

REVIEW ARTICLE

Refined Falsificationism Meets the Challenge from the Relativist Philosophy of Science

GUNNAR ANDERSSON [1988]: *Kritik und Wissenschaftsgeschichte. Kuhns, Lakatos' und Feysabendts Kritik des Kritischen Rationalismus*. x + 218 pp. Hardback DM 98. ISBN 3-16-945308-4.

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I TOUR D'HORIZON OR THE ECOLOGY OF THE PHILOSOPHY OF SCIENCE

In our century, the philosophy of science has been overshadowed by two towering figures: Popper and Wittgenstein, both Viennese emigrants, who have become subjects to the Queen (*cf.*, *e.g.*, Radnitzky [1987a] *Entire Wittgenstein et Popper*...). The discussion has been structured by two great controversies: from the 30s Popper versus logical positivism (or falsificationism versus verificationism/probabilism), and from the 60s 'the new philosophy of science' versus Critical Rationalism. (Exemplary contributions to these two controversies can be found, *e.g.*, in the two collections Radnitzky and Andersson (*eds.*) [1978], *Progress... Science*, and [1979], *Structure... Science*.) Wittgenstein's *Tractatus* has been the idée directrice of the Vienna Circle and its successor, Logical Empiricism. The cynosure of 'the new philosophy of science' is Wittgenstein's later philosophy as presented, in particular, in his *Philosophical Investigations*. If you apply it to the philosophy of science, you will view 'normative' methodology as a counterpart of ideal language philosophy, and hence regard any methodological prescriptions as unrealistic and claim that philosophy of science cannot do more than describe the practice of science, preferably in terms of case studies culled from the

history of science. You will be placed on the road to relativism (Kuhn, Feyerabend, Hübner, and others). An interesting variant of relativism is Stegmüller's attempt, with the help of the formal methods developed by Sneed, to give Kuhn's results a more exact form (Sneed, Stegmüller, Moulines, Mostarín)—what Feyerabend has called 'the Sneedification of science'. Today, departments of philosophy of science are dominated by philosophers who either sympathize with logical empiricism or with 'the new philosophy of science'. Critical Rationalism has an outsider position, but enjoys the sympathy of many scientists (Bartley [1989], *Unfathomed Knowledge*...; see also Bartley [1987b], 'Philosophy of biology...', and Munz [1987], '... the mirror of Rorty').

In addition to Wittgenstein's work two books have been of paramount importance for the discussion: Popper's *magnum opus* of 1934, *Die Logik der Forschung*, and Kuhn's volume of 1962, *The Structure of Scientific Revolutions*. Popper's book went almost unnoticed until the English edition appeared in 1959. It made Popper so-to-speak famous overnight. Kuhn is a Ludwik Fleck *redivivus*—if you combine Fleck with the later Wittgenstein you get Kuhn's position. Fleck's book of 1935 remained unnoticed and was rediscovered only recently (cf., e.g., Andersson [1984a], '... Fleck's conception...'; see also Jarvie's explanation of Kuhn's success in the philosophy establishment in Jarvie [1988], '... Kuhn as ideologue...', and Bartley [1989], *Unfathomed Knowledge*...).

A common characteristic of the critics of Critical Rationalism is that, following Wittgenstein's later philosophy, they adopt what Popper has called 'the myth of the framework' and interpret the history of science as sequences of 'incommensurable' styles, traditions, or 'paradigms', in which no criteria are available that overarch traditions or 'paradigms'; and this in turn suggests that rational appraisal of such traditions or paradigms is impossible. Popper's Critical Rationalism, on the other hand, maintains that traditionalism and the demand for rational scrutiny can be combined (cf., e.g., Andersson (ed.) [1984], *Rationality*...). The relativists and the so-called 'sociology of knowledge' claim that the critical tradition is impossible. W. W. Bartley, III has argued that their criticism applies to what has been called 'comprehensive rationalism' but not to comprehensively critical rationalism or 'pancritical rationalism' (see, e.g., Bartley [1987a], 'A refutation...'; for a criticism of the 'sociology of knowledge' see Part III of Radnitzky and Bartley (eds.) [1987], and Jarvie [1988], '... sociological turn...'). Relativism and the 'sociology of knowledge' has far-reaching implications outside the field of the philosophy of science. However, the strength of the arguments upon which their adherents base their claims resides on their work in the philosophy of science. If it could be shown that their philosophy of science is untenable, this should have repercussions in many other fields.

2 DO THE CASE STUDIES WHICH THE 'NEW PHILOSOPHY OF SCIENCE'
HAS CULLED FROM THE HISTORY OF SCIENCE SHOW THAT THE
FALSIFICATIONIST METHODOLOGY IS UNREALISTIC?

Gunnar Andersson (University of Umeå, Sweden) submits an answer to this question in his latest book. Before the case studies used by Kuhn, Lakatos and Feyerabend can be analyzed the logical structure of falsification arguments has to be clarified and some concomitant methodological problems have to be solved. The author examines the most important of the 'classical' case studies which the critics of Popperian methodology have submitted, and he shows that none of them withstands a critical scrutiny. In the process of appraising the arguments of the critics it turns out that the Popperian methodology needs to be improved in certain central areas. By introducing 'pancritical rationalism', W. W. Bartley, III has improved the epistemological framework of Critical Rationalism (Bartley [1962/86], *Retreat to...*, Bartley [1987b] 'Philosophy of biology...'). This achievement is of interest mainly to epistemology and to philosophy in general. Andersson's book provides the first major improvement of falsificationist methodology since the 30s. For the first time, falsification arguments are provided with a *metalogical basis*. The author also shows: (a) how *unproblematic test statements can be deduced from problematic ones*; (b) that *empirical testing concerns theoretical systems as wholes*; and (c) how *Popper's view of auxiliary hypotheses and ad-hoc hypotheses can be improved*. As a result of his investigations Kuhn's criticism of falsificationist methodology collapses and the so-called Incommensurability Thesis turns out to be false.

3 OBJECTIVES, STRUCTURE AND RESULTS OF THE VARIOUS
CHAPTERS OF THE VOLUME

The first chapter outlines and summarizes the criticism that Kuhn, Lakatos, and Feyerabend have brought forward against falsificationist methodology, a criticism based on case studies culled from the history of science.

The second chapter clarifies the logical structure of falsification arguments. In order to appraise the conclusions that the critics have drawn from their case histories, the logical structure of explanation, prediction and falsification has to be elucidated and the logical relationships between them examined. In his *The Logic of Scientific Discovery*, Popper only deals with two special cases of falsification arguments: (1) the falsification of a theoretical system (including the statements of the initial conditions) with the help of the negation of a prognosis; (2) the falsification of an isolated hypothesis with the help of a 'basic statement'. Popper has not given a satisfactory answer to the question of why certain falsification arguments are valid. Before that question can be answered, the logical relationships have to be clarified between explanation

and falsification on the one hand and explanation and prediction on the other hand. The author shows that, if predictive arguments are valid, certain types of falsificatory arguments are valid too, and vice versa. The formal proofs for these metalogical equivalences are gathered in an Appendix. It turns out that the two special types dealt with by Popper are not the only valid ones, that the idea of a falsificatory argument can be generalized—that many other forms are valid too. In order to appraise the criticism against falsificationism brought forward by Kuhn, Lakatos, Feysabend and others, it is imperative to show how theoretical systems that consist of several general hypotheses can be falsified. After all, what is at stake in the history of science and the practice of science are very rarely isolated hypotheses, normally theoretical systems are at the center of interest. It turns out that the thesis that explanatory and predictive arguments have the same logical form does not hold in general. It only holds for explanation and for the deduction of unconditional predictions, but it does not hold for explanation and the deduction of conditional predictions. Explanations and arguments having a conditional prediction as conclusion are metalogically equivalent. The author extends the analysis of the logical structure of explanation (Popper, Hempel, Oppenheim, Stegmüller, etc.) to the analysis of the logical structure of predictions (conditional and unconditional), and the analysis of the structure of falsification arguments.

The *third* chapter is devoted to *Thomas Kuhn*. It is shown that his criticism of falsificationism hinges on the position he takes *vis-à-vis* to two methodological problems: (1) *What are the implications of the theory dependence of experience?*, and (2) *What is the 'rational' reaction of researchers to a falsification?* From different starting points, Popper, Kuhn, N. R. Hanson, and others, have reached the conclusion that the so-called basic statements or test statements are theory-dependent, fallible, and hence revisable. So far so good. Kuhn overstates the case when he claims that scientists who have adopted radically different background assumptions or paradigms eventually 'live in different worlds', and thus arrive at different test statements, with the result that the theories they develop are incommensurable.

With respect to his position *vis-à-vis* the second methodological problem (the researcher's response to a falsification) Kuhn keeps in the context of justificationist philosophy. He believes that falsification is something definite and irreversible. This mistaken view of falsification underlies his claim that in 'normal science' a falsification does not lead to the rejection of the theoretical system falsified but rather to attempts to modify that system. Kuhn then argues that this strategy immunizes theoretical systems against falsifications. Having played down the role of falsification in scientific research Kuhn thinks that Popperian (normative) methodology is unrealistic. Chapter 3 is devoted to outlining Kuhn's position. Andersson shows that Kuhnian key concepts like 'puzzle', 'anomaly', 'counter example', 'paradigm', etc., are ambiguous and

unclear and also that Kuhn's methodological arguments are marred by irrelevant psychological analyses. The decisive criticism of Kuhn's position is postponed to Chapters 6 and 7.

The *fourth* chapter is devoted to *Imre Lakatos's* attempt to remould Popper's position in such a way that it accommodates those parts of Kuhn's criticism that Lakatos considered correct. It is an attempt to save the rationality of science from relativism—in spite of Kuhn. As is well known, Lakatos examines various proposals how to deal with the problem of the theory dependence of experience. After having rejected the positivist proposal that certain basic statements can be proven to be true, Lakatos first interprets Popper as a naive falsificationist, who thinks that basic statements are made unfalsifiable by fiat, by a conventional decision. According to Popper—as a sophisticated falsificationist—basic statements can be criticized by deducing further consequences from them. Lakatos criticizes this correct insight by asserting that basic statements are not checked in the way Popper claims that they are. He claims that they are checked with the help of 'interpretative theories'. (Allegedly, such an 'interpretative theory' functions to allot truth values to test statements.) Andersson shows that Lakatos's attempt to solve the problem of the empirical base by means of introducing the concept of 'interpretative theories' is circular: basic statements are *ultimately* appraised with the help of 'interpretative theories', and these theories are then in turn appraised by means of basic statements. Playing down the role of falsification in research, Lakatos eventually arrives at a paradoxical mixture of conventionalism and inductivism.

Lakatos accepts Kuhn's thesis that, in the context of 'normal science', theories are relatively immune against falsification, and he attempts to analyze the mechanism of such immunization strategies. Whenever a theoretical system faces a falsification, various reactions or (internal) research policies are possible. You can try to repair the situation either by modifying peripheral parts or by modifying central parts of the theoretical system concerned. Lakatos comes to believe that by making modifications in the peripheral parts (in the 'protective belt'), scientists try to salvage the central parts of the theory; they do so by the methodological decision to make the central parts 'unfalsifiable'—hence, the robustness of the so-called 'hard core' of a research program. Thus, Lakatos concludes that the immunization strategies that Kuhn had claimed for 'normal science' are rational after all. By contrast to Kuhn—who in Lakatos's view has in the last resort fallen back on consensus in the sense of 'mob psychology'—Lakatos tries to develop objective criteria for the appraisal of various research strategies. Andersson shows that these criteria are problematic, because they involve an *ex ante* appraisal of the 'heuristic potential' of a research program and thereby involve guesses about the future performance of the program in question. Lakatos has not been able to explain

how such an *ex ante* appraisal could work. Andersson conjectures that this difficulty may have induced Lakatos to abandon methodology and retract to the 'rational reconstruction' of the history of science. Left without a methodology that could supply criteria of appraisal, Lakatos falls back on appraisals contained in the 'normative basic judgments' of 'the scientific elite'. This again leads to a circle: in order to determine who should belong to 'the scientific elite', we need objective criteria for appraising past achievements.

The *fifth* chapter is devoted to *Paul Feyerabend's position*. Fallibilism combined with a particular interpretation of the theory dependence of experience eventually led Feyerabend to the Incommensurability Thesis and to a general position that he himself has characterized as 'epistemological anarchism' or 'dadaism'. Andersson examines Feyerabend's criticism of Lakatos's methodology of scientific research program and of Lakatos's idea of a 'rational reconstruction' of the history of science. He then analyzes Feyerabend's criticism of Popperian methodology. Feyerabend sharpens the relativistic implications of Kuhn's position. With Kuhn and Lakatos he believes that, in 'normal science', theories are relatively resistant against falsification, and he explains that alleged fact by asserting that a falsified theory can always be defended with the help of auxiliary hypotheses or *ad hoc* hypotheses. He regards such a strategy as economical, recommendable or at least defensible, because it reduces the risk that a theory is prematurely abandoned. Abandoning a theory 'too early' could involve high opportunity costs in terms of scientific progress foregone—the progress we might have achieved if only we had given the falsified theory more opportunity to show its worth. However, in the context of Feyerabend's 'epistemological anarchism', it is not possible to give an objective explication of the idea of scientific progress—according to him there are no objective or general criteria. The only thing Feyerabend can do is—how he himself has expressed it—to use the term 'scientific progress' in the sense in which 'others use it'. Feyerabend has abandoned the problem of rational theory preference, because he regards it as unsolvable.

Andersson proceeds by analyzing Feyerabend's case histories, in particular, Galileo's defense of the Copernican system by introducing new auxiliary hypotheses like the hypothesis about the reliability of observations through a telescope and the hypotheses that introduce a new dynamics. Like Kuhn, Feyerabend uses the case studies to support the Incommensurability Thesis, in particular, his claim that the Ptolemaic and the Copernican systems are optically and dynamically incommensurable.

The analysis of Feyerabend's criticism of Popperian methodology shows that his criticism, exactly like Kuhn's, hinges on the position he takes with respect to the two *aforesaid methodological problems*: (1) the problem of the theory dependence of observation, and (2) the problem of the empirical testing of

theoretical systems. That means that the challenge to falsificationism from the history of science is basically a discussion of methodological problems with the help of examples culled from the history of science. Thus, Andersson now turns to the analysis of these two basic methodological problems.

The sixth chapter is devoted to the problem solutions that Popper has offered to the so-called problem of the empirical base. Andersson shows that Popper's formal requirement that a basic statement should have the form of a 'there-is'-sentence has the unacceptable consequence that basic statements cannot contradict each other and therefore are unfalsifiable (and hence also non-scientific). Andersson shows that this difficulty can be overcome by a slight modification of the Popperian requirement, and he proposes that *all* sorts of singular sentences that are properly individuated and describe observable phenomena qualify as basic statements or as test statements. This proposal is supported by the metalogical equivalence between explanation and falsification (which has been demonstrated in the Appendix). For the empirical testing of theories the importance of the reproducibility of the effects described in the basic statements can scarcely be overrated.

Popper's basic statements follow from a conjunction of singular statements that describe the antecedent conditions and a negated unconditional prediction. It is appropriate to view a falsification argument as an argument whose premises consist of the antecedent conditions *A* and a negated unconditional prediction $\neg P$. This interpretation is preferable to the customary one according to which the premises consist of a single basic statement, mainly (but not exclusively) because thereby the relationship between falsification and the deduction of predictions is clearly shown: '*A, H* \vdash *P*' being metalogically equivalent to '*A, \neg P* \vdash $\neg H$ '. Hence, Andersson's explication of the concept of a Falsifying Argument is wider than that of Popper.

Popper claims that a critical discussion of theory-dependent test statements is possible, but he has given only some hints on how this could be done. Andersson shows in detail how unproblematic test statements can be derived from problematic ones with the help of auxiliary hypotheses. It is always possible from two theories that describe the same sort of phenomenon but are allegedly incommensurable to deduce further test statements until one arrives at test statements that are unproblematic in the sense that they are neutral *vis-à-vis* the two competing theories. In order to substantiate this claim, Andersson analyzes some of Kuhn's and Feyerabend's historical case studies and demonstrates in detail how theories, which according to Kuhn and Feyerabend, are incommensurable, can be made commensurable by deducing further test statements that are unproblematic. Thus, he can show that, *e.g.*, the Copernican and Ptolemaic theories turn out to be optically and dynamically commensurable, and that the phlogiston theory and the oxygene theory can be compared with each other. It transpires that a falsificationist

interpretation of the 'classical' case studies of Kuhn and Feyerabend is far superior to the interpretations that Kuhn and Feyerabend have offered. The incommensurability thesis evaporates.

The seventh chapter is devoted to the problem of the modification of theoretical systems and theories after a falsification. Popper's anti-conventionalist methodological proposals are discussed. As is well-known, Popper recommends that new auxiliary hypotheses that are introduced after a falsification must meet the following requirements: (1) Their introduction must not reduce the empirical content of the theoretical system, but should increase it; (2) they should be independently testable, and (3) their introduction should be viewed as a rebuilding of the theoretical system. By contrast, Andersson argues that, for logical reasons, adding an auxiliary hypothesis to a theoretical system can never lead to a reduction of the empirical content of that system, and that therefore it is impossible to neutralize a falsification by adding new hypotheses to the premises of a falsification argument. Popper's first requirement is superfluous. It is not recommendable to require that the newly introduced auxiliary hypotheses should be independently testable. Such a requirement would *a limine* preclude modifications of the theoretical system that might prove profitable in terms of new knowledge. It is sufficient to regard the new auxiliary hypotheses as a modification of the theoretical system and to test them *as parts of the theoretical system*. The falsificationist methodology only requires that, after a falsification, the theoretical system be modified; and this by no means entails that according to falsificationist methodology a falsified theoretical system has to be 'rejected' *in toto* or 'abandoned'.

Thus, Kuhn's claim that in 'normal science' scientists react to a falsification by modifying the falsified system but not by abandoning it, is *consistent with* falsificationism. Therefore, many of Kuhn's case studies cannot function as 'counter examples' against falsificationism. Likewise, the strategy recommended by Feyerabend, *i.e.*, the interplay of the principle of proliferation and the principle of tenacity, is *compatible with* falsificationism. The conventionalist objections against falsificationism (which Lakatos accepted), namely the claim that falsifications can always be outmanoeuvred merely with the help of *ad hoc* hypotheses, is false. The only rational way of reacting to a falsification is by *modifying* the theoretical system. Of course, in any concrete case it is impossible *ex ante* to know whether modifying the theoretical system or completely remaking it is the better strategy. Only with the benefit of hindsight, only after having tested the empirical consequences that we have got by applying those strategies, can we tell which strategy has proved to be more profitable in the case at hand.

Feyerabend has based his thesis of the criticism-deflecting effect of *ad hoc* hypotheses on certain case studies. Andersson discusses in detail Galileo's introduction of new auxiliary hypotheses, *i.e.*, the *hypothesis that the telescope is*

a reliable instrument for astronomical observations, and his replacing the Aristotelean dynamics by different dynamical auxiliary hypotheses (circular inertia). Andersson shows that these auxiliary hypotheses did not function as devices that deflect criticism or neutralize a falsification. They modified the theoretical system, and equally importantly, they made explicit certain auxiliary hypotheses which so far had remained implicit—for instance the hypothesis that astronomical observations with the naked eye are reliable, and the assumption that the Aristotelean theory of motion is correct. The new auxiliary hypothesis about the reliability of the telescope can be regarded either as a part of the theoretical system to be tested or as an independently testable premise in the falsifying argument. The same holds, *mutatis mutandis*, of the new dynamical hypotheses that Galileo used to explain the tower experiment. Feyerabend regards Galileo's hypotheses as *ad hoc* in an objectionable sense, because Galileo introduced them after the alleged falsification of the traditional assumptions about the motion of the earth that the tower experiment was supposed to test, and he thinks that Galileo had introduced them with a view to deflecting the criticism against the Copernican system. However, for a methodological appraisal of a theory it is irrelevant when an auxiliary hypothesis has been introduced, and it is also irrelevant what psychological motives may have prompted the researcher to introduce it.

4 STOCKTAKING

The challenge to falsificationist methodology from examples culled from the history of science has proved a powerful incentive for efforts to solve the two methodological problems that underlie that criticism. The theory dependence of experience and the revisability of basic statements are in harmony with falsificationist methodology. They need not lead to relativism, because it can be shown how basic statements can be criticized. The so-called problem of incommensurability can be solved, or better dissolved, by the deduction of unproblematic test statements from problematic ones. The research strategy that Kuhn claims to be typical for 'normal science', i.e., reacting to falsifications by attempting to modify the theoretical system hit by the falsification but not totally 'abandoning' it, is in perfect agreement with falsificationist methodology. Falsificationist methodology only requires that a falsified theoretical system has to be changed in some way. It could not request that a falsified theoretical system be rejected in the sense that a researcher who tried to improve such a system could *eo ipso* be accused of having adopted an irrational investment strategy—investment of time and effort into a particular research project (see, e.g., Radnitzky [1987b], "... 'Economic Approach' ..."). The introduction of new auxiliary hypotheses modifies the theoretical system concerned, but that introduction need not be interpreted as a criticism-deflecting device. Falsificationist methodology cannot give any

advice as to the best way of reacting to a falsification, whether a minor modification of the old theoretical system or a complete replacement of it by a new one will lead to scientific progress. There is no 'logic' of scientific discovery in the sense of an algorithm.

In the 20th century, the philosophy of science has produced three important 'schools' or styles: positivism, Wittgenstein's later philosophy, and Critical Rationalism. Popper and Wittgenstein II are both critics of positivism. Popper criticized positivism much earlier than Wittgenstein did. Yet, Wittgenstein is still widely seen as the main critic of positivism.

The 'new philosophy of science' criticizes the positivist approach to the philosophy of science, and it wrongly views Popper as a fellow-positivist. It imputes that Popper believes 'basic statements' to provide an epistemological rockbottom and that falsifications definitely disprove the theory falsified, *i.e.*, that falsifications function like negative verifications. 'The new philosophy of science' keeps in the context of justificationist philosophy. Wittgenstein's later philosophy starts from 'practice'. In practice, the Ideal Language does not function. The starting point of the new philosophy of science is the practice of research as it is described and explained in the history of science—hence, the 'challenge to methodology from the history of science'. It comes to the conclusion that a study of the history of science shows that Critical Rationalism gives methodological recommendations or prescriptions that are unrealistic. In Lakatos's view the history of science falsifies falsificationism.

The main results of Andersson's investigations are the following:

- (1) The challenge to the philosophy of science from the history of science hinges on methodological considerations, in particular, on two fundamental methodological problems: the problem of how empirically to test theoretical systems, and the problem of how to criticize, how to 'test' basic statements (test statements).
- (2) The criticism brought forward by 'the new philosophy of science' is found to be partly justified. It draws attention to weak spots in the methodology of Critical Rationalism. Popper has only dealt with the empirical testing of isolated hypotheses—not of theoretical systems, which are what usually is at stake in the practice of research. One reason for this may be the fact that an isolated universal statement can be falsified with the help of existential statements ('there-is'-statements), and that it is relatively easy to show how this functions. It is by far more difficult to show how a theoretical system as a whole can be falsified. In Andersson's book that problem has been solved by demonstrating the metalogical equivalence of explanation, prediction, and falsification. This upgrading of the methodology of Critical Rationalism is completely new.
- (3) By showing how a critical discussion, an empirical testing of test statements, can be done, Andersson develops some ideas that Popper has

only sketched. He shows in detail how unproblematic test statements can be deduced from problematic ones with the help of auxiliary hypotheses. These statements are unproblematic in the sense that they can be intersubjectively tested so that it becomes possible that adherents to different 'paradigms' agree to such an unproblematic test statement. Hence, the so-called incommensurability problem has been solved, or perhaps more accurately speaking, has been dissolved. Then, Andersson tests his contentions by applying them in a detailed discussion of some of the 'classical' case studies which Kuhn and Feyerabend have submitted in support of the Incommensurability Thesis.

In summary, Andersson has solved two important methodological problems. This made it possible to meet the challenge to the methodology of Critical Rationalism from the history of science. The methodological problems have been solved by processing the methodology of Critical Rationalism. In its capacity to solve methodological problems the revised version of Critical Rationalism is far superior to both positivism and Wittgensteinian relativism. Refuting the claims of relativism with respect to methodology will have important implications for the discussion of relativism in moral and political philosophy. Critical argumentation has been shown to possess a greater problem solving capability than propaganda, persuasion, or other non-rational or irrational methods.

The challenge to falsificationism from the history of science has led to intellectual progress, i.e., to the processing of falsificationist methodology. Like Popper's classic of 1934 Andersson's book will make an impact only when it has become available in the new *lingua franca*. It is to be hoped that it will not have to wait for an English translation as long as Popper's volume.

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