Popper, Basic Statements and the Quine-Duhem Thesis

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Abstract

In this paper I explore Karl Popper's 'critical rationalism', focusing on its presuppositions and implications as a form of realism regarding the nature of scientific truth. I seek to reveal an underlying tension in Popper's thought pertaining to his account of basic statements and the related question of whether the falsification of a universal theory can ever justifiably be regarded as final or conclusive. I conclude that Popper's account of basic statements is implicitly conventionalist, and that it should, in consistency, have forced him in the direction of Quinean holism.

1. The nature of science

Popper's main enterprise was to construct a model of scientific rationality which embodied an account of the logical relationships that obtain between theoretical and observation statements in science, and, associated with this, a prescriptive methodology. He was an epistemological fallibilist who recognized that the 'central problem of epistemology has always been and still is the growth of knowledge', but who held that the growth of knowledge '*can be studied best by studying the growth of scientific knowledge*'.[1] Consequently, his concern with the problem of demarcation in philosophy of science was intended to be seen as having the most wide-ranging philosophical implications, since he proposed that epistemology 'should be identified with the theory of scientific method'.[2] He described how the issue of demarcation relates to his own general objectives as follows:

> [My] business, as I see it, is not to bring about the overthrow of metaphysics. It is, rather, to formulate a suitable characterization of empirical science, or to define the concepts 'empirical science' and 'metaphysics' in such a way that we shall be able to say of a given system of statements whether or not its closer study is the concern of empirical science.[3]

[1] Karl Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), p. 15.
[2] Popper, *The Logic of Scientific Discovery*, p. 49.
[3] Popper, *The Logic of Scientific Discovery*, p. 37.

Demarcation, then, is the problem of distinguishing the empirical sciences from non-empirical areas such as logic, pure mathematics, and metaphysics, as well as 'pseudo-sciences' such as astrology and phrenology. Popper's approach to formulating a criterion of demarcation was non-naturalistic or prescriptive, the offering of 'a proposal for an agreement or convention'.[4] For him, the line between science and metaphysics was to be drawn by agreement and decision, not by discovery. Such an agreement, however, must be informed by both logical and methodological considerations, and while he acknowledged that 'a reasonable discussion of these questions is only possible between parties having some purpose in common',[5] he was optimistic about the possibility of rational assent on the part of those who share his goal of adequately characterizing empirical science.

Popper rejected the naturalistic approach to the problem of demarcation taken by the logical positivists, and counter-pointed that with his repudiation of their view that science is characterized by its inductive methods, in which universal laws are supposedly inferred from a set of singular statements such as experimental observation-reports. This is the traditional observationalist-inductivist paradigm of scientific investigation, the repudiation of which links the problems of induction and of demarcation in Popper's philosophy. His case against it rests upon three central contentions. First, there are no 'pure' or theory-free observations. 'Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem'.[6] As such, it is theory-laden, and involves the application of theoretical terms, a descriptive language and a conceptual scheme to particular experiential situations. Second, scientific laws are strictly unverifiable. All such theories are universal in nature, and no finite collection of observation statements, however great, is logically equivalent to, or can justify, an unrestricted universal proposition. Third, induction, conceived of as a system of logical inferences which generates scientific law from the particularity of experimental results, is 'a myth'.[7] Such inferences, Popper held, play no role in scientific investigation or in human life generally.

[4] Popper, *The Logic of Scientific Discovery*, p. 37.
[5] Popper, *The Logic of Scientific Discovery*, p. 37.
[6] Karl Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge, 1963), p. 46.
[7] Popper, *Conjectures and Refutations*, p. 53.

At one level, then, Popper concurred with Hume's critique of induction. However, the crucial counterpart of the latter, which he believed Hume himself had missed, is that while no number of positive outcomes at the level of experimental testing can demonstrate the truth of a scientific theory, a single genuine counter-instance is logically decisive. 'Hume showed that it is not possible to infer a theory from observation statements; but this does not affect the possibility of refuting a theory by observation statements'.[8] By the canonical *modus tollens* rule of classical logic, it is possible to deductively infer the falsity of a universal proposition once the truth-value of an appropriately related singular proposition is established. For Popper, this means that the only kind of inferences involved in science are deductive ones, from observation-reports to the *falsity* of the corresponding universal hypotheses, and inferences occur not in the generation, but in the critical testing, of such hypotheses.

Accordingly, Popper's view of the relationship between scientific theory and experience was both anti-inductivist and anti-Humean: theory is not logically derived from, nor can it be confirmed by, experience, though experience can and does delimit it. He argued that human knowledge generally, including scientific theory as one of its most refined forms, is both fallible and wholly hypothetical, and is produced, not by logical inference, but by the creative imagination. The central rational activity in science is that of problem-solving, whereby new hypotheses are imaginatively projected to solve problems which have arisen with respect to a pre-existing theoretical framework, a process which may be retrospectively retraced though 'more and more primitive theories and myths … [to] unconscious, inborn expectations'.[9] The critical role of experience in science is to show us which theories are false, not which theories are true. However, a theory that has successfully withstood critical testing is thereby 'corroborated', and may be regarded as being preferable to falsified rivals. In the case of rival non-falsified theories, for Popper, the higher the informative content of a theory the better it is scientifically, because every gain in content brings with it a commensurate gain in predictive scope and testability. For that very reason, he propounded the view that a good scientific theory will be more *improbable* than its rivals, because the probability and informative content of a theory vary inversely: 'If our aim is the advancement or growth of knowledge,

[8] Popper, *Conjectures and Refutations*, p. 55.
[9] Popper, *Conjectures and Refutations*, p. 47.

then a high probability (in the sense of calculus of probability) cannot possibly be our aim as well: *these two aims are incompatible*'.[10]

Popper's perception of the asymmetrical logical relation which exists between verification and falsification, it is thus not too much to say, lies at the heart of his philosophy of science: a universal scientific theory cannot, in principle, be verified, but a single counter-instance can and does decisively falsify it. Accordingly, he held that, from a logical perspective, a system of theories is scientific only if it is refutable or falsifiable:

> I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that can be singled out, by means of empirical tests, in a negative sense: *it must be possible for an empirical scientific system to be refuted by experience.*[11]

This demarcation criterion was most clearly defined by Popper in terms of the relation that holds between a scientific theory and 'basic statements', where the latter are to be understood as singular existential observation-reports of the form 'There is an X at Y'. On this definition, where a theory is scientific, it must exhaustively divide the class of basic statements into two non-empty subclasses. These are (1) the class of basic statements that are consistent with the theory, or which the theory 'permits'; and (2) the class of basic statements that the theory 'prohibits' or rules out. The latter class—which, in Popperian terms, is by far the more important of the two—is the theory's *potential falsifiers*, since the truth of any one of them implies the falsity of the theory. In short, for Popper, 'a theory is falsifiable if the class of its potential falsifiers is not empty'.[12]

2. Logical and methodological falsificationism

Popper was aware that there is a significant disparity between precision of the logical analysis of statements contained in his demarcation criterion and the complex, heterogeneous nature of actual scientific practice. Since observation is, as he insisted, itself fallible, it is always possible to question an experimental result. His account of the

[10] Popper, *Conjectures and Refutations*, p. 218.
[11] Popper, *The Logic of Scientific Discovery*, pp. 40-41.
[12] Popper, *The Logic of Scientific Discovery*, p. 86.

logic of falsifiability was thus tempered by an explicit recognition that scientific theories are often retained in the face of conflicting or anomalous empirical evidence, and that, in actual scientific practice, a single conflicting or counter-instance is never sufficient to force the repudiation of an established theory. He was also cognizant of the fact that, against dogmatic or uncritical advocacy, 'no conclusive disproof of a theory can ever be produced'.[13] In short, he recognized that a logical analysis of statements alone is not sufficient to recapitulate the unique character of empirical science.

To achieve that objective, Popper concluded, it is necessary to embed falsifiability in a normative methodology, which, in this connection, relates to decisions which must be made as to how to deal with scientific statements, which in turn are determined by our aims. For Popper, the aims of elucidating empirical science and constructing a model of scientific rationality bring with them the need to adopt a set of rules that will 'ensure the testability of scientific statements; which is to say, their falsifiability'.[14] Accordingly, the 'supreme rule' associated with the falsifiability criterion, which functions as a norm with which all other methodological rules must accord, is 'the rule which says that the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification'.[15] This rule prohibits, as unscientific, *ad hoc* reformulation of theory to meet contradictory evidence. The recognition of the hypothetical and fallible nature of human knowledge, and, with this recognition, the willingness to subject even one's most cherished theory to a critical test which could conceivably show it to be false became, for him, the defining characteristic of the true scientific mentality:

> The wrong view of science betrays itself in the craving to be right; for it is not his *possession* of knowledge, of irrefutable truth, that makes the man of science, but his persistent and recklessly critical *quest* for truth.[16]

3. The Quine-Duhem Thesis and holism

Quine (1953), following Pierre Duhem, and Lakatos (1970) have

[13] Popper, *The Logic of Scientific Discovery*, p. 50.
[14] Popper, *The Logic of Scientific Discovery*, p. 49.
[15] Popper, *The Logic of Scientific Discovery*, p. 54.
[16] Popper, *The Logic of Scientific Discovery*, p. 281.

both attacked the key Popperian notion that the falsification of scientific theories can be yielded by discrete critical tests, arguing strongly for holistic conclusions against it. Quine, who called into question the assumption of a clear-cut distinction between analytic and synthetic statements as one of the two key 'dogmas of empiricism', also vigorously advocated a holistic view of empirical testing, contending that 'our statements about the external world face the tribunal of experience not individually but only as a corporate body'.[17] On this view, the entire system of human knowledge impinges upon experience 'only along the edges',[18] hence contradictory empirical evidence has no necessary connection with any given theory, and may in fact force a re-assessment of a range of elements within the system. For Quine, scientific theories are *underdetermined* by physical evidence, i.e., no physical observation can ever *decide* between two theories, because each theory can be revised to accommodate the new evidence by adding new auxiliary hypotheses. For this Reason, The Q-D Thesis is sometimes called 'the Underdetermination Thesis'.

In a similar vein, Lakatos argued that many of the most respected scientific theories cannot be refuted by individual critical tests, as they are not in themselves prohibitive, but rather '*forbid an event occurring in some specified finite spatio-temporal region… only on the condition that no other factor … has any influence on it*'.[19] Such theories, in other words, require the addition of an implicit *ceteris paribus* clause if they are to have prohibitive implications at all, and the *ceteris paribus* clause can always be replaced by another to make the theory consistent with apparently falsifying evidence. Hence theories are not falsified in isolation, Lakatos contended, but as integral elements of a 'degenerating research programme' which is supplanted by a rival programme possessing equal predictive success and additional 'heuristic power'.[20]

Popper's response to these and related criticisms was to emphasize the significance of assumed background knowledge in the encounter between theory and experimental results, acknowledging that such assumed knowledge is fully as open to challenge and revision as the

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[17] W.V.O. Quine, 'Two Dogmas of Empiricism' in *From a Logical Point of View* (Cambridge, Mass.: Harvard University Press 1953), p. 41.
[18] Quine, 'Two Dogmas of Empiricism', p. 42.
[19] I. Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in *Criticism and the Growth of Knowledge,* edited by I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1970), p. 101.
[20] I. Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', p. 155.

theory which its tentative acceptance permits us to test. He argued, however, that such considerations do not entail holistic conclusions, as in many cases it is quite possible to determine which hypothesis or group of hypotheses 'is responsible for the refutation'.[21]

4. Basic statements and conventionalism

While this reply appears plausible, I wish in conclusion to argue that Popper's account of basic statements should have forced him in the direction of holism. Popper follows Kant in repudiating the positivist/empiricist view that basic statements are infallible, and argues instead that such basic statements are not mere 'reports' of passively registered sensations. Rather they are descriptions of what is observed as interpreted by the observer with reference to a determinate theoretical framework. This is why Popper repeatedly emphasises that basic statements are not infallible, and it indicates what he means when he says that they are 'theory laden'—perception itself is an active process, in which the mind assimilates data by reference to an assumed theoretical backdrop. He accordingly asserts that basic statements themselves are open-ended hypotheses: they have a certain causal relationship with experience, but they are not *determined* by experience, and they cannot be *confirmed* by experience.

This is extremely important, as it differentiates Popper's position from that of the logical empiricists (Russell and the early Wittgenstein) and of a number of the logical positivists, such as Schlick, Neurath and Carnap (with whose positions Popper's was and is sometimes confused), all of whom advocated, in one way or another, that what were variously termed 'basic', 'simple', 'elementary' or 'protocol' statements are the ultimate and incorrigible verifiers of scientific theory.

However, this poses a difficulty regarding the consistency of Popper's theory: Methodologically, in order to actually test a scientific theory it must be possible to determine whether or not the basic propositions which would, if true, falsify it, are *actually* true or false (i.e. whether its potential falsifiers are actual falsifiers). But how can this be known, if such basic statements cannot be verified by experience? The nature of this difficulty can be illustrated by reference to the logical considerations underlying Popper's account of demarcation. In prescribing his falsificationist methodology, Popper's construal of the

[21] Popper, *Conjectures and Refutations*, p. 239.

logic of testing is on the model of the *modus tollens* inference, which—assuming the existential import of a universal theory—is as follows:

> If theory A is true, then X (a singular proposition) is true.
> And X is false
> Therefore, theory A is false.

Hence Popper's claim that our starting point has to be the recognition that, whereas we can never, even in principle, establish a universal statement to be true, we can, in principle, establish it to be false.

Given that the problem of confirmation/justification/falsification originates in the recognition that universal laws or statements of science are not themselves confirmable (i.e. that we cannot know that a universal theory is or is not true), methodologically the focus necessarily shifts to the singular propositions which constitute what Popper terms its 'potential falsifiers'. How can we know whether these are true/false, and thus put them to work in the testing of universal statements? Oddly (or perhaps ironically) enough, this is not a question which would have caused a Hume or a Schlick a moment's hesitation, since on their analysis basic propositions are reports of direct experience, and as such are impervious to error. It is precisely Popper's (in my view, justified) rejection of such crude empiricism that rules out such an answer for him, and throws up again the crucial methodological question as to *how basic statements can be known*. In terms of the (oversimplified) *modus tollens* construction given above, how, if ever, can we know that 'X is false'? His answer is that we *cannot*: 'basic statements are not justifiable by our immediate experiences, but are .... accepted by an act, a free decision'.[22]

This seems to be a sophisticated form of conventionalism—the only condition for the acceptance of a 'contradictory' basic statement is that it should simultaneously corroborate a falsifying hypothesis: "If accepted basic statements contradict a theory, then we take them as providing sufficient grounds for its falsification only if they corroborate a falsifying hypothesis at the same time'.[23] Corroboration, however, is invariably provisional, open-ended and revisable, which entails that falsification can never be conclusive. This is a conclusion which he sometimes affirms:

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[22] Popper, *The Logic of Scientific Discovery*, p. 109.
[23] Popper, *The Logic of Scientific Discovery*, p. 110.

> It is impossible ... that any theoretical system should ever be conclusively falsified;[24]
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> No conclusive disproof of a theory can ever be produced;[25]
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> …we may have made a mistake when we accepted the singular statement ... and, for this reason, the falsification of the theory *is not* 'absolutely certain'.[26]

But it is a conclusion which he also frequently also denies:

> Every time we succeed in falsifying a theory …[27]
>
> If we renounce this requirement [verifiability] and admit as empirical also statements which are decidable in one sense only—unilaterally decidable, and more especially, falsifiable … the contradiction disappears.[28]

However, it is the suggestion *alone* that theoretical falsification can sometimes be conclusive and final, and be known to be conclusive and final, that has given Popperian falsificationism its longstanding appeal, and has separated it from the 'underdeterminationism' of those who, like Quine and Lakatos, have argued for holistic conclusions. There is also a question as to whether it is methodologically sounder to accept, on the basis of a free decision, the falsity of a basic proposition in order to *infer* the falsification of a universal law rather than to accept *directly*, on the basis of a free decision, the universal law itself? No doubt, answers in the affirmative may suggest themselves in terms of decision-theory and the rationality of pragmatic choice, and I do not wish do be understood as dismissing these *a priori*. But it seems very difficult to reconcile this with Popper's view that science progressively moves closer to the truth, conceived of in terms of the correspondence theory, and more difficult still to see how Popper can thereby resist holism (this might explain the relative weakness of his responses to Quine, Lakatos and Putnam in the Schilpp volume).

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[24] Popper, *The Logic of Scientific Discovery*, p. 42.
[25] Popper, *The Logic of Scientific Discovery*, p. 50.
[26] Karl Popper, *Realism and the Aim of Science: From the Postscript to the Logic of Scientific Discovery*, edited by W.W. Bartley III (London: Hutchinson, 1983), p. 186.
[27] Karl Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Clarendon Press, 1979), p. 196.
[28] Popper, *The Logic of Scientific Discovery*, p. 42.

Bluntly stated, either Popper retains his account of basic statements and is forced to accept pragmatic holism of a form advocated by Quine or Lakatos, or he retains his opposition to holism by abandoning his account of basic statements. He cannot, in my view, consistently retain both.[29]

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[29] Part of this paper was also published as the entry 'Popper, Karl Raimund' in *The Philosophy of Science: An Encyclopedia*, edited by S. Sahotra & J. Pfeifer, 2 Vols. (London: Routledge, 2006), pp. 571-78.