Proper recognition came belatedly to the work of Karl Popper. The novelty and power of his comprehensive philosophy went largely unnoticed for decades, his views being misapprehended, to the extent they were apprehended at all, as an uncomprising variation on the dominant Positivist theme. In the past two decades, however, recognition has become widespread. He can lay claim to being the initial figure in a vital and flourishing tradition in the contemporary literature; his views find expression even in undergraduate curricula; and he finds himself the welcome subject of a two-volume issue in the Schilpp series, The Library of Living Philosophers. The appearance of this collection is the occasion of this notice.

Unfortunately, the spectre of misapprehension surrounds Popper still, and haunts what should have been some of the most interesting pages in this collection. At issue here is Popper’s conception of rational scientific methodology, a conception which has received substantial criticism from inductivist and anti-inductivist quarters alike. Partly from conviction, and partly for the sake of argument (Popper is an anti-inductivist), I shall adopt the latter viewpoint in this essay. My sympathies, however, will be with his critics.

Popper contends that there is a crucial distinction between the empirical (the scientific) and the non-empirical (the metaphysical). The distinction, as Popper draws it, is not at all what it was in the hands of the Positivists — its significance is exhaustively methodological, rather than ontological or semantical — but I suggest that the attempt to draw it, and to exploit it, is nonetheless an unfortunate legacy of the Positivist period. I shall argue below that, to the extent that it is clear,

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1 The Philosophy of Karl Popper, books I and II, edited by Paul A. Schilpp (Open Court, 1974).
the distinction is exceedingly superficial and does not deserve the pride of place Popper assigns it. The guiding suspicion is the idea that the better and more interesting (powerful, enlightening, potentially revolutionary) our theories become, the harder it is to subject them to crucial, unambiguous, observational test — in Popper’s sense of “test,” not just in the sense of effort and money required. Accordingly, any philosophy of science which makes ‘degree of testability’ the central measure of (a priori) theoretical virtue must be seriously awry somewhere.

It is necessary to begin the discussion with a brief review of Popper’s position. His criterion for the empirical is falsifiability (testability, refutability). A statement or system of statements is empirical just in case it is falsifiable; otherwise it is metaphysical. Our interest therefore shifts to the notion of falsifiability, but here there is a problem. A clear, complete, non-metaphorical, and utterly unambiguous definition of this notion is surpassingly difficult to find in Popper’s writings. Not that this is the initial impression — quite the contrary. The most straightforward interpretation of the clearest of the many characterizations provided yields the following definition.

(A) A statement or set of statements T is falsifiable if and only if T is logically inconsistent with some possible or conceivable basic statements.2

[Basic statements are singular existential statements — simplest paradigm: (3x) (Fx & x is at point k) — which meet the material condition of being observational.3 The full story on basic statements is both substantial and important, but this will serve present purposes.] Definition (A) is certainly a common construal of Popper, and it may appear to serve one well as one proceeds into the Popperian architectonic. But it cannot be right. That is, one can ascribe (A) to Popper, the cited passages notwithstanding, only at the price of rendering him inconsistent with his views on the testability of basic statements, and with his frequently repeated remarks to the effect that the metaphysical status of an hypothesis can change as a function of changes in our background knowledge. Moreover, Popper’s replies to some of his critics, to Kneale and to Lakatos in particular, will be


3 See L.Sc.D., chapter V, esp. section 28; also, Conjectures and Refutations, pp. 386-88.
rendered unintelligible if one interprets his comments through the lens of definition (A).

I will not contend, however, that the claims just made are transparent and incontestable, that any careful reader of Popper must come quickly to the same determination. On the contrary, I am impressed (as I think any careful reader will be) with the murkiness of Popper's expositions on this point, and with the exasperating variety of interpretations they permit and invite.

Just the same, a strong case can be made for withdrawing (A), and for replacing it with a specific alternative. The first consideration concerns the testability of basic statements (test statements, observation statements). Given the logical form of basic statements, any basic statement will be logically consistent with almost every other. The statement "There is exactly one thing at k and it is red" will be logically inconsistent with "There is exactly one thing at k and it is not red," but that about exhausts it. This is enough, perhaps, to make the former statement empirical (just barely), but it hardly represents a plausible mode of testing the former statement. To generate a non-trivial test of any basic statement, it is necessary to conjoin a universal statement with it, a fact Popper readily acknowledges:

"... even a singular statement like 'Here is a glass of water' is testable only because we assume that glasses, and water, exhibit a... law-like behaviour." (the Schilpp volume, p. 989)

The inconsistency Popper desires between hypotheses and possible basic statements would appear to be material rather than formal.

The second consideration is his view that metaphysical hypotheses can become testable, a view that is difficult to square with definition (A). Relevant here is the following passage.

"Take the singular historical statement: 'Napoleon dreamt vividly, on the night before Waterloo, of his impending defeat, but when he woke up he could not remember anything of his dream, and he never recalled it; nevertheless, unconsciously, his vivid dream shook him so badly that he was not himself during the battle'. I do not see any possibility of ever testing this statement, and I should regard it as untestable, at least in our present state of knowledge; but I could imagine (though I do not for a moment believe it will ever happen) an advancement of our knowledge which could turn even this wild statement into a testable statement." (Schilpp, p. 989)

Testability, then, is relative to background knowledge, and both of the above considerations agree in suggesting the following definition:

(B) A statement or set of statements T is falsifiable if and only if the conjunction of T with what can be taken as unproblematic background knowledge A is formally inconsistent with some possible or conceivable basic statements (where A alone is formally consistent with them).

If we accept this definition, as expressing Popper's view, we can also
understand his preference, in his later writings, for the more general term “clash” in stating the desired relation between theory and possible observations. In any case, I shall proceed on the assumption that (B) does represent Popper’s understanding of the term “falsifiable.” From this point, Popper’s methodological prescriptions are readily outlined.

(i) Seek to develop and pursue theories which are falsifiable, and the greater the empirical content, the better.4
(ii) Seek out observations which might falsify the theory; seek refutations rather than ‘confirmations’.
(iii) When faced with ‘falsifying’ observations, it is an unacceptable policy to protect the theory from refutation by the introduction of ad hoc modifications or assumptions, of modifications or assumptions which reduce the empirical content of the theory, for that way lies metaphysics.

With these broad strokes we are presented with an appealing methodological picture: science proceeds with maximum rationality only when it proceeds in accordance with these directives.

But the appeal of this picture is compromised somewhat by the appeal of a competing picture of science, some central elements of which are skillfully outlined in the present context by Putnam5 and Lakatos6. In essence, their claim is that most of our fundamental theories are frankly metaphysical, by Popper’s criterion, and that the bulk of rational research is spent not in search of refutations, but in search of systematic subsidiary understanding of how the fundamental theory at issue applies (variously) to the many different kinds of empirical situations on which it promises to throw light. The simplicity and generality we rightly seek in our fundamental theories, combined with the great variety and great complexity of the empirical

4 “...The empirical content of a theory is determined by (and equal to) the class of those observational statements, or basic statements, which contradict the theory.” (Conjectures and Refutations, p. 385). We may note here the same difficulty discussed above. Does “contradict” here mean “formally contradict,” period, or “formally contradict in the context of unproblematic background knowledge?” The natural reading is the former, but on the reasonable assumption that T is falsifiable if and only if the empirical content of T is not empty, this leads us back to definition (A). But (A) renders Popper’s position incoherent. My conclusion is that Popper’s use of “contradict” conforms to the latter reading.

5 “The ‘Corroboration’ of Theories,” Hilary Putnam (the Schilpp volume, pp. 221-240).

phenomena to be penetrated with their help, renders it inevitable that
so much research effort — it will be ‘theoretical’ as well as experimen-
tal — is spent in the manner described. And in general, the expec-
tations we develop with the theory’s help are, when observationally
frustrated, construed as failures of application, not as refutations. A
fundamental theory may provide only a strategy, as it were, for
detailed empirical understanding, leaving the tactics to be worked out
in the field. And tactical failures need be no more than that. The exam-
ple stressed by both authors is Newtonian dynamics, here construed as
consisting of the gravitation law plus the three laws of motion.

The view just outlined is of course hardly unique to Putnam and
Lakatos, and so one turns to Popper’s replies with some interest. But
his replies (Schilpp, pp. 993-1013) to the contributions of these two
authors are not as sensitive or as responsive as they might have been.

With a good deal of table-pounding, Popper insists (i) that Newtonian
dynamics is falsifiable, and (ii) that both authors have failed badly to
understand his position. To some extent this is understandable. Both
Putnam and Lakatos appear to be operating on the assumption that
definition (A) captures Popper’s understanding of falsifiability, and
there are other lapses as well. But Popper’s replies themselves radiate
rather more heat than light, and after all is said and done one is left
with the impression that the basic challenge posed here has gone un-
met.

The challenge posed is clear enough. To frame it in our terms, the
claim is that there are (or at least could be) legitimate, powerful, scien-
tific theories which are not themselves inconsistent with any basic
statements, and where logical inconsistencies with basic statements
can be generated only by conjoining the theory at issue with singular
auxiliary assumptions which are themselves neither basic statements
nor a part of our unproblematic background knowledge. Any theory
meeting the business end of this characterization would be in-
contestably unfalsifiable, on either of the two definitions discussed
above.

That a powerful theory might meet this characterization is not out
of the question. If a universal theory introduces a novel ontology or
categorial scheme of non-observational items and/or properties; if
the relations between these novel categories and the familiar observa-
tional categories are not known or appreciated in any exhaustive
way; then applications of the theory in specific cases will require the
imposition of a singular description in the language of that theory
which is in every sense a speculative interpretation of the initial em-
pirical conditions. This is not to imply that the formulation of singular
auxiliary hypotheses in such cases must be pure mad guesses or utter
stabs in the dark (though in some cases they may be). The explanatory
goals for which the theory was conceived and the world-picture
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provided by the theory itself will provide at least some rough counsel concerning interpretations of the kind at issue. The point to be emphasized is that in such cases the singular auxiliary assumptions (which make a 'test' of the theory possible) are as much under test as is the theory itself. Even if, therefore, the conjunction of such an auxiliary with the theory has observational consequences, such 'quasi-tests' are not genuine tests in the sense of (B); the theory runs no risk of falsification in that sense, for the auxiliary is not unproblematic background knowledge.

Of course, Popper may well view such cases with equanimity, for unlike the Positivists he has a high regard for the role of metaphysics in the development of science. He may view a theory of the kind described as being (perhaps) a bold and suggestive metaphysical theory on its way to becoming testable by way of further articulation and the piecemeal formulation of additional laws connecting its novel categories with observables. The successes and failures encountered in the 'quasi-tests' described above may help us formulate additional laws which, when added to the theory, produce an amplified theory that is testable in his sense. Popper has emphasized the example of Democritus' atomic theory, a theory that was untestable in its Democritean formulation, but which in the hands of the likes of Dalton and Maxwell received articulation in an amplified and (Popper would claim) testable form.

However, I do not think this reply is entirely satisfactory, for it does nothing to dispel the suspicion that the great bulk of rational research, both theoretical and experimental, consists nonetheless in the formulation and quasi-testing of metaphysical theories, rather than in the formulation and testing of testable theories. Nor does it dispel the suspicion that it is precisely evaluation of this less decisive kind that is the primary determinant of our acceptance or rejection of our fundamental theories. For it is clear that a theory which is only quasi-testable (in the sense described two paragraphs ago) may be a smashing success even so, or equally, a dismal failure.

Let me try to bring this discussion down to earth with some examples. The success that Newtonians enjoyed in accounting for the planetary motions was in one respect a piece of fantastic good fortune. One can appreciate the respect in which they were lucky by reflecting on a possible situation in which they would have been unlucky, despite the truth of their theory. (We shall here ignore Einstein.) Suppose that the motions of the solar planets were governed not just by the familiar gravitational forces, but also by some additional forces whose source, we may assume, resides in something like the (inaudible) Pythagorean 'tones'. These are sounding throughout the solar system and produce forces (varying with pitch) which make up a significant percentage of the net forces (gravitational plus
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Pythagorean) to which the planets are severally subject. The result is a pattern of planetary motions which is still roughly Copernican, but which differs substantially from the roughly Keplerian pattern so familiar to us.

Enter now Newtonian dynamics, freshly minted. The attempt to render intelligible these (more complex) motions will proceed as it did in our own case. The theory itself requires us to see the major masses, the sun and planets, as significant sources of force, but neither theory nor observation suggests that anything else is involved. Accordingly, we proceed on the assumption that gravitational forces, centered in the obvious masses, exhaust the significant forces in the system. Some such assumption is of course quite necessary, since we need some reckoning of net forces if we are to account for orbital paths with the three laws of motion. With this singular auxiliary in hand, plus others (e.g., that the mass of any planet is insignificant in comparison to that of the sun), we proceed to deduce elliptical orbits for the planets and Keplerian behaviour for their orbital velocities and relative periods. These results, unhappily, diverge substantially from what we observe in the system at issue. A first attempt at improving the situation may involve some more generous assumptions about the masses of the planets, but this will turn out not to help matters either. In fact, nothing in the way of a Newtonian interpretation of the initial conditions, short of a systematic recognition of the complicated distribution of Pythagorean forces, will produce satisfactory agreement with observation. The succession of progressively more imaginative attempts at applying the theory will fail to bear fruit, and Newtonian dynamics would quite rightly be put aside in favour of some other approach.

We must appreciate, however, that the failure described would not amount to a falsification in the sense of (B). Quite aside from their falsity, the interpretive auxiliary assumptions made in order to 'try out' the theory could hardly be counted as unproblematic background knowledge. In the absence of some successful theory of planetary dynamics, what forces (if any) move the heavens must remain a moot point. Newtonian dynamics will here have been subjected only to quasi-tests, but it will have turned in a dismal performance nonetheless.

Consider now our own history. It differs from the preceding fable only in that the same attempts at interpreting the initial conditions led to observational success. As luck would have it, the auxiliary assumptions were correct in the context of this solar system, and our confidence in them grew in direct proportion to the successes achieved by Newtonian dynamics with their help. But these successful 'tests' were just as much quasi-tests as are their unsuccessful analogues in the fable discussed above. The same speculative auxiliary assumptions
necessary to 'try out' the theory were neither better nor differently 'founded' than they are in the fable. The success enjoyed by Newton's theory in accounting for the planetary motions did not consist in its passing tests in the sense of (B). But it turned in a spectacular performance nonetheless.

The pattern displayed here can be found in many other cases as well, in Dalton's atomic theory, for example. The law that elements combine chemically in constant proportions with respect to mass was one of the most striking features of this theory. To put this law to the test, however, required some considerable interpretation concerning what was and what wasn't a pure elemental sample, and what was and what wasn't a genuine chemical compound as opposed to a mere mixture or solution, a distinction which itself made little clear sense save against the background of the theory. The law of constant proportions, by helping us to sort these matters out, brought an unprecedented order into the realm of chemical phenomena, but not because it was being systematically subjected to, and passing, tests in the sense of (B).

The point I am trying to make here is not that Newtonian dynamics (or Dalton's theory) is unfalsifiable. I am here content to leave that point moot. But there is a weaker conclusion here that plainly can be drawn from the preceding considerations. It is not necessary that a theory be subjected to Popperian tests, and fail them, in order that the theory turn in a dismal explanatory performance despite sincere and reasonable attempts to understand the relevant empirical phenomena in its terms. Nor is it necessary that a theory be subjected to Popperian tests, and pass them, in order that the theory prove highly successful as a means of systematic empirical understanding. Given that we have seen this to be true not just of some arcane example, but of some of our most highly respected theories, we must wonder how Popperian falsifiability can be of any fundamental significance for scientific methodology. Why need a theory be falsifiable? It is clear that non-Popperian evaluation is possible, and also that it has played a prominent role in our own theoretical progress. And I think it transparent that such evaluation is rational, for (to use a phrase of Popper's) in the case of many theories and many applications there is no more rational course open to us. It is time also to point out that there is certainly no dichotomy between Popperian tests and what I have been calling quasi-tests. With respect to the status of auxiliary assumptions, there will be a smooth continuum spanning the distance between 'pure mad guess' and 'unproblematic background knowledge'. To what end, then, should we labour to find any fundamental methodological distinctions of the kind at issue?

There is one, of course, at least from Popper's point of view. Perhaps the most central element in his epistemology is the view that,
while our experience can never verify (prove, establish, or even render probable) our theories, it can falsify them; the growth of knowledge proceeds by conjectures and refutations. A methodological premium is therefore placed on theories which are falsifiable.

But we have just seen that the growth of knowledge can proceed by conjectures and slow strangulations as well, and we found the difference between falsifications and strangulations to be a difference in naught but degree. The general formula would be, “all of the potentially acceptable interpretations of the initial conditions that we have so far been able to think of have led, when conjoined with the theory, to results inconsistent with observation.” The case of a Popperian falsification occurs when for some reason there is only one interpretation we are willing to entertain. Why these latter cases should be singled out as methodologically crucial is far from obvious, and it becomes even less obvious when one rehearses the fallibility of what we may take to be ‘unproblematic background knowledge’, and the extent to which it is called into question during theoretical crises.

In sum, the criticism levelled here is that Popper’s notion of falsifiability, and the demarcation it entails, is arbitrary and unimportant as far as rational scientific methodology is concerned. Some theories are (rightly) rejected by us not because we are able to find or produce unproblematic applications where the theory does clash with observation, but because we are not able systematically to produce plausible applications where the theory doesn’t clash with observation. Correlatively, some theories are (rightly) embraced by us not because they pass tests in the sense of (B), but because our speculative applications of them are more or less systematically rewarded.

The preceding criticisms are in the same spirit as those tendered by Putnam and Lakatos (and others). But I have tried to frame my objection in a fashion that escapes Popper’s central reply to these authors. Their objection, it will be recalled, is that Newtonian dynamics (for example) is simply unfalsifiable. And Popper’s reply7 is basically that, unless one is willing to violate methodological principle (iii) and pursue ‘immunizing strategems’ indefinitely, Newtonian dynamics is indeed falsifiable.

Whether or no this constitutes an adequate reply I shall leave for the reader to judge. What I wish to claim here is that, in light of the criticisms offered above, the question of whether Newtonian dynamics is falsifiable is somewhat academic. The burden of those criticisms is that Popper’s distinction between the falsifiable and the unfalsifiable is arbitrary and unimportant, and I do not see how an

7 See especially pp. 1004-5 in the Schilpp volume.
appeal to principle (iii) will go any way at all towards meeting this objection.

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We have so far been discussing what amounts to a theory of scientific rationality, a theory of what intellectual virtue consists in with respect to theoretical progress. But there is a larger issue here, larger than any so far discussed, and it is nicely expressed in the title of Kuhn's contribution, “Logic of Discovery or Psychology of Research?” (the Schilpp volume, pp. 798-819). The issue here concerns the proper aims of epistemology itself. What sort of theory-of-rationality should we be seeking, and how go about it? Should we be seeking an essentially normative account, a ‘logic’, an account whose defense will be basically a priori? Or should we be after a descriptive account, one whose defense will be overtly a posteriori? Kuhn holds that an adequate understanding of the nature of theoretical progress and scientific methodology is not to be had short of a penetrating empirical/historical/sociological study of the complex features characteristic of the scientific subculture which is the agent of that progress. Popper regards such matters as irrelevant to the true concerns of a theory of knowledge. As he sees it, knowledge (and the manner in which it grows) can and should be understood in a fashion which does not require us to see any person or group of persons as its seat or subject. In “Epistemology Without a Knowing Subject”8, he enjoins us to see it as having an ‘objective’ existence, and invites us to recall Plato’s world of ideas and Hegel’s objective spirit as being similar to what he has in mind. The intelligibilia which populate Popper’s ideal world (he calls it world 3) are, however, propositions, theories, and arguments. We labour on the collective contents of world 3 in somewhat the same fashion that masons labour on a cathedral. The object of labour in both cases exists independently of any labourer, and may be the life’s work of a great many generations past and yet to be. And the effects are reciprocal. The development of world 3 can affect our beliefs and mental states generally (these comprise world 2), and can thereby have profound effects on the state and arrangement of things in the physical world (world 1). To return to our concerns, Popper holds that a theory of scientific knowledge and its growth must be a theory about world 3, and that “. . . world 3 science can be investigated only logically” (the Schilpp volume, p. 1148). His position here gives ontological expression to a long-standing rejection of what in L.Sc.D. he calls ‘Psychologism’.

8 Objective Knowledge, pp. 106-152.
Now I think Popper is mistaken in all this, but the ontological excesses that many will see here do not strike me as the most important issue. That Popper has chosen to objectify or reify what he takes to be the crucial elements for epistemology is less interesting than what he takes to be the crucial elements themselves. On this latter score, Popper could not be more orthodox if he tried. Popper shares an assumption that is so universal it deserves to be described as an unquestioned dogma. It is the assumption that, for the purposes of a theory of knowledge and its growth, one's knowledge (or our knowledge, or knowledge, period) at any given time is relevantly and adequately represented by a set or system of sentences or propositions. Once one conceives things in this way—and it is the 'obvious' way—the relevant questions and the admissible kinds of answers are already settled, at least in outline. One asks, "When is a set of propositions a rational or justifiable set?", and/or "When is a transition from one set of sentences to another a rational transition?" And the proposed answers concern the relations that sentences and sets of sentences can bear to one another. The natural conviction is that the parameters relevant to a characterization of rationality are to be found among the host of logical and quasi-logical properties and relations holding of and among sentences, and sets of sentences. Theories of knowledge spawned by this conception of things I shall call sentential epistemologies. And what I wish to suggest, by way of drawing this essay to a close, is that the conception of things just outlined is profoundly confused.

The rationale of such a conception is not difficult to grasp. Conceived as 'knowers', we are in some fashion 'representing' the world, and the growth of our knowledge is just the expansion and improvement of said 'representation'. But the primary instrument of our representational talents is our language; it is the set of descriptive sentences we accept that constitutes our representation of reality. What else pursue, then, but sentential epistemologies?

The hole in this rationale is the claim that language is the primary canvass of representation. I suggest that it is quite obviously the physical brain that constitutes the primary canvass or instrument of world-representation, and that language is merely a device which permits distinct brains to engage in a collective rather than an individual process of improving their representations of reality. In just what fashion a brain comes to 'represent', to 'embody a model of', to 'contain systematic information concerning' the world is still a matter for speculation, but that it does so is patent. And that it can do so without language is equally patent. Among the many kinds of 'knowers' on this planet, only the human species uses language, and then only subsequent to a period of spectacular learning spanning the entire first year of any individual's life. Just what takes place during that pre-linguistic
period is also a mystery, and the idea that sentential epistemologies can render it intelligible is a curious hope indeed.

I have no wish to downplay the general role of language here. I happily agree that the institution of language has been decisive for the unparalleled intellectual progress of our own race, and I have neither grounds nor wish to deny that linguistic elements can form representations of reality. What I do deny is that they form the fundamental mode of such representation; and what I suggest is that an adequate theory of what knowledge is and how it grows will have to be concerned with whatever is our fundamental mode of representation and with the parameters of its elements. One could of course nurture a hope that the structure, elements, and operations of human language systematically reflect or mirror all of the theoretically relevant structure, elements, and operations of the brain, but there is no empirical evidence to sustain such a hope, and one would expect on the contrary that linguistic structures/operations reflect brain structures/operations only very grossly, selectively, and superficially.

What I ask you to consider then is the claim that, by reason of taking as fundamental a set of parameters which are at best superficial, sentential epistemologies will never achieve more than scattered and partial successes. If this so, then quite aside from the question of whether to objectify or no, Popper is objectifying the wrong items.

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