationism; for instance, no result of statistical sampling is ever inconsistent with a statistical theory unless we *make them* inconsistent with the help of Popperian rejection rules.)

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