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Historical-Critical Dictionary of Marxism

Falsificationism

A : [naẓarīyat] at-tazyīf. – G : Falsifikationismus.
F : falsificationnisme. – R : fal'sifikacionizm.
S : falsificacionismo. – C : zhengwei zhuyi 证伪主义.

'Falsificationism' names a group of various, often significantly different philosophical doctrines which derive, directly or indirectly, from the work of Karl Raimund **Popper** (1902–94), though it is also true that his thought has significant affiliations with elements in that of earlier epistemologists (cf. **Niiniluoto** 1978), and that **Popper** himself explicitly distanced himself from the term (1982, I, xxxi). The key element is the central place assigned to empirical refutability (rather than confirmability) in the philosophy of the sciences. The anchor-point of **Popper**'s work is his *The Logic of Scientific Discovery* (*Logik der Forschung*, 1935), though he re-presented and developed the ideas he first set out there in a long series of subsequent publications.

Falsificationism comes within the purview of Marxism firstly, and most directly, because **Popper** used it to try to show (mainly in *The Open Society and Its Enemies*, 1945) that Marxism is pseudo-scientific; and secondly, more indirectly, because of its claims as a general theory of knowledge, especially scientific knowledge. Consideration of falsificationism acquires special importance from the fact that in one or another of its forms (often vulgarised) it has had a world-wide resonance, especially among scientists and philosophically interested intellectuals; in particular, it has significantly influenced the ways in which many questions about the nature of social theory have been debated (see, e.g., **Maus** and **Fürstenberg** 1969).

1. **Popper**'s falsificationism can only be properly understood in terms of a certain given problem-situation to which it was a response. In what was in effect the first draft of *The Logic of Scientific Discovery*, namely, *Die beiden Grundprobleme der Erkenntnistheorie* (written 1930–3, first published in German in 1979; in English as *The Two Fundamental Problems of the Theory of Knowledge* in 2008), **Popper** identified these as: (1) the problem of induction: that of justifying the idea ('inductivism', as held by 'inductivists') that the basic form of empirical inference is that from empirical premises to empirical conclusions, where the latter are said to be evidentially 'supported' ('confirmed', etc.) by, but are not deducible from, the former; and (2) the 'demarcation problem [*Abgrenzungsproblem*]': that of distinguishing statements which are scientific/empirical (**Popper** uses the two terms interchangeably) from ones which are not. The traditional approach to these problems is, in effect, to assume that the solution to (2) is that what makes a statement scientific/empirical is that it is susceptible to inductive support; the crucial problem is then (1). However, **Popper** held that there is no such thing as inductive support (all genuine inference being deductive). His arguments for this position include technical ones (mainly from the theory of probability). His general conclusion is that inductivism entails epistemological scepticism (**Hume**). Hence for **Popper** problem (1) simply disappears, and is replaced by the problem of the nature of scientific/empirical inference. That is, in effect the crucial problem becomes (2).

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Popper's proposed solution to the latter is in two parts: (a) he introduces the term 'basic statement [*Basissatz*]' to designate a statement which asserts (truly or falsely) something observable about some spatio-temporally limited part of the world. Then a statement is scientific/empirical only if it is either a 'Basissatz' or logically inconsistent with ('falsifiable' by) a 'Basissatz' (its 'falsifier'). However, this is only a necessary but not also a sufficient condition for the solution of problem (2). For, as Pierre **Duhem** argued (1914, Chapter VI), a putative falsification of a statement can always be avoided, at least in principle, by conjoining the latter with certain other statements: there is no unconditional 'crucial experiment [*experimentum crucis*]'. Thus, (a) must be supplemented by condition (b): a statement ceases to be scientific/empirical if a falsification is avoided by making purely *ad hoc* adjustments, that is, ones made only to avoid that falsification and having no other consequences (the 'conventionalist stratagem', cf. *The Logic of Scientific Discovery*, §20). (a) and (b) may be called respectively the 'logical' and 'methodological' components of **Popper**'s *Abgrenzungskriterium*.

Popper calls a statement that is not analytic (one whose truth-value can be established simply by reference to the meaning of its constituent terms and principles of logic), but does not satisfy his *Abgrenzungskriterium*, 'metaphysical'. This account does not, like logical positivism, regard such expressions as simply meaningless. Indeed, **Popper** holds that they may and often do play a positive rôle in science (sometimes after appropriate reformulation) as procedural/regulative rules of inquiry (e.g., the principle of determinism – e.g., *The Logic of Scientific Discovery*, §12) or as the source of problems and heuristic guidance in solving them (see **Popper** 1982, III, §20, and **Agassi** 1964). However, they may be and often are objectionable insofar as they *pose* as scientific, but are 'pseudo-scientific' (in particular Marxism and psychoanalysis; see **Popper** 1984); hence the importance of solving problem (2).

2. *Selected problems.* Contemporary (see particularly **Neurath** 1935) and later criticism (e.g., **Feyerabend** 1975, Chapter 15; **Hübner** 1979; **Lakatos** 1978, I, Chapter 1; **Schilpp** (ed.) 1974, which contains **Popper**'s 'Reply to Criticisms') pointed to a number of decisive inadequacies in this position, some of which are the following.

Anti-inductivism and scepticism. If the only genuine empirical inference is from a *Basissatz* to the *falsity* of another (in particular, universal) statement, then a *failed* attempt at falsification gives no empirical ground whatever for holding that unfalsified statement to be *true*. To think otherwise would be to fall into the error of 'inductivism'. Furthermore, a *successful* attempt at falsification gives no empirical ground for holding that some *other* statement is true, for there is an in-principle unlimited number of statements consistent with the falsifying *Basissatz*. (Apart from this, an apparently falsifying *Basissatz* may later turn out to be itself false – acceptance of such is ultimately a matter of 'decision', cf. *The Logic of Scientific Discovery*, §29 – and different tests may succeed in falsifying a statement which has survived attempts to falsify it.) So falsificationism falls prey to the very scepticism it was partly designed to avoid. (**Popper**'s radically 'anti-psychologistic' account of *Basissätze*, *The Logic of Scientific Discovery*, Chapter V, also has arguably sceptical implications; see **Hindess** 1977.) Popper attempted to avoid this objection by introducing the idea of 'corroboration [*Bewährung*]' (*The Logic of Scientific Discovery*, Chapter X), a degree of which is to be attached to a statement according to the severity of the attempts made to falsify it. But, arguably, this cannot be a truly objective measure, and, in any case, would seem to be only another name for inductive evidence. Similar considerations apply to his later concept of 'verisimilitude' (e.g., 1982, I, §4).

Pseudorationalism. Otto **Neurath** (1935, 356) charged **Popper** with not taking into account the ambiguity of the sciences. They are encyclopædias and not systems. Thus, it is false to act on the assumption of systems of clean statements as the foundation of observation. 'Thus we not only contest that there could be general methods of "induction" for the real sciences, but also that there could be general

methods of "control" – but it is precisely the possibility of such general methods of "control" that is proposed by **Popper**'.

Problems with the demarcation criterion. These problems have a (a) logical and (b) methodological component. – (a) Many statements which are generally (and rightly) regarded as scientific/empirical are not falsifiable in **Popper**'s sense. This is true of all those which have the logical form of the principle of the conservation of energy, which asserts that *every* change in the magnitude of energy at a given place and time is associated with *some* fully compensating change in the magnitude at another place and time. For a negation (falsifier) of the 'some' clause is logically equivalent to a strictly universal statement, which cannot be exhaustively verified, and so cannot be established. This problem also arises, *inter alia*, with regard to probability statements on a 'frequency' interpretation which involves the idea of a limit-value of an infinite sequence of numbers (see, e.g., *The Logic of Scientific Discovery*, §66). – (b) In general, actual scientific theories are never sufficiently formalised to permit a clear knowledge of all the consequences of changing one part of it to deal with a putative falsifier. Case studies show that sometimes the use of a 'conventionalist strategem' pays off in the longer run, and indeed that all of **Popper**'s rules have been fruitfully violated at some time in the history of the sciences.

3. *Lakatos's 'Methodology of Scientific Research Programmes'* (*MSRP*). – **Lakatos** took his point of departure from the final criticism of **Popper** outlined above. According to **Lakatos**, the proper unit for evaluation as to scientificity/empiricalness is not a single theory or even a set of alternative theories considered in themselves at a particular time, but a 'scientific research programme' (SRP). This consists of a 'hard core' of fundamental principles ('positive heuristic'), and a 'protective belt' of ways of dealing (in a non-conventionalist way) with putative falsifications of said fundamental principles ('negative heuristic'), both of which generate a sequence of particular theories. A SRP is evaluated by reference to whether, first, in the more-or-less long-term, it exhibits a 'progressive' or a 'degenerating' 'problem-shift'; that is, roughly, whether it produces or fails to produce new knowledge rather than just accommodating anomalies through more-or-less *ad hoc* adjustments. Second, a SRP is evaluated by comparing it in this regard with competing ones.

Critics (e.g., in **Cohen** et al. (eds.) 1976; **Feyerabend** 1975, especially Chapter 16, and 1981; **Hacking** 1979) have raised a number of fundamental objections to the MSRP. One is that the MSRP can free **Popper**'s version of falsificationism from its rigidity only at the cost of a lack of *effective* decision-procedures for applying its criteria of empiricalness/scientificity, whereas **Lakatos** emphasised that his aim was to produce a normative/prescriptive (rather than merely descriptive or conventionalist) theory of the proper conduct of scientific inquiry. Another objection claims that, despite case studies by **Lakatos** himself (e.g., 1978) and by co-workers (e.g., **Howson** (ed.) 1976; **Zahar** 1989), the MRSP is not consonant with the history of successful science. In his original presentation, **Lakatos** admitted that his examples from the history of science were not always literally faithful to the facts; where they were not, they were to be regarded rather as 'rational reconstructions' of such episodes; that is, accounts of what *would* have happened if the practioners had been acting in a fully rational way, namely, in accordance with the MSRP. However, critics objected that this was arguing in a circle. Both **Lakatos** and his co-workers have attempted to develop the MSRP to take account of such objections. However, as, for example, **Musgrave** (in **Cohen** et al. (eds.) 1976, 457–91) has pointed out, this has ended up in the construction of more and more complicated, *ad hoc* methodological epicycles, which amount to what, in the language of the MSRP itself, is a 'degenerating problem-shift'.

4. Problems for falsificationism such as those canvassed above are familiar in the literature of mainstream epistemology. The specific contribution of a Marxist approach is to locate their basic sources. From the point of view of a Marxist epistemology, the basic

defect of falsificationism is that it is fundamenally committed to the traditional conception that the main task of the philosophy of knowledge is to ascertain the fixed (general and absolute) normative principles according to which a rational subject allegedly proceeds in representing what is objectively the case. This assumes, *inter alia*, that 'science' is a methodologically homogeneous category. Amongst other things, this generates epistemological 'closures', that is, representations of what are, at best, historically and theoretically contextually valid procedures as uniquely, absolutely valid ones, something which blocks the growth of knowledge. (**Gramsci**'s warning against this is especially apposite; cf. *Q* 11, §15). In the case of falsificationism, a basic closure involves the absolutisation of the 'hypothetico-deductive' (HD) model of inquiry; that is, the method of advancing knowledge by proposing general hypotheses and deducing from them observationally testable consequences.

However, even in the physical sciences the HD model does not have unrestricted validity. For instance, this approach obscures the significance of the processes – and in the first place *experimental* ones – by which hypotheses are *generated*, and hence too the *evidential* significance of what is learned at this stage, evidence independent of what is furnished by tests (see, e.g., **Nickles** 1989). Even within physics, the area of natural science which falsificationism regards as paradigmatic of the latter, inquiry does not always proceed by the use of theories with a deductive structure. For instance, the qualitative study of unstable periodic behaviour in deterministic nonlinear dynamical systems ('chaos theory') makes indispensable use of computer simulations (see, e.g., **Kellert** 1993, 91ff., 102ff.).

Outside of physics, but still within the area of the natural sciences, in cosmology at one end of the scale and geology at the other, and furthermore in macro-biology, there is no question of strictly deductive theory. Moreover, tests in the strict sense envisaged by falsificationism are only possible in systems which are optimally isolated (naturally or experimentally) from causal interference by factors irrelevant to the test, and since this condition is not satisfied in the domain of 'natural history', the HD model is at best of limited significance in areas belonging to the latter (see **Gould** 1989, **Sober** 1988). So the basic inapplicability of this model to human history does not preclude the latter from having, in principle, the status of a science. In the preceding areas at least, **Freud**'s likening of explanation to doing a jigsaw puzzle is more to the point (**Freud** Volume 3, 205 and Volume 19, 116).

5. *'Metaphysics' and science.* – **Popper**, **Lakatos** and others are right in pointing to the pervasive presence in the history of scientific knowledge of various 'metaphysical' assumptions. However, whether that influence is positive or negative is in general an historically and theoretically contextual matter. Moreover, there is no 'royal road' to their identification, such as falsificationism seeks to provide. To take a pertinent example, **Marx** showed that the ultimate source of the inability of classical political economy to solve the problem of the origin of surplus-value (due to its inattention to the form as opposed to the quantitative aspect of the relation of commodity-exchange) was its wholly implicit assumption that social production is inherently capitalist in character (*MECW* 35, 91). A necessary condition for **Marx**'s being able to make this assumption explicit was the existence of a workers' movement which, by its struggle for an alternative (socialist/communist) organisation of production, made plain the special character of capitalism. In brief, the identification of the key 'epistemological obstacle' (cf. **Bachelard** 1938) to the solution of the problem in question was a result of an intersection of intra-theoretical considerations (incoherencies and contradictions in the classical account), and conjunctural social developments (for related discussions, see **Balibar** 1994; **Baltas** 1986; **Canguilhem** 1977). As **Brecht**'s Galileo says (*GW* 3, 1304), the aim of science is not 'to open a door for infinite wisdom, but rather, to set a limit to infinite error'. But in the present case, as elsewhere, this is a matter of material-theoretical practice, not of *a priori* philosophical nostrums.

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Wal Suchting

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