THE TENSION BETWEEN FALSIFICATIONISM AND REALISM: A CRITICAL
EXAMINATION OF A PROBLEM IN THE PHILOSOPHY OF KARL POPPER

by

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Thesis submitted to the Faculty of the

Virginia Polytechnic Institute and State University

in partial fulfillment of the requirements for the degree of

MASTER OF ARTS

in

Philosophy

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June, 1999

Blacksburg, Virginia

Key Words: Falsificationism, Scientific Realism, Popper, Putnam, Boyd, Hacking

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EXAMINATION OF A PROBLEM IN THE PHILOSOPHY OF KARL POPPER

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(ABSTRACT)

Karl Popper's philosophy of science includes both falsificationism and realism. I explore
the relationship between these two positions in his philosophy and find a strong tension
between them. Drawing upon this tension in Popper's philosophy, I explore the general
relationship between falsificationism and realism in an effort to determine whether or not
the two positions can be successfully combined. Rather than criticizing falsificationism, I
focus instead on the realist side of the tension and seek to resolve the tension through the
introduction of an alternative form of scientific realism. I examine three alternatives in
detail: Hilary Putnam's internal realism, Richard Boyd's realism, and Ian Hacking's
entity realism. Internal realism is shown to be an unsatisfactory solution because of its
failure to incorporate a notion of approximate truth. Boyd's version of realism is also
shown to be unsatisfactory due to its ascription of absolute approximate truth to scientific
theories. Hacking's entity realism, while consistent with falsificationism in many
respects, is also shown to be problematic due to its apparent reliance upon induction.
Finally, I propose a solution to the problem, which consists in the elaboration of an
alternative version of scientific realism based primarily on a reinterpretation of Hacking's
entity realism that stresses non-inferential knowledge of causes. I also argue that the
reinterpreted form of Hacking's realism can be used to support Boyd's notion of a
theoretical tradition, although one of entities and their causal properties rather than one of
approximately true theories.

which turned out to be decisive. Most importantly, she was willing to make a temporary financial and professional sacrifice in order to help me pursue a career change. I am extremely fortunate to have her in my life.

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Chapter 1: The Tension in Popper's Philosophy

I. Introduction

Karl Popper is well known for having introduced the notion of falsification as both the key to the logical methodology of science and as a criterion of demarcation between science and other areas of human thought. Indeed, Popper developed an entire philosophy of science based on a falsificationist approach. But he also considered himself a realist. I do not believe that the relationship between these two positions—i.e., falsificationism and realism—in Popper's philosophy has been adequately addressed by previous research. As I will show, this relationship is highly problematic. My aim in this thesis is to explore the relationship between falsificationism and scientific realism in Popper's philosophy as a way of shedding light on the general tension between these two positions.

While I think that both falsificationism and scientific realism have some appeal, I suspect that, at least in their Popperian formulations, they are not truly compatible with one another. By "compatibility" here, I do not mean mere logical consistency. Instead, I mean a kind of full-blooded agreement in which two philosophical positions or theories can be combined to form a persuasive whole. Let me now sketch the basic difficulty as I see it. At this point, my aim is simply to present a general idea of the problem; consequently, I will have to oversimplify matters a bit. These issues will be discussed more fully later in my analysis.

According to Popper's "solution" of Hume's problem of induction, the so-called inductive arguments of the natural sciences can never be justified on logical or epistemological grounds. Moreover, Popper claims that induction does not even exist as a psychological reality. Popper replaces the idea of induction in science with that of theoretical conjecture and refutation based on a purely deductive logic. When applied to science, such a logic leads to a fundamental asymmetry between verification and falsification. That is, since Popper treats all scientific theories as universal statements, no amount of empirical evidence restricted to claims about particulars can ever verify or

justify a theory. But a single piece of appropriate contradictory evidence can refute a theory. Thus, according to Popper, all attempts at justification or confirmation of scientific theories are misguided. The best that we can ever do is to falsify them. From a given theory and relevant background knowledge, we may deduce particular empirically testable consequences. If the statements that describe the experimental results contradict the empirically testable statements predicted by the given theory and background knowledge, then, via modus tollens, the system consisting of theory and background knowledge is refuted, and we may conclude that it is false.

But what happens if attempts at falsification fail? What are we then entitled to conclude about a theory? From the standpoint of falsificationism per se, absolutely nothing. We can simply tentatively accept the theory, but we cannot regard it as true. And herein, I think, lies a problem for anyone who, like Popper, also wishes to maintain a realist position with regard to scientific theories. For if unfalsified theories cannot be legitimately regarded as true, then how can they be understood to represent any sort of underlying reality? To his credit, Popper does introduce the notions of corroboration and verisimilitude as well as Tarski's correspondence theory of truth in an attempt to make the relationship between falsificationism and realism more plausible. But these elements of his philosophy are not part of falsificationism proper. And, as I will show later, they do not represent an adequate solution of the problem, even on Popper's own terms. Thus, the problem that I intend to address is whether, if one accepts Popper's falsificationism, one can also consistently and convincingly maintain a realist position.

With this sketch of the problem in hand, let me now describe how the thesis is organized. In the remainder of this first chapter, I offer a detailed explication of the problem. My account of Popper's falsificationism is not meant to address the many significant questions raised by critics of Popper's falsificationism. Rather, I deal with those questions, for example, the role played by conventions in falsificationism, only insofar as they bear directly on the relationship between falsificationism and realism. Next, I examine Popper's notion of corroboration and his use of the concept of verisimilitude. I compare my treatment of Popper's use of verisimilitude with Anthony

O'Hear's. This leads me to consider O'Hear's argument that Popper's philosophy is really closer to instrumentalism than to realism. That discussion leads to an analysis of Popper's utilization of Alfred Tarski's correspondence theory of truth, which, in turn, leads to a discussion of Popper's metaphysical realism.

It becomes clear from my analysis that, although not strictly incompatible, Popper's falsificationism and his metaphysical realism do not form a convincing conjunction. I show that this problem arises out of the relative emptiness of falsificationism proper. Furthermore, the specific places where problems arise in Popper's arguments become evident. Since it is not my primary aim in this thesis, however, to critique Popper's falsificationism, I concentrate on the realist side of the problem. Thus, I use the specific problems identified by this analysis, chiefly in Popper's brand of realism, to suggest possible avenues of approach to the development of a consistent, convincing blend of falsificationism and realism.

Chapter two, therefore, is a discussion of alternative forms of realism that have some potential of being convincingly combined with Popperian falsificationism. I begin by briefly discussing Jarrett Leplin's classification of the strategies employed by realists in defending their position against their anti-realist opponents. Leplin maintains that there are two such strategies: 1) to focus on reference and approximate truth, and 2) to show why scientific method and scientific reasoning are unintelligible without realism (Leplin 1984, p. 4).

The question of where Popper fits into Leplin's classification scheme serves as the launching pad for the main portion of chapter two, in which I carefully examine three alternative forms of realism that appear to have the greatest promise of being persuasively paired with Popperian falsificationism. Since I believe that Popper's approach to realism actually possesses elements of both of Leplin's strategies, I will need to look at alternative versions of realism representing each of those strategies. Consequently, I will consider in detail one particular brand of realism that focuses on reference and truth, namely Hilary Putnam's internal realism. And I also examine Richard Boyd's realism, which adopts Leplin's other strategy—i.e., the demonstration of

why scientific method is unintelligible without realism. Finally, I will consider Ian Hacking's entity realism, which, like Popper's own approach to realism, contains elements of both of Leplin's strategies. I will determine to what extent any of these alternatives are genuinely compatible with Popper's falsificationism.

Chapter three consists in assessing the relative merits of these three alternative brands of realism for resolving the tension between falsificationism and realism. This assessment leads to the establishment of certain criteria necessary for any adequate solution to this problem. Based on these criteria, I propose my own alternative, hybrid form of realism as a solution. Finally, I conclude by highlighting some important questions and problems left unresolved by my proposed solution.

II. Falsificationism

Popper's doctrine of falsificationism has its origin both in his search for a criterion of demarcation between science and pseudo-science and in his treatment of the problem of induction. In *Conjectures and Refutations,* Popper recounts that it was his desire to find a satisfactory means of distinguishing scientific from pseudo-scientific theories that first led him, in the winter of 1919–20, to the concept of falsifiability. According to this criterion of demarcation, what makes a theory scientific is that it may be tested, falsified, refuted (Popper 1965, pp. 33–37).

Yet, this idea of falsification did not take on its full significance within Popper's philosophy of science for a number of years—in fact, not until he seriously grappled with the problem of induction. While accepting Hume's claim that induction is not logically justifiable, Popper rejects Hume's psychological account of induction. Hume believed that our propensity to inductively infer universal scientific laws from limited experience was based on a fundamental psychological fact about us: we perceive repetitions based upon similarities or resemblances. Popper rejects this account on the purely logical ground that, since repetitions generally involve similarity rather than perfect sameness, they cannot simply be repetitions pure and simple. Instead, the repetitions and the similarities upon which they are based are dependent on a particular point of view. That

is, the detection of similarities depends upon specific expectations, presuppositions, *conjectures*. Thus, according to Popper, Hume's account of how knowledge is actually acquired is incorrect, and there simply is no such thing as induction. Based on his realization that the perception of similarities requires interpretation, Popper replaces induction with the idea of conjecture and refutation (Popper 1965, pp. 42–46).

Popper's realization of the importance of conjecture leads him to adopt a particularly theoretical view of science. According to this view, the scientific enterprise consists in putting forth testable (falsifiable) theories. Furthermore, Popper claims that "The theories of natural science, and especially what we call natural laws, have the logical form of strictly universal statements; thus, they can be expressed in the form of negations of strictly existential statements or, as we may say, in the form of *non-existence statements* (or 'there-is-not' statements)" (Popper 1968, pp. 68–69). Thus, the falsifiability of scientific theories derives from the fact that, as universal statements, they prohibit or exclude certain existential statements.

Coupled with his emphasis on scientific theorizing is Popper's view of science as deductive, not a surprising position given his rejection of induction. To be more precise, he regards scientific methodology as a combination of deduction and experimental testing. We start with conjecture and deduction. That is, from a theory (universal statement) and given initial conditions (singular statements), statements describing certain empirical consequences can be deduced. We can then perform certain experiments to determine whether or not the empirical conditions described by the deduced statements actually obtain. If the statements that actually describe the experimental results contradict the empirically testable statements predicted by the theory and initial conditions, then, via modus tollens, the system consisting of the theory and initial conditions is falsified. This deductive inference may be represented formally if we let t represent a system of statements consisting of a theory and initial conditions and let p represent some empirically testable statement deducible from t. Then the falsifying inference is: $t \rightarrow p$, $\sim p$ / $\therefore \sim t$ (Popper 1968, p. 76).

Now, of course, this bare logical account of falsification has to be fleshed out a bit. What are the requirements for a scientific theory to be falsifiable? In order to answer this question, it is first necessary to introduce the concept of a basic statement. Basic statements are a subset of all singular statements—the subset that may serve as falsifiers. Popper describes them by saying that "The system of basic statements … is to include, rather, *all self-consistent singular statements* of a certain logical form—all conceivable singular statements of fact, as it were" (Popper 1968, p. 84). In order for a theory to be considered falsifiable or empirical it must divide "the class of all possible basic statements unambiguously into … two non-empty subclasses" (Popper 1968, p. 86). These two classes are: 1) the class containing all the basic statements that are inconsistent with the theory and 2) the class containing all the basic statements allowed or permitted by the theory. The former class is the important one for falsificationism and is dubbed the class of "potential falsifiers" by Popper. Thus, the class of potential falsifiers must not be empty if a theory is to be falsifiable (Popper 1968, p. 86). In addition to being falsifiable, a scientific or empirical theory must be consistent, since from any inconsistent system all possible statements may be derived (Popper 1968, p. 92).

For present purposes, Popper makes an important comment in discussing falsifiability. He says, "It may be added that a theory makes assertions only about its potential falsifiers. (It asserts their falsity.) About the 'permitted' basic statements it says nothing. In particular, *it does not say that they are true*" (Popper 1968, p. 86; italics added). This statement is significant for my purposes because it bears on the relationship between falsificationism and realism. More specifically, it suggests that Popper's falsificationism may not be compatible with realism.

If Popper is right, then one would have to wonder how a theory could ever get to the truth. To put the point a different way, how could we ever regard a theory as giving us a realistic representation of nature when the theory did not assert the truth of its permitted basic statements? We might imagine a theory that had been carefully articulated and rearticulated (honed, refined) to include an ever greater number of basic statements in its class of potential falsifiers, and, thus, to exclude an ever greater range of

empirical phenomena. We might also imagine that all the phenomena described by these potential falsifiers had been found via severe experimental testing not to obtain across a wide range of conditions. But, given the apparently infinite nature of the empirical world, what reason is there to suppose that a theory would ever converge on the truth in this manner?

We might, however, carry this imagined scenario a bit further. Although it is impossible for a *universal* theory to entail only one basic statement, we could hypothetically imagine a scientific theory that divided the set of all basic statements in such a way that the set of all potential falsifiers included all the basic statements except for one, which was the sole permitted basic statement. Even in this case, on Popper's account, we would not be justified in maintaining that the sole permitted basic statement was true. The statement's truth would depend not only on the theory but also on the proper initial conditions. If different initial conditions obtained, then a different permitted basic statement would follow from the theory. The truth of the theory, therefore, is only a necessary, but not sufficient, condition for the truth of the sole permitted basic statement. And even if the basic statement were true, that fact would not make the theory true. Under different conditions, the theory might be falsified.

Just as this example sheds some light on the tension between Popper's falsificationism and realism, so too is such tension evident in his conventionalism. In order to understand his particular brand of conventionalism, it is necessary to examine more closely the role played by basic statements in Popper's philosophy. First, Popper believes that, in order to be a basic statement, a statement must satisfy two formal criteria: 1) it must not be deducible from a universal statement minus initial conditions, and 2) it must be capable of contradicting a universal statement. As a result of these criteria, Popper specifies the rule that "*basic statements have the form of singular existential statements*" (i.e., statements "of the form 'There is a so-and-so in the region *k*' or 'such-and-such an event is occurring in the region *k*'" (Popper 1968, pp. 101–2).

Basic statements, however, must also satisfy Popper's material criterion: they must be inter-subjectively testable via 'observation'. Careful to avoid any hint of

psychologism, Popper refuses to define the term 'observable'; instead, he treats it "as a primitive concept whose use the epistemologist has to learn" (Popper 1968, pp. 102–3). Now what is of interest here is that Popper does not attempt to provide any solid foundation for observability. Related to his noncommittal approach toward observability is his view that any given basic statement can itself be inter-subjectively tested (falsified), since, from the basic statement and a suitable theory, further basic statements can be deduced. Consequently, there is no *logical* end to testing (Popper 1968, p. 104). In testing a theory, we simply *decide* to accept certain basic statements. Basic statements are, therefore, conventional, since their acceptance is based on decision or agreement 'on the spot' (Popper 1968, p. 106).

Unlike the standard conventionalist, however, Popper asserts that "the statements decided by agreement are *not universal but singular*" (Popper 1968, p. 109). Although he admits that his form of conventionalism may lead to an infinite regress problem associated with basic statements, he regards the problem as innocuous, since his falsificationist approach does not attempt to prove or justify any statements whatsoever (Popper 1968, p. 105). He also admits that "The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp" (Popper 1968, p. 111). The problem then for Popper is that, while he wishes to be a realist about scientific theories (universal statements), neither these statements *nor* the basic statements by which they are tested can, according to falsificationism, be proved or justified. Thus, there appears to be nothing sufficiently solid in Popper's falsificationist approach upon which to justify scientific realism. While Popper reproaches standard conventionalists for treating scientific theories as mere constructions (Popper 1968, p. 79), his own falsificationism appears to *destroy* any basis for a realist view of scientific theories.

III. Corroboration and Verisimilitude

Popper's philosophy of science, though, does not end with falsificationism, or at least with falsificationism proper. He fleshes out his basic falsificationist account by

introducing the important concepts of corroboration and verisimilitude. I believe that Popper introduces these concepts in an attempt to supplement falsificationism proper in a way that will make it more amenable to scientific realism. I will argue here, however, that this attempt does not succeed.

What role does corroboration play in Popper's philosophy of science? First of all, I should emphasize that I do not consider corroboration simply to be a part of Popper's falsificationism proper. Admittedly, corroboration does play a part in falsificationism proper, namely with regard to low-level empirical hypotheses (i.e., falsifying hypotheses). Popper asserts that we do not accept the falsification of a theory unless such a low-level empirical hypothesis describing "a *reproducible effect* which refutes the theory" is corroborated (Popper 1968, pp. 86–87). The corroboration here described, however, is not of a highly universal theory but instead of a low-level empirical claim, which Popper says, "need not in fact be a strictly universal statement" (Popper 1968, p. 87, n. 1). Thus, this sort of corroboration is really something like an instrumental component of falsificationism proper.

The kind of corroboration that I want to discuss now is that which pertains to highly universal theories themselves. Popper introduces this kind of corroboration in order to provide a positive account of theory appraisal to go along with his negative, falsificationist account. Thus, this kind of corroboration stands, as it were, on the same (or a similar) hierarchical level as falsification within Popper's philosophy. As Popper admits, the falsificationist view that theories are 'provisional conjectures' is itself an appraisal (Popper 1968, p. 265).

What then is corroboration? Perhaps it would be wise to begin by describing what Popper does *not* mean by 'corroboration'. He does not mean 'verification', for he rejects any form of justificationism. And, consistent with his critique of probability logic, he does not mean 'probable' (Popper 1968, p. 251 and n. *1). Instead, corroboration is a measure of how well a given theory has withstood severe tests. But since the severity of tests is dependent "upon the *degree of testability,* and thus upon the simplicity of the hypothesis," corroboration is connected with falsifiability. Thus, the more highly

falsifiable a theory is, the greater is its degree of corroborability (but not, necessarily, of its actual corroboration) (Popper 1968, pp. 267–68). The two aspects of a theory's appraisal are, therefore, nicely tied together by Popper.

It is important to note at this point that nowhere in my discussion of corroboration so far does the word "true" appear. This is because, in order to relate the concept of corroboration to that of truth, Popper first has to connect corroboration with the concept of verisimilitude. Before discussing this connection, however, I would like to remark on a passage in which Popper compares the notions of corroboration and truth. Popper regards 'truth' as a logical, and thus timeless, concept. While he admits that an appraisal of corroboration is also a logical, and thus timeless, one, he thinks that there is an important difference between corroboration and truth. Although we can speak of truth 'pure and simple', we cannot so speak of corroboration, for all corroboration is relative to a system of basic statements "accepted up to a particular point in time" (Popper 1968, p. 275). Popper's emphasis here seems to be simply on the time-dependency of corroboration. But I think that one can place the emphasis instead on the relativity of the system of basic statements required for corroboration. That is, Popper appears to implicitly claim that truth, unlike corroboration, is *not* relative to a system of basic statements. This is an important difference that I will examine further when I discuss Popper's interpretation of realism as a metaphysical position.

Given this difference between corroboration and truth, it is not surprising that Popper introduces the concept of verisimilitude. The introduction of this concept allows him to associate the concept of corroboration with that of truth and, thus, potentially, to

provide a realist interpretation of falsificationism.[1] The relationship between corroboration and verisimilitude is presented in the following table from *Conjectures and Refutations* (Popper 1965, p. 228):

truth	conjecture
testability	empirical test
explanatory or predictive power	degree of corroboration
'verisimilitude'	(that is, report of the results of tests).

As can be seen from the table, the epistemological concept of degree of corroboration relates to the metaphysical notions of explanatory power and verisimilitude. The concept of verisimilitude combines the notions of truth and content so that it can be formally defined simply as the truth-content minus the falsity-content. Thus, for any theory *a*, $Vs(a) = Ct_T(a) - Ct_F(a)$, where Vs = verisimilitude, Ct_T = truth-content, and Ct_F = falsity-content (Popper 1965, pp. 232–34). The truth-content of a statement is the class of all those true statements (consequences), excluding tautologies, that follow from it. The falsity-content of a statement, by contrast, is the class of all those false statements (consequences) that follow from it (Popper 1965, p. 233; Popper 1972, pp. 47–48). On a more intuitive level, Popper describes verisimilitude as "the idea of a degree of better (or worse) correspondence to truth or of greater (or less) likeness or similarity to truth" (Popper 1965, p. 233) and as "the notion of a better approach or approximation to the truth, or of a nearness to truth" (Popper 1972, p. 47).

Rarely stated explicitly, the actual nature of the relationship between corroboration and verisimilitude is far from straightforward. One might conceive of the relationship as follows: a highly corroborated theory is one which has withstood many

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[1] It is interesting that Popper did not introduce the idea of verisimilitude in his *Logik der Forschung* of 1933. Apparently, he did not feel the need for such an idea until much later. Verisimilitude was first introduced in a major work in *Conjectures and Refutations: The Growth of Scientific Knowledge* (1963). The concept was then developed further in *Objective Knowledge: An Evolutionary Approach* (1972).

severe tests, and, thus, has shown that it has a greater truth-content than falsity-content (i.e., that it has a high degree of verisimilitude). But this would clearly be wrong, for Popper denies that corroboration can be regarded simply as a measure of verisimilitude (Popper 1974, p. 1011). He does, however, believe that "the degree of corroboration *may* be taken as an indication of verisimilitude" (Popper 1974, p. 1011; italics added). Such an assessment must be regarded as a guess or conjecture, but one which itself may be critically examined (Popper 1965, p. 234; Popper 1974, p. 1011).

These remarks concerning the nature of the relationship between corroboration and verisimilitude lead rather nicely into the first of two ideas that I would like to address because of their bearing on the tension between falsificationism and realism. This idea is that the concept of verisimilitude is, in a methodological sense, superior to the concept of truth. Popper says, for example, that "To say that the aim of science is verisimilitude has a considerable advantage over the perhaps simpler formulation that the aim of science is truth" (Popper 1972, p. 57). He also asserts that "the search for verisimilitude is a clearer and a more realistic aim than the search for truth" (Popper 1972, p. 57). Such statements are, of course, in perfect agreement with Popper's falsificationist view that it is better to put forth a bold conjecture that can be severely tested—*"even (and especially) if it soon turns out to be false"*— than to simply list uninteresting truths (Popper 1965, pp. 230–31). Popper lends further support to this sort of agreement when he says, "… the idea of verisimilitude is most important in cases where we know that we have to work with theories which are *at best* approximations—that is to say, theories of which we actually know that they cannot be true" (Popper 1965, p. 235). Although Popper implies that such cases are limited to the social sciences (Popper 1965, p. 235), according to strict falsificationism, the idea of verisimilitude would have to apply to *all* cases, even those involving theories from the mature natural sciences. But this would mean that truth (i.e., absolute truth) would no longer play *any* methodological role in scientific discovery. Indeed, even the concept of verisimilitude plays only a comparative and relative role. As Popper explains, we are not concerned with "the maximum degree of verisimilitude" for the "comparative use of the idea is the point" (Popper 1965, p. 234). That is, what

Popper wants is to be able to say that one theory has a higher degree of verisimilitude than another, even if neither of them is very close to the truth. He is interested only in relative verisimilitude, not absolute verisimilitude. This use of verisimilitude, though, seems to leave us far from any sort of meaningful realism.

The second idea that bears on the tension between falsificationism and realism has to do with a difference between corroboration and verisimilitude. This difference is expressed by Popper when he asserts, "The degree of corroboration of a theory has always a temporal index: it is the degree to which the theory appears well tested at the time *t.* This cannot be a measure of its verisimilitude, but it can be taken as an indication of how its verisimilitude *appears* at the time *t,* compared with another theory" (Popper 1972, p. 103). What Popper is saying here is simply that, while corroboration is temporal, verisimilitude is timeless. Corroboration, as an epistemic notion, can only ever provide an *apparent* verisimilitude, since verisimilitude is an absolute quality. As Popper explains, "Our idea of approximation to truth, or of verisimilitude, has the same objective character and the same ideal or regulative character as the idea of objective or absolute *truth.* It is *not an epistemological* or *an epistemic idea*—no more than truth or content" (Popper 1965, p. 234). Thus, verisimilitude is, like truth, a metaphysical concept. This result, of course, should come as no surprise, for it is simply the same sort of relationship between corroboration and verisimilitude that we saw in the table from *Conjectures and Refutations* presented above. The problem here is that if verisimilitude is just another metaphysical concept like truth, then how is its introduction supposed to bridge the gap between Popper's falsificationist epistemology and his realism? If the apparent verisimilitude afforded by corroboration were taken seriously by Popper, then this would enable him to connect the epistemological concept of corroboration to the metaphysical notion of truth and, thus, to connect falsificationism to realism in a persuasive manner. But, of course, Popper dismisses apparent verisimilitude because of its justificationist overtones.

This critique of Popper's use of the concept of verisimilitude is, however, not the only one that could be made. In fact, Anthony O'Hear (1982) offers an analysis which

suggests the opposite conclusion. Instead of claiming that Popper's use of corroboration and verisimilitude saves falsificationism at the expense of realism, as I argue, O'Hear suggests that Popper's use of those concepts gets him realism at the expense of falsificationism. The first part of O'Hear's argument centers on Popper's discussion in *Objective Knowledge* of the improbability of accidental correspondence between a logically very improbable and relatively comprehensive scientific theory and reality. O'Hear points out Popper's assertion that an "accidentally very improbable agreement between a theory and a fact can be interpreted as an indicator that the theory has a (comparatively) high verisimilitude" (O'Hear 1982, p. 66; Popper 1972, p. 103). O'Hear thinks that by admitting that we have a reason to think that a theory possesses some verisimilitude, Popper comes close to inductivism (O'Hear 1982, p. 66). The second part of O'Hear's argument concerns an even more explicit admission of inductivism by Popper. Referring specifically to Einstein's theory, Popper again asserts the improbability that such a theory could make correct predictions of highly precise measurements unless the theory contained some truth (Popper 1974, p. 1192, n. 165b). And although he does not want to admit anything other than *relative* verisimilitude, he confesses that "there may be a 'whiff' of inductivism here. It enters with the vague realist assumption that reality, though unknown, is in some respects similar to what science tells us" (Popper 1974, pp. 1192–93, n. 165b). O'Hear (1982, p. 67) points out that if "we are entitled to conclude … that reality is at least to some extent as science teaches us and that the methods of science do get us nearer the truth, then are we not equally entitled rationally to rely on the findings of science?" If such inductivism were accepted, then it would seemingly bring Popper's philosophy in line with some form of common sense realism; but for Popper to accept inductivism would be to reject the whole basis for his falsificationist approach.

IV. O'Hear's Critique: Popper as an Instrumentalist

I turn now to another relevant criticism of Popper's philosophy of science presented by O'Hear. O'Hear's argument, in fact, confronts head-on the central question of my thesis.

He explains that he wants to show "that the emphasis on falsifiable predictions in Popper's account of scientific explanation brings his philosophy close to instrumentalism, despite his explicit commitment to realism" (O'Hear 1982, p. 90).

The first part of O'Hear's argument has to do with Popper's position regarding the role of essences in scientific explanation. According to Popper there are three possible positions that one can hold on this issue: 1) that there can be ultimate explanations which require essences, 2) the instrumentalist view that there are no such things as scientific explanations or essences, and 3) Popper's own position, which he calls 'modified essentialism' (Popper 1972, pp. 194–95). Popper's position is related to essentialism in that it too contains the idea "that much is hidden from us, and that much of what is hidden may be discovered" (Popper 1965, p. 105). But, in maintaining modified essentialism, Popper denies the possibility of ultimate explanations, which are based on essences and which, thus, require no further explanation (Popper 1965, p. 105; Popper 1972, p. 195).

As O'Hear correctly points out, this denial of ultimate essences by Popper is a concession to instrumentalism (O'Hear 1982, p. 92). Indeed, whether one accepts Popper's three-part classification of positions on essences or the standard instrumentalist/essentialist classification, Popper's position definitely represents a step away from essentialism and towards instrumentalism. And, according to the standard dichotomy between instrumentalism and realism, any step towards instrumentalism would be a step away from realism. A coherent doctrine of real ultimate essences underlying all phenomena would certainly seem to provide a better foundation for realism than Popper's own somewhat paradoxical view that, although we seek ever deeper and more universal explanations, there can be no natural end to our search. According to O'Hear, Popper views this "drive for universality and depth" as "itself anti-instrumentalist, because it leads us to probe into more fundamental properties of matter and thereby to link apparently un-connected phenomena" (O'Hear 1982, p. 92). The problem, however, is that "As universality and depth are analysed by Popper in terms of testability, … an instrumentalist could agree that they are desirable properties of a tool,

because the more applications a tool has, the more useful it is" (O'Hear 1982, p. 92). Thus, without an appeal to ultimate essences, Popper's emphasis on greater universality and depth seems to leave him closer to instrumentalism than to realism.

Yet, O'Hear's critique of Popper as an instrumentalist does not end there. He succinctly assesses the basic tension between the falsificationist and realist tendencies in Popper's thought as follows: "what the realist has to show is that theories are not merely instruments, but actually give us knowledge of the real world. Here, of course, Popper is in a fix, because he thinks that we cannot know this, beyond saying that some theories have survived some tests" (O'Hear 1982, pp. 92–93). Popper's strategy in dealing with this tension ultimately relies more on attacking instrumentalism than on defending realism. Popper's argument against instrumentalism is that scientists, in fact, do *not* treat theories simply as instruments but, instead, reject falsified theories. Scientists do not just regard the predictions of a given theory as determining the range of the theory's applicability but also regard such predictions as potential falsifiers. And a falsified theory is rejected even though, from an instrumentalist perspective, it may still be fruitfully employed within the limited range of applicability revealed by testing (O'Hear 1982, p. 93; Popper 1965, pp. 111–14). But, according to O'Hear, this argument is rather weak. Not only does it fail to acknowledge that some scientists *are* instrumentalists, it also does not give an accurate picture of actual scientific practice: older, refuted theories are not usually discarded but are, instead, generally incorporated into newer theories as approximations. Furthermore, as mentioned above, Popper's emphasis on severe testing of theories and on creating theories of greater universality and depth can simply be reinterpreted from an instrumentalist perspective as the pragmatic goal of attaining better and more widely applicable instruments (O'Hear 1982, p. 93).

The last major point that O'Hear makes in his account of Popper's instrumentalism has to do with Popper's position regarding the continuity of scientific explanations across theory changes. Popper cites the sun's failing to rise tomorrow as an example of an occurrence that would necessitate drastic theory revision. In such a case, according to Popper, "the revised theories would not merely have to account for the new

state of affairs: *our older experiences would also have to be derivable from them*"
(Popper 1968, p. 253). As O'Hear points out, it is significant that Popper does *not* also
require that our older explanations/explanatory structures be derivable from the new
revised theories as well. Realism requires that we regard what lies between initial
conditions and a theory's predicted effects—i.e., the theory's particular explanatory
structure—as reflecting reality in some way. Thus, a realistic conception of theory
change would seem to require that, in addition to old phenomena, older explanatory
structures also be "saved" somehow (O'Hear 1982, pp. 94–95). Yet, despite his claim to
be a realist, Popper does not insist on this requirement.[2] Instead, his falsificationism,
with its emphasis on testing, actually only stresses "the contact points between theories
and experience" in much the same way that instrumentalism does (O'Hear 1982, p. 95).
Thus, once again, we find a basic tension between the falsificationist and realist strands in
Popper's philosophy.

V. Popper's Use of Tarski's Theory of Truth

That Popper does indeed intend a realistic interpretation of his philosophy of science is
also made quite evident in his use of Alfred Tarski's theory of truth. It is, in fact,
Tarski's theory upon which Popper chiefly attempts to ground his realism; his use of
verisimilitude is, as I will demonstrate shortly, actually dependent upon his use of
Tarski's theory. Although in this section I will suggest some ways in which Popper
employs Tarski's theory to support realism, I will postpone much of my discussion of this
topic until I examine Popper's metaphysical realism in the next section. What I do here
is describe how Popper utilizes Tarski's theory and to suggest where difficulties arise.

Popper admits that, before he adopted Tarski's theory, he considered the concept
of truth problematic and was reluctant to talk of 'truth' and 'falsity' (Popper 1965, p. 223;
Popper 1968, p. 274). Yet, he accepted the more or less commonsensical notion of "the

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[2] O'Hear speculates that Popper's unwillingness to adopt such a requirement is due to its "justificationist
tone" (O'Hear 1982, p. 95).

objective or absolute or correspondence theory of truth—truth as correspondence with the facts" (Popper 1965, p. 223). But what could the correspondence between a statement and a fact possibly consist in? Popper discusses both Schlick's and Wittgenstein's failed attempts to explain such correspondence (Popper 1972, p. 312; cf. Popper 1965, p. 223). The problem then for Popper was to understand how it could meaningfully be said that a statement corresponds with a fact. According to Popper, Tarski's theory solved this problem simply by introducing the idea of a metalanguage capable of speaking both about facts and about statements, or, to be more specific, capable both of *referring* to statements in the object language and of *describing* facts. Thus, within the metalanguage one "can speak about correspondence between statements and facts without any difficulty" (Popper 1972, p. 314). Popper regards Tarski's theory—despite its apparent triviality—as "a great philosophical achievement" and as no less than "a rehabilitation of the correspondence theory" (Popper 1972, p. 314).

Obviously then, Tarski's theory is a very important element in Popper's philosophy,[3] and, thus, it is clearly worth examining more closely the way in which Popper thinks that the theory works. To begin with, a metalanguage clearly has to be a language in which we can talk about the object language. More formally, a metalanguage must contain the following kinds of expressions: 1) names of object language statements, 2) metalanguage descriptions of facts described in the object language (this may be achieved either by translating the object language into the metalanguage or by making the object language a part of the metalanguage), and 3) "terms denoting predicates of, and relations between," the first two "fundamental kinds of expressions" (Popper 1972, p. 325).

Popper gives a revealing example of this theory in which English serves as the metalanguage and German as the object language. Having satisfied the formal requirements of the theory, we are able to make the following metalanguage statement:

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[3] Popper says explictly that, except for the problem of demarcation between science and nonscience, "only two other ideas . . . have become quite as important for my philosophy: indeterminism . . . and Tarski's theory of truth" (Popper 1974, p. 976).

"'Das Gras ist grün' corresponds to the facts if, and only if, grass is green." Now the revelation comes when Popper interprets the significance of such a statement. He says:

> This makes it possible for our assertion to express a relationship between a (German) *statement,* and a *fact.* (The *fact* is neither German nor English, although it is, of course, described or spoken about in our metalanguage, which is English: the fact is non-linguistic, it is a fact of the real world, although we need of course a language if we wish to talk about it.) And what our metalinguistic assertion asserts is that a certain (German) statement *corresponds to a certain fact* (a non-linguistic fact, a fact of the real world) under conditions which are precisely stated. (Popper 1972, p. 315)

What is revealing here is what Popper puts in parentheses and the fact that he feels obliged to include such parenthetical material. Popper clearly thinks it necessary to insist that his use of Tarski's idea of a metalanguage in no way turns facts into mere linguistic entities. A fact, although described in the metalanguage, is supposed to be "non-linguistic" and "of the real world"–i.e., a fact simpliciter. Similarly, Popper claims that facts are "neither German nor English." Although facts have to be described in a given language, they are not supposed to be relative to that language.

Thus, Popper clearly treats the metalanguage as *conceptually neutral* or as *atheoretical.* Popper seems to think that it does not matter what particular metalanguage is chosen, provided it satisfies the formal requirements. Although he avoids the potential problem of incommensurability between the object language and the metalanguage by insisting that the object language can be made a part of the metalanguage, he seems to deny that any given metalanguage will necessarily express strong conceptual commitments—commitments that prevent the language from representing certain facts. This is truly paradoxical, for, in order to be consistent with his falsificationist or conjectural approach, Popper would have to admit that our languages themselves embody many conjectures about the underlying nature of the world. In fact, he admits as much when he argues against Hume's psychology of induction. Popper claims, contra Hume, that we do not form ideas based on perceived similarities in repeated observations, but,

instead, in order to perceive any similarities in the first place, we constantly have to make conjectures about the world around us (Popper 1965, pp. 44–46). Moreover, such conjectures are necessarily reflected in the language that we use to describe the world. Thus, Popper appears to use Tarski's theory in a way that runs counter to his own falsificationist philosophy. Moreover, as O'Hear points out, Popper may simply be misusing Tarski's theory, for Tarski's "definition [of truth] is always of truth relativized to a particular language and not of truth as such" (O'Hear 1982, p. 206).

Yet Popper clearly takes Tarski's theory to be a theory "of truth as such." He says, for example, that Tarski "re-established a correspondence theory of *absolute* or objective truth" (Popper 1965, p. 223; italics added). He also claims that "Tarski says that his sole intention was to define truth, in its *absolute* sense" (Popper 1983, p. 273). It is important to note, however, that Popper explicitly denies that Tarski's theory supplies any *criterion of truth.* In fact, Popper points out that Tarski has proved that "there can be no criterion of truth" (Popper 1972, p. 317). Thus, the concept of absolute or objective truth serves primarily as a regulative idea (Popper 1972, pp. 317–18, 46–47). This is important because it ties into Popper's use of the concept of verisimilitude. That is, if truth is a regulative idea or—as Popper also describes it—a "standard of which we may fall short," (Popper 1965, p. 229) then there is an obvious need for some measure of how close we come to reaching it. This is, of course, the role supposedly played by verisimilitude.[4] Popper explains that his "aim is to achieve … for verisimilitude something similar to what Tarski achieved for truth: the rehabilition of a commonsense notion which has become suspect, but which is in my opinion much needed for any critical commonsense realism and for any critical theory of science" (Popper 1972, p. 59). Popper's concept of verisimilitude, though, is obtained by combining Tarski's Calculus of Systems with his theory of truth (Popper 1972, pp. 329–35). Thus, Popper's use of

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[4] It is important to remember, however, that Popper does not regard verisimilitude as an epistemological concept. Thus, as previously discussed, verisimilitude cannot serve as a bridge between the epistemological concept of corroboration and the metaphysical notion of truth. In effect, Popper simply substitutes one metaphysical ideal (verisimilitude) for another (truth).

verisimilitude to provide a realistic interpretation of falsificationism is ultimately dependent on Tarski's theory of truth.

VI. Metaphysical Realism

In this section I shall examine Popper's more or less explicit professions of realism, beginning with his remarks concerning the relationship between Tarski's theory of truth and realism. According to Popper, "Tarski's theory allows us to *define truth* as correspondence to the facts; but we can use it also to *define reality* as that to which true statements correspond." This is important for Popper since it allows him to reassure himself "that the concept of reality is not 'vacuous', or suspect for any other reason; no more than is the concept of truth" (Popper 1972, p. 329). Popper also praises Tarski's theory for eliminating the suspicion of the correspondence theory long felt even by those who valued it "simply because of its commonsense and realist character" (Popper 1972, p. 324). Moreover, he explains that, although he had felt a distinction should be made between "the vague and highly metaphysical notion" of truth to which scientific theories approach (i.e., "'Truth' with a capital 'T'" or absolute truth) and Tarski's truth (i.e., with a small 't'), he came to realize that "there was no particular difficulty in applying Tarski's fundamental idea to it [the notion of absolute truth]" (Popper 1965, pp. 231–32). Popper adds that this is because "there really is no barrier here between what at first sight appeared to be Truth with a capital 'T' and truth in a Tarskian sense" (Popper 1965, p. 232). Thus, Popper's use of Tarski's theory clearly involves the idea of absolute truth.

Now, as I mentioned when discussing corroboration, Popper seems to implicitly claim that truth is not relative to a system of basic statements. Since it turns out that we are dealing with absolute truth, this claim is not surprising. On Popper's account, basic statements must be inter-subjectively testable via observation, *and* they also must themselves be falsifiable in the same way. But this means that testing has only a conventional and not a logical end; that is, we simply make the decision to accept certain basic statements. But surely absolute truth is not dependent upon mere convention.

But exactly what sort of realism does Popper think Tarski's theory supports? This question is answered unequivocally in the following statement from *Objective Knowledge:* "Tarski's theory, as you all know, and as he stressed first, is a *rehabilitation* and an elaboration of the classical theory that truth is correspondence to the facts; and this seems to *me* to support metaphysical realism" (Popper 1972, p. 323). Moreover, Popper explicitly says that he believes in metaphysical realism (Popper 1983, p. 80). He elaborates on the nature of this sort of realism when he states, "when I say that we cannot know, even of a well-corroborated scientific theory, whether or not it is true, I am actually assuming a 'concept of absolute scientific truth'; just as somebody who says 'I did not succeed in reaching the goal' operates with an 'absolute concept of a goal'—that is, one whose existence is assumed *independently of its being reached*" (Popper 1965, p. 157). And Popper explains that metaphysical realism "forms a kind of background that gives point to our search for truth. Rational discussion, that is, criticial [sic] argument in the interest of getting nearer to the truth, would be pointless without an objective reality, a world which we make it our task to discover: unknown, or largely unknown: a challenge to our intellectual ingenuity, courage, and integrity" (Popper 1983, p. 81). Thus, Popper holds a weak version of metaphysical realism according to which there is an absolute truth and there is an objective reality, a world that exists independently of our minds.

Popper also holds what may be described as a strong version of metaphysical realism according to which scientific theories are causal explanations. This is a very significant feature of Popper's philosophy for it allows him to distance himself from instrumentalism. O'Hear describes Popper's emphasis on causes as follows: "universal law statements [i.e., scientific theories] will typically involve reference to the underlying structural properties of matter, including unobservable entities and forces, which are supposed to be the causes of the phenomena we do observe" (O'Hear 1982, p. 90).[5] Although Popper rejects the 'principle of causality' (i.e., "the assertion that any event

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[5] I will have more to say about Popper's view of causes when I discuss Hacking's entity realism.

whatsoever *can* be causally explained") because it is metaphysical (not falsifiable), he proposes "a methodological rule which corresponds so closely to the 'principle of causality' that the latter might be regarded as its metaphysical version" (Popper 1968, p. 61). This rule says that "we are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain *causally* any kind of event we can describe" (Popper 1968, p. 61; italics added). Thus, while Popper's strong version of realism is *not* metaphysical in the sense of requiring a priori that all events have causes, it *is* metaphysical in the meaningful sense of maintaining, contra instrumentalists, that the search for underlying, hidden causes is a legitimate scientific end.

There is still another, more fundamental sense in which Popper's realism is metaphysical. Popper states that, although his form of realism could with good reason be called 'scientific', "its (apparent) lack of testability" leads him to call it 'metaphysical' instead (Popper 1972, p. 40). In fact, he regards both realism and idealism as irrefutable *and* indemonstrable. Thus, for Popper, realism is not an empirical or scientific theory; it is metaphysical (Popper 1972, p. 38; Popper 1983, p. 82). In short, Popper's form of realism is metaphysical in the sense that, according to his demarcation criterion, it is nonscientific.

Yet, Popper does think that one can argue for realism, primarily by arguing against idealism and by appealing to common sense (Popper 1972, pp. 38–39). And he thinks that realism is "the only sensible hypothesis … to which no sensible alternative has ever been offered" (Popper 1972, p. 42). He says that the arguments for realism's alternatives (i.e., "positivism, idealism, phenomenalism, phenomenology, and so on") fail primarily because they are based on a faulty commonsense, subjectivist epistemology—what Popper calls the 'bucket theory of the mind' (Popper 1972, p. 42).

It is important, however, to understand how Popper fleshes out his metaphysical realism. He explains that he believes "in the reality of the physical world" and "that the

world of theoretical entities is real" (Popper 1972, p. 323, n. 7). But he also asserts that "the reality of physical bodies is implied in almost all the common sense statements we ever make; and this, in turn, entails the existence of laws of nature: so all the pronouncements of science imply realism" (Popper 1983, p. 128). He expresses this view even more clearly when he says that "physical bodies are only an aspect of the law-like structure of the world which alone guarantees their (relative) permanence; which means … that the existence of physical bodies … entails that of objective physical regularities" (Popper 1983, p. 80).[6] Thus, although Popper professes to be a realist about physical bodies and theoretical entities, such realism is really secondary: Popper is primarily a realist about natural laws. That is, he is a nomic realist.

But if this is true, then there may be no real tension between his falsificationism and his realism. It would indeed be consistent to hold *both* that the logical methodology of science is characterized by attempts to falsify theories *and* that there are objective natural laws. But such a weak version of nomic realism just says that there *are* objective natural laws in the same way that weak metaphysical realism says that there *is* an objective reality. This means that falsificationism and weak metaphysical realism also possess this same sort of bare logical consistency. Thus, in this sense no real tension exists between Popper's basic falsificationist approach (what I have been calling falsificationism proper) and the weak form of his metaphysical realism.

But there are three problems here for Popper. First, Popper also maintains a stronger version of metaphysical realism, which incorporates the idea that we can have knowledge (albeit conjectural) of unobservable causes. The difficulty here is that falsificationism prohibits us from having any confidence that the conjectured causes are, in fact, real. Second, Popper professes to be a realist about more than natural laws. I have already provided quotes in which Popper claims to regard both the physical world

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[6] This is, of course, entirely consistent with his view that *"all universals are dispositional"* and that even terms like 'red' and 'glass' are universal and, thus, have a conjectural or theoretical character. Thus, when we speak of a glass of water, the terms 'glass' and 'water' actually reflect the existence of natural laws (Popper 1965, pp. 118–19; see also Popper 1968, pp. 94–95).

and theoretical entities as real. While his belief in the reality of the physical world could, I suppose, be reduced to a belief in underlying natural laws or regularities, for Popper, any belief in the reality of theoretical entitiesmust be intimately tied up with theories. This is because, like most other realists, Popper's whole approach is based on theory. Consequently, being able to make reference to theoretical entities depends entirely on the theories in which those entities are embedded.[7] Thus, Popper also has to be a realist about theories. And, in fact, he says that he is. In a passage confronting Berkeley's linguistic argument for instrumentalism, Popper asserts that the view of scientific theories as genuine descriptive statements "is a realist position: it makes it possible for us to say that a scientific theory, *or* a law of nature, can be true (even though we can never be sure of it)" (Popper 1983, pp. 110–11; italics added). It seems, then, that Popper does not want to draw any sharp distinction between natural laws and scientific theories. (Presumably, natural laws are to be reflected in scientific theories.) Both are to be interpreted realistically. But this means that the tension between falsificationism and realism in Popper's philosophy is not an illusion. Although Popper wants us to interpret scientific theories realistically, his falsificationist approach does not give us sufficient reason to do so.

This is related to the third problem, which is that, although the combination of falsificationism proper and weak metaphysical realism is consistent, it is not a convincing position; it is, instead, rather empty. The weak metaphysical realist notion of truth as a regulative idea (or as a standard) does not do enough work by itself to provide us with full-blooded realism (i.e., to give us adequate reasons to regard our scientific theories as reflecting reality). This version of metaphysical realism is simply too weak. As Hilary Putnam points out, such metaphysical realism entails that a highly verified (corroborated) theory, even if epistemically ideal, may still be false (Putnam 1979, p. 125). Consequently, metaphysical realism just collapses into the view that there is some ideal

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[7] Popper's realism about theoretical entities and unobservable causes is based on realism about theories. One could, however, take Hacking's approach and maintain a belief in the reality of theoretical entities from the standpoint of experimentation or manipulation, without being a realist about theories.

theory although we may never know when we have found it. Thus, metaphysical realism by itself cannot tell us where we are in the quest for truth. And, unfortunately, the connection between falsificationism and metaphysical realism is also far too weak. Popper plainly admits this weakness when he says, "The task of science, which, I have suggested, is to find satisfactory explanations, can hardly be understood if we are not realists. … And yet it seems to me that within methodology we do not have to presuppose metaphysical realism; nor can we, I think, derive much help from it, except of an intuitive kind" (Popper 1972, p. 203). If the epistemological/falsificationist and metaphysical/realist components of Popper's philosophy could somehow be more closely integrated, then perhaps a convincing conjunction of these two positions could be achieved.

VII. Conclusion

Let me now offer a brief summary of the various points of tension between falsificationism and realism that have been uncovered. I begin by highlighting the two points in which Popper's views were found to be consistent with realism but ran afoul of falsificationism. First, O'Hear makes the case that Popper makes significant inductivist concessions in his use of the concept of verisimilitude. And I argued that Popper treats Tarskian metalanguages as atheoretical, thus failing to reflect his own conjectural approach.

But what about all the ways in which Popper's views reflect his falsificationist methodology but fail to persuade us of realism? These are, of course, the important ones for my purposes since I am trying to discover whether realism can be convincingly conjoined with falsificationism. Although the points of tension of this type far outnumber those of the other type, I think it worthwhile to offer a complete enumeration here. First, I argued that Popper's claim that theories cannot justify the truth of their permitted basic statements does not square with realism. Second, we have seen that

Popper's view that neither universal statements (theories) nor basic statements can be proved or justified appears to go against realism. Third, I claimed that, although Popper's epistemological concept of corroboration is relative to a system of basic statements, his metaphysical notion of truth does not reflect this. Fourth, I showed that Popper's use of the concept of verisimilitude is limited to a comparative or regulative role and so gives us no reason to think that any theory possesses high *absolute* verisimilitude. Fifth, I argued, however, that verisimilitude as an absolute metaphysical concept cannot bridge the gap between Popper's falsificationist epistemology and his realism. Sixth, O'Hear offers a three-pronged critique that paints Popper as an instrumentalist rather than as a realist. Seventh, Popper's claim to be a realist about scientific theories and theoretical entities was shown to be thoroughly unconvincing. Eighth, following Putnam, I argued that metaphysical realism is simply too weak a form of realism to persuade us to interpret scientific theories realistically. And, finally, I claimed that the connection between falsificationism and metaphysical realism is also too weak to provide a convincing realistic interpretation of falsificationism.

Chapter 2: Alternative Versions of Realism

I. Introductory Remarks

There are, then, several major difficulties in Popper's attempt to simultaneously maintain falsificationism and realism. I have shown that there is, in fact, a significant tension between these two positions in Popper's thought. As I indicated at the outset, my aim is to find out whether some form of scientific realism is compatible with falsificationism. Thus, I focus my attention on the realist side of the problem. As my analysis in chapter one has shown, there are a number of aspects of Popper's form of realism that are thoroughly unconvincing given his falsificationist standpoint. But then the obvious question is: might there be some other form of realism that could be combined with falsificationism in such a way as to eliminate the tension and form a persuasive whole?

This is the question which serves as the focal point of chapter two. In addressing this question, my goal will be to explore *some* of the chief avenues or strategies that one might take in attempting to eliminate the tension. Given the plethora of different forms of realism, I obviously will not be able to explore all possible approaches to this question. Instead, I will simply analyze a few of the most promising alternative forms of realism in order to discover whether, and if so, to what extent, they eliminate the tension with falsificationism. This analysis should yield some valuable insights into the nature of the problem. In particular, it should reveal the specific pressure points existing between falsificationism and each form of realism.

But which of the many possible brands of realism should be examined? How can we even begin to decide which of the many possible alternatives to consider? What we need is a very basic scheme by which the various forms of realism can be classified. By introducing some order into the realist landscape and, perhaps, by also allowing a classification of Popper's form of realism, such a scheme would suggest a way to narrow the range of alternatives to be examined.

Fortunately, such a basic classification is provided by Jarrett Leplin in his introduction to a collection of papers on scientific realism that he edited (Leplin 1984).

Leplin maintains that there are two basic strategies employed by realists to defend scientific realism from its critics: 1) to focus on the problems of reference and truth and 2) to point out that certain elements of scientific method and/or reasoning are unintelligible without realism. Leplin uses this scheme to categorize all of the papers by realists in the collection. He suggests that any given philosopher's arguments can be classified in one or the other of these two groups, since such arguments must employ one or the other of the two strategies (Leplin 1984, p. 4).

Now if one could determine into which of the two groups Popper's arguments fit, then one might plausibly cut the range of relevant forms of realism to explore in half. That is, assuming that Popper's strategy in defending realism were required in some way to fall into one of these groups by his falsificationism, then one would only need to look at those forms of realism that employed the same type of strategy. On the other hand, if it were suspected that Popper's strategy was itself somehow at fault, then one might justifiably choose instead to examine only those forms of realism that utilized Leplin's other strategy.

But what strategy does Popper employ? We cannot obtain the answer to this question directly from Leplin, since, as no selection from Popper is included in *Scientific Realism*, Leplin does not categorize Popper's arguments. We can, however, reasonably surmise an answer. Popper regards falsificationism as the logical methodology of science, and he thinks that such a method is unintelligible unless one supposes an objective reality. That is, our attempts at falsification simply do not make sense if there is not an objective reality with which our theories may clash. This sort of argument fits squarely into Leplin's second strategy, i.e., that scientific method is unintelligible without realism. Yet this argument is largely implicit in Popper's writings. Much more explicit is the way in which he attempts to base his realism upon Tarski's correspondence theory of truth. In engaging with the question of truth in this way, however, Popper clearly employs Leplin's other strategy. Thus, I think it is fair to say that *both* of Leplin's strategies are represented in Popper's work. Sometimes he argues for realism on the grounds that, without it, scientific methodology is unintelligible, and other times he

argues for realism from considerations of truth and reference. This finding, though, means that we cannot use Leplin's classification in a straightforward manner.

Leplin's classification clearly has its faults. The works of certain philosophers such as Popper possess arguments which may be classified in each of Leplin's groups. Thus, although Leplin's scheme is useful when applied to specific realist arguments, it may not be as useful when applied to complete forms of realism, as represented in the thought of some philosophers. In addition, there are other legitimate ways of categorizing forms of realism. Leplin's scheme, therefore, cannot be regarded as definitive or exhaustive.

Nevertheless, it is still useful, for it highlights an important division among the various versions of scientific realism. Since Popper's thought possesses elements from both of Leplin's groups, it seems reasonable to utilize Leplin's scheme in the following manner. I will select a promising candidate version of realism from each of Leplin's groups. In addition, I will select a third promising version of realism that, like Popper's own version, possesses elements of both. Thus, we can examine examples of realism drawn from the spectrum of possibilities in a well-motivated way—one that reflects one major division of the spectrum.

Among the papers that Leplin classifies in the group which focuses on reference and truth is a paper by Hilary Putnam (1984). Although the paper, which was originally given as a lecture in 1976, is from Putnam's earlier, realist period, I think that Leplin's categorization can be extended to the arguments of the later, internal realist Putnam as well. In the works in which Putnam presents his internal realism, his strategy clearly is to argue from considerations of reference and truth. In fact, as I will argue later, Putnam can be viewed as providing a critique of Popper's use of Tarski's theory of truth. Thus, there is a definite point of contact here between Putnam and Popper. Moreover, Putnam's internal realism, with its indebtedness to Kant and its emphasis on the significance of conceptual frameworks, is consistent with Popper's conjectural approach. For these reasons, Putnam's internal realism represents a plausible candidate for potentially eliminating the tension between falsificationism and realism.

Among the papers that Leplin classifies in his other group—i.e., those that argue for realism from considerations of intelligibility—is a paper by Richard Boyd (1984). As Leplin points out, Boyd's strategy in defending realism is to offer an account of "the instrumental reliability of the theory-dependent methodology of the 'mature sciences'" (Leplin 1984, p. 5). Boyd argues that, despite the arguments of empiricists and constructivists to the contrary, such instrumental reliability is only truly intelligible if one accepts realism.[8] Boyd's form of realism—I will call it methodological realism, so that we have a shorthand name for it parallel to the names of the other forms of realism discussed—is a dialectical one in which our possession of approximately true theories provides for an instrumentally reliable methodology, which, in turn, allows us to improve our theories and get even closer to truth. In its acceptance of the theory-dependence of scientific methodology and in its emphasis on the dialectical nature of scientific discovery, Boyd's form of realism clearly has some affinity with Popper's falsificationism. Thus, Boyd's methodological realism is a second promising candidate.

Finally, Leplin's collection also includes a paper by Ian Hacking (1984). Although Leplin classifies Hacking's paper in the intelligibility group, many readers regard Hacking as primarily focusing on questions of reference. Such readers would argue that Leplin has misclassified Hacking. This apparent disagreement is evident in Leplin's own brief discussion of Hacking's paper. Leplin admits that Hacking's entity realism basically amounts to the claim that "The central terms of the best current theories are genuinely referential" (Leplin 1984, p. 1). This certainly makes it sound as if Hacking argues for realism from considerations of reference. I suggest that within Hacking's philosophy there are elements of both of Leplin's categories. Leplin's (and our) difficulty in classifying Hacking arises because Hacking's approach differs "significantly from familiar approaches in focusing on the nature of experimentation in

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[8] It should be pointed out that Leplin's classification of Boyd might be questioned, for Boyd also argues for realism from considerations of reference. In fact, he offers his own naturalistic theory of reference based on epistemic access. However, I think it is fair to say that such arguments are presented in the context of, and are subordinate to, Boyd's main argument, which is based on scientific methodology.

science as against theories and their successes" (Leplin 1984, p. 5). Hacking's entity realism, then, is a potential third candidate form of realism since it possesses elements of both of Leplin's groups. What makes it a promising choice is that it shares with Popper's approach a concern with discovering causal relationships. Hacking's version of realism is also promising because Hacking specifically argues against the more typical arguments for realism based solely on considerations of representation and language. As it turns out, one of Hacking's chief targets is Putnam's internal realism. Thus, Hacking's entity realism also provides a natural foil to Putnam's internal realism.

Let me now analyze how each of these three forms of realism relates to Popperian falsificationism in an effort to determine to what extent the tension between falsificationism and realism can be eliminated. I begin with an examination of Putnam's internal realism. I then consider Boyd's methodological realism before concluding the chapter with an analysis of Hacking's entity realism.

II. Putnam's Internal Realism

A. The Löwenheim-Skolem Theorem

In any discussion of Putnam's views, it is important to make clear which period of his thought is being considered. My discussion of Putnam will concern his internal realist period. For my purposes here, however, it is also important to see how Putnam's views during his internal realist period developed from his earlier views. Specifically, we need to understand why Putnam abandoned 'metaphysical realism' in favor of his own internal realism. An understanding of this question requires that we first understand the full significance that Putnam grants to the Löwenheim-Skolem theorem. I begin, therefore, with a brief description of that theorem.

As Hacking points out, the Löwenheim-Skolem theorem is a theorem of mathematical logic and was developed during a period when mathematicians were attempting to use postulated axioms to characterize mathematical objects, such as sets. A class of intended objects could then be defined by these postulated axioms (Hacking

1983, p. 102). The Löwenheim-Skolem theorem is the proof that any such theory (any such set of sentences or axioms), which is expressible in first-order logic and which has a model, has a denumerable model. Paradoxically, this is the case even if the domain of objects of which a theory is true is (provably, on the standard interpretation of the axiom system) non-denumerable (Flew 1984, p. 216; Hacking 1983, p. 103). For example, the real numbers can be characterized in a system consisting of finitely many axioms. Within this system of axioms, as standardly interpreted, it can be proven that the set of real numbers is non-denumerable. Application of the Löwenheim-Skolem theorem to this system, however, means that the system has a denumerable model according to which the set of real numbers is denumerable (Flew 1984, p. 216).

This paradox—the Skolem paradox—is really just the problem of unintended interpretations of theory. In the case of the real numbers, for example, although a given axiomatic system (theory) is supposed to capture our intuitive notion or intended interpretation of the reals, it turns out that there are models which flatly contradict our intended interpretation (i.e., models according to which the set of real numbers is denumerable rather than non-denumerable). Putnam points out that, since Skolem's argument applies to any system of statements formalizable in the first-order predicate calculus, it also "shows that even a *formalization of total science* (if one could construct such a thing), or even a *formalization of all our beliefs* (whether they count as 'science' or not), could not rule out denumerable interpretations, and, *a fortiori,* such a formalization could not rule out *unintended* interpretations" (Putnam 1983, p. 3). Thus, Putnam concludes that "the Skolem argument can be extended … to show that the *total use of the language* (operational plus theoretical constraints) does not 'fix' a unique 'intended interpretation' any more than axiomatic set theory by itself does" (Putnam 1983, p. 4).

B. Extension of Löwenheim-Skolem Theorem to Language

Putnam believes that Skolem's arguments can be extended to the philosophy of language and that, when thus extended, the 'Löwenheim-Skolem paradox'—as he calls it—is an

antinomy in the philosophy of language (Putnam 1983, p. 1). The problem arises when we look at language and attempt to understand the notions of truth and reference. More specifically, the problem arises when we ask, not how we understand our language, but, instead, how we can single out a unique, intended interpretation of our language—an interpretation that assigns names to objects and establishes criteria for when objects satisfy predicates in the way that we intend. As Putnam notes, we generally fix the interpretations of our language through the use of theoretical and operational constraints (Putnam 1981, p. 29). Theoretical constraints on interpretation are simply those formal features which we think any theory should possess, such as determinism, conservatism, or simplicity (Putnam 1981, pp. 30–31). Operational constraints, on the other hand, may take the following form: "'an admissible interpretation is such that *most of the time* the sentence S is true when the experiential condition E is fulfilled'" (Putnam 1981, p. 30).

Now the problem, Putnam contends, is that the Löwenheim-Skolem theorem shows that one can have interpretations of the language which satisfy all operational and theoretical constraints and yet are unintended—i.e., do not fix reference in the intended way. According to Putnam, the difficulty arises because the interpretation just fixes the truth-conditions for entire sentences (Putnam 1981, pp. 32–33). Thus, even if "operational and theoretical constraints … determine which sentences in the language are *true*. … such constraints cannot determine what our terms *refer* to." Putnam summarizes this result as follows: *"no view which only fixes the truth-values of whole sentences can fix reference,* even if it specifies truth-values for sentences *in every possible world"* (Putnam 1981, p. 33).

Putnam offers an illustration to support this claim. He says that the sentence 'A cat is on a mat' can be reinterpreted in such a way that its truth-value is unaffected in any possible world even when 'cat' and 'mat' refer to cherries and trees, respectively, in the actual world. Of course, according to the standard (intended) interpretation of this sentence, 'cat' refers to cats, 'mat' refers to mats, and the sentence is true whenever at least one cat is on at least one mat at some time (Putnam 1981, p. 33). Putnam argues that this ordinary sentence can be given a non-standard (unintended) interpretation in

which the sentence effectively means 'A cat* is on a mat*.' He explains that there are three relevant cases with regard to the definitions of these two new terms:

> (a) Some cat is on some mat, and some cherry is on some tree.
> (b) Some cat is on some mat, and no cherry is on any tree.
> (c) Neither of the foregoing. (Putnam 1981, p. 34)

Given these three possible cases, 'cat*' can be defined so that "x is a cat* if and only if case (a) holds and x is a cherry; or case (b) holds and x is a cat; or case (c) holds and x is a cherry." Similarly, 'mat*' can be defined so that "x is a mat* if and only if case (a) holds and x is a tree; or case (b) holds and x is a mat; or case (c) holds and x is a quark" (Putnam 1981, p. 34). Now we can imagine three different kinds of possible worlds, each characterized by one of the three cases, and see how the truth-value of the reinterpreted sentence compares with that of the standard interpretation. In possible worlds characterized by case (a), both 'A cat is on a mat' (standard interpretation) and 'A cat* is on a mat*' (non-standard interpretation) come out true. Likewise, in possible worlds characterized by case (b), both the sentences are true. And in possible worlds characterized by case (c), both sentences are false. Thus, according to Putnam, the truth-values of the sentences come out the same in all possible worlds despite the reinterpretation of the terms.

Putnam, however, does not content himself with merely providing an example of how a single sentence can be given an unintended interpretation. He goes further and offers a proof[9] to "show that a more complicated reinterpretation of this kind can be carried out for all the sentences of a whole language" (Putnam 1981, p. 35). On the basis of this proof, he claims "that there are always infinitely many different interpretations of the predicates of a language which assign the 'correct' truth-values to the sentences in all possible worlds, *no matter how these 'correct' truth-values are singled out*" (Putnam 1981, p. 35). But then, since such interpretations meet all operational and theoretical constraints, how can we say that they are unintended? As Putnam (1979, p. 126) puts it:

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[9] See the appendix in Putnam (1981).

"what *further* constraints on reference are there that could single out some other interpretation as (uniquely) 'intended' … ? The supposition that even an 'ideal' theory (from a pragmatic point of view) might *really* be false appears to collapse into *unintelligibility*."

C. The Problem with the Metaphysical Realist's Use of Tarski's Theory

This last sentence of Putnam really represents the heart of his argument against metaphysical realism. In order to understand Putnam's argument, however, we must first see how his extension of the Löwenheim-Skolem theorem to the philosophy of language undermines the metaphysical realist's use of Tarski's theory of truth. Now recall that Popper is a metaphysical realist who attempts to base his realism on Tarski's theory. Thus, Putnam's critique challenges metaphysical realism on precisely those grounds by which Popper attempts to support it.

Let us begin then by reexamining, from a Putnamian perspective, how a metaphysical realist like Popper utilizes Tarski's theory. First of all, we should remember that Popper regards Tarski's theory as having resurrected the correspondence theory of truth. By introducing the notion of a metalanguage, Tarski's theory is supposed to allow for meaningful talk of correspondence between statements and facts. The metalanguage is supposed to tell us the conditions under which the sentences of the object language are true (e.g., 'Snow is white' is true if and only if snow is white) and thus, also what the terms of the object language refer to. In short, the metalanguage is supposed to single out a unique interpretation of the object language. Thus, according to a metaphysical realist like Popper, Tarski's theory provides us with the desired correspondence between statements and *mind-independent reality or facts.*

Putnam contends, however, that this use of Tarski's theory will not work. Just as the object language may be interpreted, so may the metalanguage. So if we try to deny so-called unintended interpretations of the object language (e.g., one in which 'cat' refers to cherries) by pointing to the metalanguage sentence '"cat" refers to cats', we simply shift the problem to the metalanguage level. As Putnam explains, "The axiom of the

metalanguage, "'cat' refers to cats' can't rule out such an unintended interpretation of the object language, unless the metalanguage itself already has had *its* intended interpretation singled out" (Putnam 1983, p. 24). But, of course, this is just what Putnam's extension of the Löwenheim-Skolem theorem to language says is impossible; there will always be infinitely many interpretations.[10]

The problem, as Putnam sees it, is that the metaphysical realist reads too much into Tarski's theory. According to Putnam, Tarski's theory is philosophically neutral. It "does not commit one epistemologically or metaphysically" (Putnam 1983, p. 76); that is, it does not commit one to any particular understanding of truth. Thus, Tarski's theory "does not vindicate the correspondence theory of truth" (Putnam 1983, p. 83), as metaphysical realists like Popper think it does. Rather, in Tarski's theory, "'True' is just a device for 'semantic ascent': for 'raising' assertions from the 'object language' to the 'metalanguage' (Putnam 1983, p. 76).

D. Putnam's Critique of Metaphysical Realism

Metaphysical realists, though, regard Tarski's theory as supporting realist or truth-conditional semantics, according to which the understanding of the sentences of a language consists in knowing the truth conditions for those sentences. But then the metaphysical realist must explain what the knowledge of truth conditions itself consists in. According to Putnam, there are basically three possible answers to this question, each answer corresponding to one of the three main positions on reference and truth: moderate realism, Platonism, and verificationism (Putnam 1983, p. 1). First, one could take the approach of a naturalistically minded metaphysical realist (i.e., a moderate realist), which Popper certainly is, and claim that the knowledge of truth conditions must itself take the form of statements (Putnam 1983, p. 82). Second, one could take the extreme Platonist approach and assert that the knowledge of truth conditions consists in some mysterious,

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[10] As Hacking points out in his discussion of Putnam's internal realism, there is an additional problem here for the metaphysical realist. No matter what theory of reference the metaphysical realist puts forward, it will even be possible to reinterpret 'refer' itself (Hacking 1983, pp. 105–6).

primitive grasp of forms (Putnam 1983, pp. 1, 10, 82). Or, third, one could adopt the verificationist approach and claim that the knowledge of truth conditions simply consists in the mastery of verification or proof procedures (Putnam 1983, pp. 1, 20).

There are a couple of problems with the second approach. Most significantly, it requires that the mind be able to grasp the truth conditions of a sentence without the use of mental representations and, therefore, that it be able to directly compare a sentence with unconceptualized reality. But such a requirement seems implausible.[11] Furthermore, this approach "seems both unhelpful as epistemology and unpersuasive as science" (Putnam 1983, p. 10). Putnam points out, however, that this approach is *not* threatened by the Löwenheim-Skolem theorem since, according to Platonism, a unique intended interpretation can still be picked out by our faculty of grasping forms (Putnam 1983, pp. 4, 10). Thus, this approach, while implausible, is not ruled out by Putnam's use of the Löwenheim-Skolem theorem.

Unfortunately, as we have seen, the same cannot be said for the first approach, moderate realism, "which seeks to preserve the centrality of the classical notions of truth and reference without postulating non-natural mental powers" (Putnam 1983, pp. 1–2). In fact, Putnam thinks that, of the three possible approaches, only moderate realism is threatened by the Löwenheim-Skolem theorem (Putnam 1983, pp. 2, 4). This result stems from the nature of metaphysical realism and what it requires. As Putnam puts it, according to metaphysical realism, "THE WORLD [i.e., mind-independent reality, the world as it is in itself] is supposed to be *independent* of any particular representation we have of it—indeed, it is held that we might be *unable* to represent THE WORLD correctly at all" (Putnam 1979, p. 125). In fact, "The most important consequence of metaphysical realism is that *truth* is supposed to be *radically non-epistemic*—we might be 'brains in a vat' and so the theory that is 'ideal' from the point of view of operational

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[11] Putnam assesses this position even more harshly, for he states that "The notion that 'grasping truth conditions' (e.g., grasping *what it is for snow to be white*) is *prior* to understanding the corresponding signs (understanding the sentence 'Snow is white' or some representation with the same meaning) is absurd" (Putnam 1983, p. 82).

utility, inner beauty and elegance, 'plausibility', simplicity, 'conservatism', etc., *might be false*" (Putnam 1979, p. 125). Thus, as Putnam points out, metaphysical realism is really "a model of the relation of *any* correct theory to all or part of THE WORLD" (Putnam 1979, p. 123), a model which requires a unique correspondence between the symbols of such a theory and the objects of THE WORLD (Putnam 1981, p. 51). Alternatively, we can say that metaphysical realism requires that there be one correct interpretation of the language by which we represent the world. Putnam explains the problem with this requirement as follows:

> To adopt a theory of meaning according to which a language whose whole use is specified still lacks something—namely its 'interpretation'—is to accept a problem which *can* only have crazy solutions. To speak as if *this* were my problem, 'I know how to use my language, but, now, how shall I single out an interpretation?' is to speak nonsense. Either the use *already* fixes the 'interpretation' or *nothing* can. (Putnam 1983, p. 24)

The problem with metaphysical realism, then, is that it appears to set up a requirement that it cannot meet. We have already seen how the other form of metaphysical realism, Platonism, fails to plausibly satisfy this requirement. Putnam maintains that even moderate metaphysical realism cannot satisfy the requirement. This is because, according to moderate realism, the knowledge of truth conditions must itself take the form of statements. But then we need a theory of our understanding of these statements, and it is no help to appeal once again to the knowledge of truth conditions, for this appeal simply leads to an infinite regress (Putnam 1983, p. 82). The problem is that such statements are just further mental representations, further theory and, therefore, according to Putnam's extension of the Löwenheim-Skolem theorem to language, cannot single out a unique, intended mapping of terms onto objects of mind-independent reality. Thus, by connecting the theory of language understanding with the theory of reference

and truth, moderate metaphysical realism creates a problem for itself that it cannot solve.[12]

E. Putnam's Internal Realism

Having shown the difficulties associated with metaphysical realism, especially in its moderate form, Putnam is compelled to develop his own alternative form of realism, internal realism.[13] Drawing upon insights gained from his critique of metaphysical realism, he is led both to deny the connection between "the theory of language understanding and the theory of reference and truth" (Putnam 1979, p. 97) and to reject truth conditional semantics. For Putnam, the problem with truth conditional semantics is that it makes truth prior to meaning (Putnam 1979, p. 110). But he points out that "one does not need to *know* that there is a correspondence between words and extra-linguistic entities to learn one's language" (Putnam 1979, p. 111). Instead, in agreement with the quotation given above (p. 49), Putnam maintains that language understanding is "the possession of a rational activity of language use" (Putnam 1979, p. 110) and, thus, does not require the notions of reference or truth.[14] Thus, he explicitly rejects the metaphysical realist view "that *truth* and *rational acceptability* should be *independent* notions" (Putnam 1983, p. 11).

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[12] It should be pointed out here that a moderate metaphysical realist may be able to satisfy this requirement in a different way. That is, he could maintain that understanding a language requires one to have the conceptual apparatus to develop the concepts of reference and truth. Rather than making the reference of terms dependent upon the truth conditions of sentences, one could instead build up to truth conditions by making them dependent upon being able to successfully refer to things in the world. Of course, Popper does not adopt this approach and, thus, Putnam's argument against Popper's use of Tarski's truth theory still holds.

[13] For historical accuracy, it should be noted here that Putnam's internal realism represents a technical development and refinement of an idea put forth earlier by Rudolf Carnap (1953). Carnap offers an anti-metaphysical argument also based on the notion of frameworks, but without making reference to the Löwenheim-Skolem theorem. Carnap's idea is that we adopt various linguistic frameworks, some of which include terms for abstract entities. Within such frameworks, 'internal' questions can be decided either logically or empirically. Most importantly, semantical questions are also internal to a chosen framework. Furthermore, according to Carnap, the decision to adopt a particular linguistic framework is strictly practical or pragmatic and thus does not require a theoretical or 'external' justification.

[14] For Putnam, the notions of reference and truth—in the sense of correspondence between words and things—do, however, play a role in explaining the success of a community's collective linguistic behavior (Putnam 1979, pp. 111, 123).

Now there is still one approach to the question of what the knowledge of truth conditions consists in that I have not yet discussed. This is the verificationist approach, and it is the approach adopted by Putnam. Like Platonism, verificationism is not threatened by the Löwenheim-Skolem theorem. This is because, by identifying truth with verifiability, verificationism can accept the truth of so-called paradoxical statements derived from 'unintended' interpretations (e.g., that the set of real numbers is denumerable): such statements are true if they can be proved within the model (Putnam 1983, p. 4). Thus, for Putnam, "truth is an *idealization* of rational acceptability" (Putnam 1981, p. 55)[15] according to which "A statement is true … if it would be justified under epistemically ideal conditions for many sorts of statements" (Putnam 1983, p. 84). Now, in this type of verificationist semantics—what Michael Dummett calls 'non-realist semantics'—the notions of truth and reference are not required (Putnam 1979, pp. 127–28). Furthermore, within this type of semantics "there is no 'basis' of hard facts (e.g. sense data)" (Putnam 1979, p. 128); there are only facts which are provable within the language as it is understood (used).

Putnam, however, prefers to call this type of semantics verificationist because he believes that it is compatible with his internal realism (Putnam 1979, pp. 128–29). That is, he believes that the acceptance of verificationist semantics is compatible with the realist view that there is a correspondence between words and things (Putnam 1979, p. 129). But, as Putnam's use of the Löwenheim-Skolem theorem shows, such correspondence cannot be understood outside of a model or interpretation; all correspondence is internal to a conceptual framework. Internal realism, then, is just the view that "'Objects' do not exist independently of conceptual schemes. *We* cut up the world into objects when we introduce one or another scheme of description. Since the objects *and* the signs are alike *internal* to the scheme of description, it is possible to say what matches what" (Putnam 1981, p. 52). Thus, according to internal realism, it simply

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[15] Putnam rejects Dummett's identification of truth with mere justification or rational acceptability since to do so is "to give up the principle that some of the statements which are *now* justified may turn out not to be *true*" (Putnam 1983, p. 85).

does not make sense to ask, independently of a conceptual scheme, how many objects exist (Putnam 1987, p. 20). Putnam claims that "Internal realism is, at bottom, just the insistence that realism is *not* incompatible with conceptual relativity. One can be *both* a realist *and* a conceptual relativist" (Putnam 1987, p. 17). Putnam insists that internal realism is, indeed, realism because "*within* a conceptual scheme truth is not a matter of convention" (Putnam 1987, p. 20); once a conceptual scheme is chosen, reality 'kicks back' and determines the correct answers to our questions. Putnam effectively describes both the realist aspect and the internalist aspect of internal realism when he states, "There are 'external facts', and we can *say what they are*. What we *cannot* say—because it makes no sense—is what the facts are *independent of all conceptual choices*" (Putnam 1987, p. 33).

F. The Relationship between Internal Realism and Falsificationism

Now we are in a position to determine how internal realism relates to falsificationism and to what extent this form of realism eliminates the tension between falsificationism and realism that we discovered in Popper's philosophy. Let me begin by discussing those aspects of internal realism that are promising in this regard, i.e., those aspects which are consistent with falsificationism. First, internal realism clearly embodies the conjectural approach of Popperian falsificationism. By emphasizing the role in representation played by conceptual schemes or frameworks, internal realism shows its appreciation of Popper's argument, offered against Hume's psychology of induction, that the perception of repetitions or regularities requires interpretation or conjecture on our part. In fact, the internal realist view of language seems to capture the spirit of Popper's conjectural approach in a way that Popper's own view of language, as represented in his use of Tarski's truth theory, does not.

In addition, like falsificationism, internal realism accepts the fact that reality 'kicks back' at our conjectures—that our conjectures can be in disagreement with the facts. Of course, internal realism insists that the facts which kick back at our conjectures

are relative (internal) to the conceptual scheme employed. The important point, however, is that internal realism, unlike idealism or relativism, acknowledges that there is an external world independent of our minds, with which the conjectures of our minds can be in disagreement.

Finally, there is a parallel between the verificationist semantics of internal realism and the conventionality of basic statements in falsificationism. Just as within verificationist semantics there is no hard basis of facts upon which the semantics rests, so too within falsificationism there is no solid bedrock of basic statements upon which falsification rests. Thus, internal realism is also consistent with falsificationism in being non-foundational.

Unfortunately, if we examine this last parallel a bit further, we uncover a difficulty with internal realism. According to the verificationist semantics of internal realism, a statement is true if it can be proved or justified under ideal circumstances. Thus, if we imagine an ideal scientific theory—one that is complete, consistent, satisfies all operational and theoretical constraints, and correctly predicts "all observation sentences *(as far as we can tell)*"[16]—and ask whether or not this theory is true, the internal realist will answer yes (Putnam 1979, pp. 125-26). But this is exactly what you *cannot* say according to the logic of Popper's falsificationism: even such an ideal theory that has passed the most severe tests may be (and, strictly speaking, probably is) false. The basic difficulty here with internal realism is just that, by incorporating verificationist semantics, it allows us to attribute truth, rather than mere approximate truth, to theories. More fundamentally, by adopting verificationist semantics, internal realism runs afoul of falsificationism's explicit prohibition of any form of justificationism or verificationism.

There is a second, related difficulty with internal realism. According to internal realism, two radically different theories about the world can *both* be true, depending upon the conceptual scheme adopted. Thus, even a basic categorial property like cardinality

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[16] To satisfy Popper here, we would have to imagine the theory to be a universal statement, and we would have to specify all the necessary initial conditions.

turns out to be conceptually relative (Putnam 1979, p. 133). For example, given a world of three individuals and asked to provide the number of objects in such a world, one can respond with two equally correct answers. One could, like Carnap, reply that there are three objects. On the other hand, if one allows mereological sums, one could truthfully reply that there are seven objects. Even something as basic as the number of objects in the world, then, turns out to be conceptually relative (Putnam 1987, pp. 18–20).

In a certain sense this difficulty might count as trivially semantic, since even a metaphysical realist would admit the importance of one's choice of conceptual schemes. Yet there is a deeper problem here. According to internal realism, metaphysical object-categories are necessarily dependent on our conceptual schemes. Thus, internal realism denies that there are absolute object-categories existing independently of our thought. What this means is that internal realism cannot be scientific realism, because internal realism does not privilege any one particular account of the world, much less the scientific account. But falsificationism presupposes that the world is a certain definite way (i.e., that there are absolute metaphysical object-categories independent of any conceptual scheme) and that the 'right' scientific theory could represent that way. Thus, in this respect, internal realism fails both to provide us with a distinctly scientific understanding of the world and to satisfy the requirements of falsificationism.[17]

Finally, there is a third fundamental difficulty with internal realism that is related to the first. The difficulty is that the verificationist semantics of internal realism does not utilize the notion of approximate truth. Instead, it simply gets by with truth as an epistemic concept (Tymoczko 1993, p. 407), i.e, truth with a little 't'. Actually, it is not readily conceivable how verificationist semantics *could* utilize the notion of approximate truth. The idea of approximate truth does not seem to make sense except by comparison

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[17] It should be pointed out here that Popper, like Putnam, professes an affinity for commonsense realism as well. One might, therefore, suspect a tension between commonsense realism and scientific realism in Popper's thought. I think it is fair to say, however, that Popper is fundamentally a scientific realist. The whole point of falsificationism, for Popper, is to advance scientific theories that get closer and closer to the truth about the world.

with absolute truth, i.e., Truth with a capital 't'. Yet, as I have shown in my discussion of Popper, the notions of Truth and approximate truth are what implicitly drive falsificationism and give it its purpose. Without the idea that successive scientific theories are closer and closer approximations to Truth, falsificationism ceases to be intelligible. Since internal realism fails to capture the idea of approximate truth, it is not ultimately compatible with falsificationism.

III. Boyd's Methodological Realism

Given the difficulties resolving internal realism with falsificationism, I turn now to another promising version of realism which also embodies Popper's conjectural approach. This is the realism of Richard Boyd, which, as I have said, I will call methodological realism.[18] After describing this form of realism, I discuss some of its epistemological and methodological consequences. I then examine in detail the relationship between methodological realism and falsificationism in order to determine what impact methodological realism has on the tension between falsificationism and realism.

A. Description of Methodological Realism

Fundamentally, Boyd seems to see his project as defending scientific realism from attacks both by empiricists and by constructivists. In order to do this, he contends that we must reject the main epistemological principles underlying empiricism and constructivism (Boyd 1984). According to Boyd, the main epistemological principle of empiricism is "the evidential indistinguishability thesis," according to which "empirically equivalent theories [i.e., those which make identical predictions about observable phenomena] are evidentialy indistinguishable; therefore, knowledge cannot extend to

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[18] Unfortunately, Boyd himself does not suggest a name. I have chosen this name because Boyd's way of arguing for scientific realism is to focus on features of scientific *methodology*. In particular, he focuses on the integral role played in methodology by theoretical considerations. He also stresses the dialectical nature of scientific methodology. Thus, I suppose it would be equally legitimate to call his form of realism 'theoretical realism' or 'dialectical realism'.

'unobservables'" (Boyd 1984, p. 43). On the other hand, the common epistemological thread in constructivist arguments, according to Boyd, is that scientific methodology is so theory-laden that the world of which it gains knowledge must be merely a theoretical construction (Boyd 1984, p. 52). Boyd thinks that these two epistemological principles must be rejected because they fail to answer a critical question. As he explains, "… no empiricist or constructivist account of the methods of science can explain the phenomenon of instrumental knowledge in science, the very kind of scientific knowledge about which realists, empiricists, and constructivists largely agree" (Boyd 1984, p. 64). Now, the question of instrumental knowledge in science (or of the instrumental reliability of scientific methodology) is particularly pressing for Boyd because he agrees with the constructivists that scientific methodology is thoroughly theory-laden. Thus, the question that occupies him is: why is scientific methodology, given its dependence on theory, instrumentally reliable?[19] More specifically, Boyd wonders:

> … why are theory-dependent standards for assessing projectability [of predicates] and degrees of confirmation [of theories] instrumentally reliable? and, how do judgments of univocality for theoretical terms contribute to the instrumental reliability of scientific methodology? (Boyd 1984, p. 58)

Boyd thinks that puzzling questions of this sort can only be answered by invoking scientific realism. The invocation of scientific realism as the only available explanation of the instrumental reliability of scientific methodology, in turn, lends support to the truth of scientific realism. Thus, as Boyd acknowledges (Boyd 1984, p. 66), he offers an abductive argument of the form:

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[19] For Boyd, scientific methodology is said to be instrumentally reliable if it contributes to the production of instrumental knowledge. Instrumental knowledge, in turn, is defined as "the knowledge about particular theories that they are instrumentally reliable" (Boyd 1981, p. 616). And, finally, a scientific theory is said to be instrumentally reliable if it provides "approximately accurate predictions about the behavior of observable phenomena" (Boyd 1981, p. 616).

Premise 1:   Scientific methodology is instrumentally reliable.
Premise 2:   If scientific realism were true, then it would account for the instrumental reliability of scientific methodology.
Premise 3:   No other available plausible position can account for the instrumental reliability of scientific methodology.
Conclusion: Therefore, scientific realism is probably true.

Boyd's argument, then, is that "… a realistic account of scientific theories is a component in the only scientifically plausible explanation for the instrumental reliability of scientific methodology" (Boyd 1984, p. 58). Of course, ultimately Boyd's defense of scientific realism depends on how plausibly he explains the way in which scientific realism accounts for instrumental knowledge in science (i.e., in how he explains the conditional of premise 2). Obviously, Boyd's explanation must give a convincing account of the role played by theory in producing instrumental knowledge. In fact, Boyd admits that, given his rejection of empiricist and constructivist epistemological principles, an adequate defense of scientific realism requires a different view of epistemology—one that explains how a theory-dependent methodology can be instrumentally reliable. Moreover, he thinks that we need "a distinctly realist conception of the logic and methods of science" in order to explain instrumental knowledge in science (Boyd 1984, p. 64). In short, Boyd needs to give us a version of scientific realism that makes its role as an explanans of instrumental knowledge in science plausible.

Boyd attempts to do just that in developing his methodological realism. Since methodological realism incorporates many rather sophisticated and subtle points, I think it wise to begin my description of it simply by presenting it in broad outline. First, Boyd claims that "The realist … must hold that the reliability of the scientific method rests upon the logically, epistemically, and historically contingent emergence of suitably approximately true theories" (Boyd 1984, pp. 64–65). It is critical, on this account, that "… the laws and theories embodied in our actual theoretical tradition are approximately true" (Boyd 1984, p. 59). Second, Boyd thinks we must accept the fact that "… the methodology of science might progress dialectically. Our methodology, based on approximately true theories, would be a reliable guide to the discovery of new results and the improvement of older theories. The resulting improvement in our knowledge of the

world would result in a still more reliable methodology leading to still more accurate theories, and so on" (Boyd 1984, p. 59). Third (and, perhaps, most importantly), Boyd explains how theoretical considerations actually contribute to instrumental reliability. He claims that

> … if the realist and dialectical conception of scientific methodology is right, then considerations of the theoretical plausibility of a proposed theory, in the light of the *actual* (and approximately true) theoretical tradition are *evidential* considerations: results of such assessments of plausibility constitute evidence for or against proposed theories. Indeed, such considerations are a matter of theory-mediated empirical evidence, since the background theories, with respect to which assessments of plausibility are made, are themselves empirically tested (again, in a theory-mediated way). Theory-mediated evidence of this sort is no less empirical than more direct experimental evidence—largely because the evidential standards that apply to so-called direct experiential tests of theories are theory-determined in just the same way that judgments of plausibility are. (Boyd 1984, pp. 60–61)

Thus, there are three main elements of methodological realism: 1) the possession of approximately true theories (or of an approximately true theoretical tradition), 2) a dialectical conception of scientific discovery according to which theoretical knowledge and scientific methodology interact in such a way so as to correct and improve each other, and 3) the integral role played in this dialectical process by theoretical considerations. With this conception of scientific realism, Boyd believes he can answer the first of the two questions given in the quotation on p. 46. That is, he claims "that the reliability of theory-dependent judgments of projectability and degrees of confirmation can only be satisfactorily explained on the assumption that the theoretical claims embodied in the background theories which determine those judgments are relevantly approximately true, and that scientific methodology acts dialectically so as to produce in the long run an increasingly accurate theoretical picture of the world" (Boyd 1984, p. 59).

Now let us take a closer look at two key specific features of methodological realism beginning with the importance of theoretical considerations in scientific methodology. Boyd expresses this idea powerfully, as follows: "The basic idea which I have defended is that theoretical considerations are so heavily and so crucially involved

in the operation of actual scientific method that the only way to explain even the *instrumental* reliability of that method is to portray it as reliable with respect to theoretical knowledge as well" (Boyd 1981, p. 618). The question here is that, since infinitely many proposed theories make the same observational predictions when added to "an existing body of accepted scientific theories," (Boyd 1981, p. 618) why does our use of theoretical considerations to choose only a few theories to test actually work? That is, we need to explain why, even though "… only a few of the infinitely many equivalence classes of alternative modifications to our existing total science are taken seriously with respect to any given issue. … This 'narrowing down' of our options seems to contribute to the instrumental reliability of the scientific method, rather than to detract from it, as one might expect" (Boyd 1981, p. 619). Boyd thinks that this phenomenon can only be explained if the scientific theories which comprise our current total science embody actual theoretical knowledge of the world.

Boyd fleshes out this picture of how theoretical considerations contribute to instrumental reliability. According to Boyd theoretical considerations are represented in three methodological principles. The first principle, having to do with theory choice, is the one that I have just discussed. This principle might be called 'conservatism', according to which we "take seriously only those theories which relatively closely resemble our existing theories in respect of their ontological commitments and the laws they contain" (Boyd 1981, p. 618). Boyd maintains that this principle contributes to instrumental reliability because, if our existing theories "provide a sufficiently accurate picture of the 'furniture of the world' and how it works, then the operation of this principle will serve to make it more likely that theories which we take seriously are themselves approximately true" (Boyd 1981, pp. 621–22). The second principle is one which "countenances theoretical criticism, modification or extension of procedures of measurement and detection for 'theoretical' entities and magnitudes" (Boyd 1981, p. 619). According to Boyd, this principle too would enhance instrumental reliability provided that "well-confirmed theories are approximately true of real entities and … that 'measurement' and 'detection' of theoretical entities really are *measurement* and

detection" (Boyd 1981, p. 622). And the third principle concerns experimental design. It suggests that we use theoretical criticism to determine "Which finite (and typically small) number of experimental tests" allow "for an assessment of the predictive reliability" of any proposed plausible theory (Boyd 1981, p. 620). Boyd also thinks that this principle would contribute to instrumental reliability by isolating the "respects in which a proposed theory is (speaking evidentially) most likely to fail if it is going to fail at all" (Boyd 1981, p. 622).

Finally, another very significant specific feature of methodological realism has to do with reference and the way that Boyd explains how linguistic terms are successively accommodated to reality. This is a significant feature because it allows Boyd to show that "… the phenomenon of reference displays the pattern of dialectical accommodation by successive approximation which is characteristic of scientific knowledge and scientific methodology. … the phenomena of partial denotation and subsequent denotational refinement illustrate the fact that the 'tightness' of fit between language and the world increases as the dialectical process of theory refinement proceeds" (Boyd 1981, p. 649). Boyd's theory of reference is a naturalistic one which "defines reference in terms of 'epistemic access'" (Boyd 1984, p. 62). According to this theory, "Roughly, a (type) term *t* refers to some entity *e* just in case complex causal interactions between features of the world and human social practices bring it about that what is said of *t* is, generally speaking and over time, reliably regulated by the real properties of *e*" (Boyd 1984, p. 62). Now what this theory does is connect the reference of linguistic terms with knowledge of theoretical entities. Thus, Boyd's theory is one in which there is a distinct parallel between the "'micro-structural' components" of reference and knowledge (Boyd 1981, p. 648). That is, according to the epistemic-access theory of reference, "THE CONSTITUENTS OF RELIABLE BELIEF REGULATION ARE THE SAME AS THE CONSTITUENTS OF REFERENCE. KNOWLEDGE AND REFERENCE (BETTER: KNOWING AND REFERRING) HAVE THE SAME 'MICRO-STRUCTURAL' COMPONENTS" (Boyd 1981, p. 648). This theory has the important consequence of showing why we are justified in holding the unity of science epistemological principle,

which requires judgments of univocality for theoretical terms occurring in different theories (Boyd 1981, p. 645). We are justified because "Univocality for theoretical terms … is sameness of *reference*" (Boyd 1981, p. 646).Boyd's realistic picture of science has three components: scientific knowledge or theories, scientific language, and scientific methodology. Boyd claims that all three of these components "represent hard-won victories in a continuing struggle to accommodate our intellectual practices to the structure of an independently existing world" (Boyd 1981, p. 613). On this account, then, "Scientific knowledge extends to both the observable and the unobservable features of the world, but it is achieved by a process of successive approximation: typically, and over time, the operation of the scientific method results in the adoption of theories which provide increasingly accurate accounts of the causal structure of the world. If we think of beliefs or theories as being 'accommodated' to the world insofar as they are accurate descriptions of some of its features, then scientific knowledge procedes by accommodation by successive approximation" (Boyd 1981, pp. 613–14). Furthermore, "What is true of scientific knowledge is also true of scientific language as well. … The very mechanisms of reference—the ways in which scientific terminology is connected to features of the world—undergo a development—typically in the direction of a closer and 'tighter' fit between scientific terminology (in use) and the important causal features of reality. Reality is prior to thought not only in that its structure is largely independent of what we believe, but also in that the very machinery of thought (or, at any rate, of the public expression of thought) undergoes continuous accommodation to the structure of a largely independent causal reality" (Boyd 1981, p. 614). Finally, Boyd claims that "Not only do theories and language accommodate to the world by successive approximation: so do the scientific methods and epistemological principles by which knowledge is achieved. … the more general features of scientific or experimental method develop by successive accommodation to the causal structure of the world" (Boyd 1981, p. 614).

B. Some Consequences of Methodological Realism

I will now examine some of the epistemological and methodological consequences which Boyd thinks follow from his methodological realism and which are also relevant to Popper's philosophy. I begin with the following claim made by Boyd: "It is not possible to draw the distinction between instances of knowledge and instances of non-knowledge in a philosophically revealing way" (Boyd 1981, p. 623). With this statement, Boyd contrasts his naturalistic and realistic view of knowledge both with the traditional account of knowledge (as justified true belief) and with more recent causal theories, since both attempt to provide a priori definitions of knowledge that *can* be used to distinguish cases of knowledge from non-knowledge (Boyd 1981, p. 623). Boyd believes that methodological realism shows that such a priori standards of knowledge are not possible. Instead, methodological realism shows "that the epistemic status of inductive strategies is not an *a priori* matter. The actual inductive strategies which we employ at a given point in the history of science will reflect theoretical commitments characteristic of that time, *and* these strategies will be reliable (even instrumentally reliable) only if the relevant theoretical commitments are nearly enough true and comprehensive. Since the truth and comprehensiveness of a body of scientific theories cannot be decided *a priori,* there are no *a priori* standards sufficient for the epistemological assessment of actual scientific methods and practices" (Boyd 1981, p. 626). Thus, according to Boyd, "epistemology emerges … as the largely *a posteriori* study of a very complex natural phenomenon—the reliable development of successively more accurate and comprehensive theories and beliefs, about both observable and unobservable features of the world" (Boyd 1981, p. 628). This view of epistemology is certainly consistent with Popper's falsificationism, according to which we put forward conjectures about the world, which, though a priori, are not a priori valid (i.e., cannot be justified a priori) (Popper 1965, p. 47). In fact, according to falsificationism, knowledge is a contingent matter entirely dependent upon severe empirical testing. Furthermore, since Popper maintains that all knowledge is conjectural anyway, strictly speaking, all conjectures or theories count as knowledge and no distinctions are necessary.

Methodological realism, though, is characterized not only by its "abandonment of *a priori* standards for the evaluation of inductive strategies" but also by its "employment of unreduced causal notions in the analysis of knowledge" (Boyd 1981, p. 625). That is, methodological realism "explains the causal reliability of scientific method with respect to instrumental knowledge not by appealing to *a priori* principles but by assuming that the relevant background theories which determine the method's operation are approximately true and comprehensive descriptions *of the unobservable causal factors which underlie the relevant observable properties of observable phenomena*" (Boyd 1981, pp. 626–27). According to Boyd, "Scientific knowledge extends to both the observable and the unobservable features of the world, … typically, and over time, the operation of the scientific method results in the adoption of theories which provide increasingly accurate accounts of the causal structure of the world" (Boyd 1981, pp. 613–14). Thus, like Popper, Boyd professes knowledge of unobservable causes. But while Popper's profession of such knowledge is rather hollow given his falsificationism, Boyd's is much more persuasive because of his argument showing that such knowledge is actually required for the instrumental reliability of scientific methodology.[20]

There is a third consequence of methodological realism that is relevant to Popper's thought. Boyd claims that if methodological realism is correct, then what is epistemologically important is "reliability in the *regulation* of belief (over time)" and not "reliability in the *initial* production *or acceptance* of particular beliefs." And he thinks this is true not just of scientific beliefs but of many everyday beliefs as well (Boyd 1981, p. 630). Now there are two parallels here with Popper's thought. First, like Popper, Boyd treats scientific knowledge as an example of general knowledge (i.e., as continuous

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[20] Boyd's emphasis on causes can also be seen in his treatment of scientific language and methodology. With regard to the former, he claims: "The very mechanisms of reference—the ways in which scientific terminology is connected to features of the world—undergo a development—typically in the direction of a closer and 'tighter' fit between scientific terminology (in use) and the important causal features of reality" (Boyd 1981, p. 614). As to methodology, he says that "the more general features of scientific or experimental method develop by successive accomodation to the causal structure of the world" (Boyd 1981, p. 614). Thus, according to Boyd, causes are reflected not only in scientific theories but also in scientific terminology and methodology.

with general knowledge).[21] Second, Boyd's notion of reliability of belief regulation over time is in agreement with Popper's view that what is important is the growth of knowledge. Recall that for Popper it is not important that we put forth conjectures that we know to be true (since they will almost certainly be false), but that we prefer those conjectures which pass the most severe tests and, thus, possess a higher relative approximate truth.

Yet another relevant epistemological consequence of methodological realism concerns truth. Boyd claims that exact (or absolute) truth is not an epistemologically important concept. Like Popper, Boyd regards exact truth as "radically non-epistemic," to use Putnam's phrase (Boyd 1981, p. 660). And he says, "… if by 'knowledge' we refer to the sort of thing which careful everyday and scientific investigations aspire to and sometimes achieve …, then (exact) truth is not necessary for knowledge" (Boyd 1981, p. 631). This is because "Truth (or, rather, *exact* truth) is disconnected from our rational methods in a way in which reference and approximate truth are not" (Boyd 1981, p. 660). Thus, according to Boyd: "It is mistaken to define knowledge in terms of exact truth *or* in terms of reliable belief *production* (or justification *at a time*) precisely because the natural phenomenon in which knowledge is manifested involves a dialectical process of *successive approximation* to the truth, whose reliability consists in a tendency *over time* for the successive approximations to be increasingly accurate" (Boyd 1981, p. 631). For Boyd, then, the epistemologically important concept is approximate truth. Here there are two strong similarities with Popper's views. First, as we have already seen, Popper dispenses with truth in favor of verisimilitude or approximate truth. Second, Boyd accepts the same sort of metaphysical realist view of truth (exact or absolute) that Popper endorses.

A fifth relevant epistemological consequence of Boyd's form of realism has to do with the nature of experimental knowledge. Boyd claims that the dispute between the

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[21] Cf. Boyd' s statement (1981, p. 622) in which he says that if methodological realism is right, "then several consequences follow which are significant for our understanding of the epistemology of science *(and epistemology generally, for that matter)*."

realistic and empiricist conceptions of knowledge "is not over the primacy of experimental knowledge [both accept this], but over its nature" (Boyd 1981, p. 638). According to Boyd's realist conception of knowledge, "There are no theory neutral standards of 'direct' experimental evidence for a theory. The evidential support (or potential disconfirmation) which a theory receives from tests of its observational consequences depends crucially on theory-mediated evidential considerations" (Boyd 1981, p. 637). Now this idea also appears to be consistent with Popper's falsificationism. Here we must recall Popper's conventionalism with regard to basic statements. Any given basic statement is itself falsifiable since other testable basic statements can be deduced from its conjunction with a suitable theory. Thus, in order to prevent an infinite regress of testing, Popper insists that we must simply decide to accept certain basic statements. Although Popper does not explicitly say so, this decision would necessarily have to be based on our current background theories about the nature of the world. That is, our decision to accept certain basic statements would itself have to be a conjecture about the nature of the world. Thus, according to falsificationism as well, experimental evidence for or against a theory reflects theoretical considerations.

Finally, there is still another parallel, this time concerning rigorous testing, between Boyd's methodological realism and Popper's falsificationism. Boyd thinks that rigorous testing of scientific theories actually *requires* theoretical considerations. He states: "… if the realistic account of scientific method presented here is correct, then 'indirect' or theory-mediated evidential considerations are absolutely essential to the *rigorous* assessment of the evidence for or against a proposed theory. This is true because theory-mediated evidential considerations are essential in identifying the experimental conditions under which a proposed theory must be tested, in order that a rigorous assessment be possible" (Boyd 1981, p. 638). Now, rigorous or severe testing is, of course, the sine qua non of falsificationism. Thus, Boyd's methodological realism can be seen as elucidating the nature of this most important element of falsificationism in a way which is consistent with Popper's overall conjectural approach.

C. The Relationship between Methodological Realism and Falsificationism

I begin my discussion of the relationship between methodological realism and falsificationism simply by summarizing the major points of agreement revealed thus far. Both hold that

1) epistemology is an a posteriori and naturalistic enterprise,
2) unobservable causes are a necessary component of scientific knowledge,
3) knowledge is a dynamic process, and thus the growth or regulation of knowledge is epistemically central,
4) the notion of truth is "radically non-epistemic" and should be replaced by the concept of approximate truth,
5) experimental evidence has a theoretical nature, and
6) scientific knowledge is a subset of general knowledge.

Unfortunately, however, there are three closely related features of methodological realism that are not consistent with falsificationism. Perhaps the most significant of these features is Boyd's use of the notion of approximate truth. It will have occurred to the careful reader that Boyd's use of this notion differs in an important way from Popper's. Recall that, according to Popper, we are only justified in using the notion of approximate truth in a relative or comparative way; Popper refuses to concede absolute approximate truth to any theory however highly corroborated it may be. Boyd, however, *is* willing to grant *absolute* approximate truth to at least some theories and theoretical traditions (i.e., those of the mature natural sciences). Boyd uses the idea of our possession of relevantly (*absolutely*) approximately true theories to support our critical testing of theories (i.e., falsificationism). It is important at this point to understand what 'absolute approximate truth' means for Boyd. One might think that its meaning would require that our actual theoretical traditions assign theoretical entities to the correct real metaphysical categories. But this interpretation would not be fair to Boyd. Instead, Boyd seems to maintain that our actual scientific theories are approximately true in an absolute sense in that they are correct descriptions of real causes, even if such causes are unobservable and even if our theories assign those causes to the wrong metaphysical categories. While this interpretation of the meaning of 'absolute approximate truth' weakens Boyd's

metaphysical commitments, his extended use of the notion of approximate truth still appears unwarranted on strictly falsificationist grounds.

This difficulty is intimately connected to a second feature of methodological realism that is clearly not consistent with falsificationism. Recall that in his discussion of whether there are a priori standards to distinguish knowledge from non-knowledge, Boyd speaks of inductive strategies. Boyd thus accepts the idea that inductive strategies are employed in scientific inquiry. But this patently runs afoul of falsificationism, according to which only deduction is legitimate. Furthermore, recall the following question which served to motivate Boyd's development of methodological realism: "… why are theory-dependent standards for assessing projectability [of predicates] and degrees of confirmation [of theories] instrumentally reliable?" (Boyd 1984, p. 58). Here, Boyd admits that theories can be, and are, confirmed. Again, this runs completely counter to Popper's own flat-out rejection of confirmation.

There is, however, an even more fundamental problem related to Boyd's inductivism. His argument for scientific realism is an abductive one in which he explains the success of science by claiming that we possess approximately true scientific theories. Such an abductive argument, though, is ampliative and thus would be thoroughly unacceptable to a falsificationist. And, in fact, Popper explicitly states the stricture against abductive arguments for scientific realism as follows: "… *no theory of knowledge should attempt to explain why we are successful in our attempts to explain things*" (Popper 1974, p. 1027).

Still another inconsistency between Boyd's realism and falsificationism is related to the notion of approximate truth. In order to express this inconsistency, I need to repeat the following statement from Boyd: "Truth (or, rather, *exact* truth) is disconnected from our rational methods in a way in which reference and approximate truth are not" (Boyd 1981, p. 660). Now the problem here is that Boyd does not believe that *approximate* truth "is disconnected from our rational methods." Thus, Boyd regards approximate truth as an epistemic concept. Popper, on the other hand, explicitly states that verisimilitude or approximate truth is, like absolute truth, *not* an epistemic idea (Popper 1965, p. 234).

A final inconsistency between methodological realism and falsificationism has to do with explanations and explanatory structures. Although Boyd, like Popper, argues against the necessity of absolute or ultimate natural kinds or essences,[22] Boyd argues for the continuity of explanations or explanatory structures across theory change. He says, "In general, classification of sensible things on the basis of knowledge of their unobservable properties *is* a prerequisite for successful and sophisticated inductive generalization regarding their observable properties" (Boyd 1981, p. 642). Now, since Boyd thinks that methodological realism integrates the explanatory view of essences with the view of essences as required for successful inductive strategies (Boyd 1981, p. 640), his statement means that successful explanation requires "knowledge of determining micro-structural properties of matter" (Boyd 1981, p. 642). This means that when an older, relevantly approximately true theory is replaced by a newer one, the explanatory structure of the older theory must be retained in the new theory. According to Popper's falsificationism, however, such retention is not required, provided that the new theory accounts for all the observational data accounted for by the older theory. That is, since falsificationism does not admit the absolute approximate truth of any theory, there is no compelling reason to retain theoretical explanatory structures across theory change.

Alas, despite having many features that fit well with falsificationism, methodological realism cannot, in the end, be persuasively combined with it. Boyd's argument for scientific realism works *only if* our theories are relevantly approximately true. But Boyd does not actually offer any separate arguments to show that they are, in fact, true. And a falsificationist like Popper would, in any case, not accept any inductive argument to that effect.

But is there some non-inductive way to demonstrate the absolute approximate truth of a scientific theory? What we seem to need is some other, radically different way—a non-theoretical way—of demonstrating this.

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[22] See Boyd (1981, pp. 642–43) and Boyd (1984, p. 77).

IV. Hacking's Entity Realism

It is time now, therefore, to consider our third promising candidate form of realism: Hacking's entity realism. Hacking's approach to realism may be just what we need, since his focus is not on theories but on experimentation. Before describing Hacking's form of realism, however, I will first show how he critiques the sort of theory-based approach to realism characteristic of all the views that I have considered thus far. An understanding of this critique of realism about theories is necessary to fully appreciate the significance of Hacking's own, radically different, entity approach. As before, I will conclude this section with an analysis of the relationship between Hacking's entity realism and falsificationism, and an assessment of whether, and, if so, to what extent entity realism alleviates the tension between falsificationism and realism.

A. Hacking's Critique of Theory-Based Realism

As I mentioned before when discussing my use of Leplin's classification scheme, one of the chief targets of Hacking's argument against theory-based defenses of scientific realism is Putnam's internal realism. I think it will be instructive to first examine this example before considering Hacking's more general argument against all such versions of realism about theories.

Hacking maintains that there are three major problems with Putnam's internal realism. First, Hacking suggests that internal realism is probably irrelevant to the debate over *scientific* realism (Hacking 1983, p. 92). This is because Hacking regards internal realism as a form of transcendental nominalism and, thus, as a component of a more general anti-realist position (Hacking 1983, p. 108). On a related note, Hacking also thinks that Putnam, due to his transcendentalism, is, like Kant, too conservative in supposing that there is no way out of our conceptual schemes (Hacking 1983, p. 111). Finally, Hacking maintains that several difficulties arise from Putnam's use (or, rather, misuse) of the premises of the Löwenheim-Skolem theorem (Hacking 1983, pp. 105–7).

Let us examine these difficulties in some detail. First, according to Hacking, since the Löwenheim-Skolem theorem is only applicable to systems of statements that are formalizable in the first-order predicate calculus, and since the statements of science have not been shown to be thus formalizable, the Löwenheim-Skolem theorem is not applicable to the language of science. Second, Hacking contends that the same sort of argument can be used to show that the Löwenheim-Skolem theorem is also not applicable to ordinary English. Third, Hacking points out that commonly used indexicals, or context dependent utterances such as 'this' and 'here', are also not expressible in first-order logic (Hacking 1983, p. 105). Fourth, Hacking claims that since "The Löwenheim-Skolem theorem is non-constructive …, there is in principle no humanly accessible way to generate an unintended interpretation" (Hacking 1983, p. 107). Fifth, according to Hacking, there is the very real problem of nominal adjectives such as 'Bing' and 'Persian'. These are adjectives used to precisely name certain kinds of things and which serve as modifiers only of the names of those sorts of things and no others. For example, 'Bing' modifies 'cherry', but it does not modify the name of any other kind of fruit. Thus, in actual real-world classification schemes, the number of kinds of thing (say, cherries) will often not be equal to the number of kinds of some other thing (say, cats). But this means that the kind of reinterpretation which Putnam thinks necessitates internal realism will not even be possible (Hacking 1983, p 107).

There are, according to Hacking, three further difficulties with Putnam's use of the Löwenheim-Skolem theorem that reveal more clearly the fundamental problem with realism about theories. Drawing upon his interpretation of Wittgenstein, Hacking points out that the Löwenheim-Skolem theorem is not applicable in cases where language is more than talking and inscribing, i.e., in cases, unlike mathematics, where language is embedded in action in the world (Hacking 1983, p. 105). Similarly, Hacking claims that in a very fundamental way the Löwenheim-Skolem theorem does not apply to science because science is not just a set of sentences; it is also 'doings', i.e., experimental manipulations (Hacking 1983, pp. 106–7). Finally, Hacking objects to one of Putnam's arguments for internal realism, according to which even if we had a general theory of

reference, this theory too could be reinterpreted (Hacking 1983, pp. 105–6). Again citing a possible inspiration from Wittgenstein, Hacking claims that a general theory of reference may well be impossible. In any case, Hacking contends that we "do not need a theory of reference in order to refer" (Hacking 1983, p. 106).

According to Hacking, the main difficulty with Putnam's argument for realism is that it focuses entirely on theory. Although Hacking thinks that Putnam is correct in asserting that terms, including theoretical terms, do not have their reference established by theories,[23] this is not because such a thing is necessarily impossible but rather because it is not even necessary. As Hacking puts it, Putnam goes wrong because he does not recognize that "(a) assuring reference is not primarily a matter of uttering truths, but of interacting with the world, and that (b) even at the level of language there is vastly more structure than Putnam discusses, be it deep questions about the language of mathematical physics, or trivial observations about Bing cherries" (Hacking 1983, pp. 107–8).

But for Hacking the problem with Putnam's approach to realism is typical of *all* theory-based defenses of scientific realism. According to Hacking, all such defenses fail because they focus entirely on knowledge as representation. As he explains, "To attempt to argue for scientific realism at the level of theory, testing, explanation, predictive success, convergence of theories, and so forth is to be locked into a world of representations" (Hacking 1983, pp. 273–74). Consequently, scientific anti-realism remains a viable position. What we need instead is a theory of knowledge that regards knowers as more than mere representers or spectators but also as interveners or actors (Hacking 1983, p. 274).

Hacking creates his own fanciful philosophical anthropology to explain why this question of scientific realism ever arose in the first place. On this view, it is the fundamental nature of humans to make representations, including language (Hacking

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[23] See Hacking (1983, p. 38 and chapter 6). Although these two passages refer to the earlier, realist Putnam and his meaning of 'meaning,' I think they apply equally well to Putnam's internal realism. According to internal realism as well, the reference of individual terms is not fixed by a theory. Instead, reference can only be fixed by an interpretation, generally by the interpretation embodied in our actual use of the theory.

1983, p. 132). Yet, "Language is not for practical affairs" (Hacking 1983, p. 135). But if language is not needed for practical affairs, why was it invented? According to Hacking's anthropological fantasy, "people invented language out of boredom. Once we had fire, we had nothing to do to pass away the long evenings, so we started telling jokes" (Hacking 1983, p. 135). Thus, "The first peculiarly human invention is representation" (Hacking 1983, p. 136). Hacking contends that this invention led, however, to yet another invention: the concept of reality. He explains that "Once there is a practice of representing, a second-order concept follows in train. This is the concept of reality, a concept which has content only when there are first-order representations" (Hacking 1983, p. 136). The problem arises when we have more than one representation of the same thing (Hacking 1983, p. 144). How do we know which representation is actually like the thing represented? How do we know which representation is real? The question of scientific realism arises because we have developed *competing* representations (Hacking 1983, p. 139). Now Hacking concludes that, since the notion of representation has itself led to the debate over scientific realism, it does not make sense to attempt to defend scientific realism primarily by arguing for the truth of any scientific representations or theories.

B. Description of Entity Realism

Instead, Hacking proposes a much different approach to defending scientific realism based on considerations of action, experiment, and entities. This does not mean, however, that he thinks that appeals to representation and theory should play no role in defending scientific realism, but only that they do not play a primary role. He explains that he rejects "the false dichotomy between acting and thinking" (Hacking 1983, p. 130). He contends, however, that "The harm comes from a single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment. That is why … I study experimental science, and find in it the sure basis of an uncontentious realism" (Hacking 1983, p. 131).

Hacking believes that by examining experimentation and the sorts of manipulations and interventions that occur in it, we obtain our best support for scientific realism. Basically, his entity realism consists of three chief elements: experiments, actions, and theoretical entities. By 'theoretical entity', Hacking just means "all that ragbag of stuff postulated by theories but which we cannot observe. That means among other things, particles, fields, processes, structures, states and the like" (Hacking 1983, p. 26). Entity realism says, in a nutshell, that if we examine experimentation rather than theorizing and look closely at the kind of actions that actually take place within experiments, we see that experimenters routinely and reliably manipulate certain 'theoretical' entities. Such experimental use of theoretical entities gives us ample reason to believe that they are real. Thus, not surprisingly given its name, Hacking's version of scientific realism emphasizes realism about entities.

He explains that he arrived at this unconventional view during a discussion with a friend concerning the specifics of an experiment to detect quarks. A crucial part of the experiment involves being able to change the charge on a niobium ball. This change is achieved by spraying the ball with positrons or electrons to increase or decrease the charge, respectively (Hacking 1983, pp. 22–23). Now it struck Hacking as significant that one so-called theoretical entity, the electron, could be used in the course of an experiment to test for the existence of another theoretical entity. He says, "From that day forth I've been a scientific realist. *So far as I'm concerned, if you can spray them then they are real*" (Hacking 1983, p. 23). He continues, "*What convinced me of realism has nothing to do with quarks.* It was the fact that *by now* there are *standard* emitters with which we can spray positrons and electrons—and that is precisely what we do with them. *We understand the effects, we understand the causes, and we use these to find out something else.* The same of course goes for all sorts of other tools of the trade, the devices for getting the circuit on the supercooled niobium ball and almost endless *manipulations of the 'theoretical'*" (Hacking 1983, p. 24; italics added).

I have added italics to particular words, phrases, and sentences in the preceding passage because I think that they reveal four important ideas in Hacking's entity realism.

Let me begin by discussing the first three of these ideas; I will discuss the fourth in greater detail in the following paragraph. One thing that is clear from this passage is that Hacking does not intend for entity realism to be a global realism about *all* theoretical entities. He says that he was convinced of realism about positrons and electrons but not about quarks. Thus, we consider real only those theoretical entities which we can manipulate to achieve certain desired effects. Another idea conveyed by this passage is expressed in the words "by now". Here Hacking suggests that our knowledge of the reality of a given theoretical entity is a temporal matter—i.e., knowing that a theoretical entity is real depends upon the available body of theoretical and experimental knowledge at a particular time (see Hacking 1983, pp. 271–75). He explains that although it was reasonable to doubt the existence of electrons when they were merely postulated to provide successful explanations, it is unreasonable to doubt their existence now that they can be effectively used (Hacking 1983, pp. 271–72). A third idea expressed in the passage concerns the phrase "manipulations of the 'theoretical'". Hacking places the word "theoretical" in single quotes here because he wants to emphasize that once a theoretical entity can be reliably manipulated, it ceases to be theoretical. Instead, it is as real as, say, a wrench, or any other tool we use in everyday life.

The fourth and final idea expressed in the above passage has to do with the word "standard" and the sentence "We understand the effects, we understand the causes, and we use these to find out something else." Hacking is here suggesting that our ability to manipulate an entity in a reliable way (i.e., via some *standard* procedures or equipment) means that we understand that entity as a causal agent, or, better, that we understand the causal properties of that entity. In such cases, we have real, practical, *non-theoretical* knowledge of theoretical entities *as* causes. Now this is, I believe, the principal idea of entity realism. Hacking explicitly emphasizes this idea in several places when discussing the example of the electron. He says, for instance, *"We are completely convinced of the reality of electrons when we regularly set out to build—and often enough succeed in building—new kinds of device that use various well-understood causal properties of electrons to interfere in other more hypothetical parts of nature"* (Hacking 1983, p. 265).

Elsewhere, he states, "There is … a family of causal properties in terms of which gifted experimenters describe and deploy electrons in order to investigate something else, for example weak neutral currents and neutral bosons" (Hacking 1983, p. 272). And this idea also finds expression in the following: "The 'direct' proof of electrons and the like is our ability to manipulate them using well-understood low-level causal properties" (Hacking 1983, p. 274).[24]

In the context of this powerful idea, Hacking's more general statements about scientific realism become quite plausible. We can, for example, readily understand why he is prepared to characterize scientific realism as the assertion "that the entities, states and processes described by correct theories really do exist" (Hacking 1983, p. 21). Similarly, we can understand why he thinks "that reality has more to do with what we do in the world than with what we think about it" (Hacking 1983, p. 17). As he puts it, "Hence, engineering, not theorizing, is the best proof of scientific realism about entities" (Hacking 1983, p. 274).

C. The Relationship between Entity Realism and Falsificationism

It is time now to discuss how all of this relates to Popperian falsificationism. Admittedly, at first glance, Hacking's form of realism does not appear to be readily compatible with falsificationism. Yet, as I will point out, there are some important similarities between the two positions. Rather than discussing such similarities and dissimilarities separately—as I have done when treating the realisms of Putnam and Boyd—I will here adopt a topical approach instead. That is, I will simply take up certain topics, which are addressed by entity realism and which bear on its relationship with falsificationism, and indicate where Hacking's treatment of these topics is or is not consistent with falsificationism. This approach is preferable because, as I will show, with regard to some topics, Hacking's views both agree and disagree with falsificationism.

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[24] Further emphasis on this idea can be found in Hacking's treatment of Nancy Cartwright's causalism. See Hacking (1983, p. 36).

This combination of agreement and disagreement may be found in Hacking's attitude toward scientific theories. While Hacking certainly emphasizes realism about entities, what exactly is his view concerning realism about theories? In order to answer this question, let us look first at his informal definition of theory realism. He asserts, "*Realism about theories* says that scientific theories are either true or false independent of what we know: science at least aims at the truth, and the truth is how the world is" (Hacking 1983, p. 27). Notice that if Hacking *were* a realist about theories, this definition would commit him only to the view that scientific theories *have* truth-values which are dependent upon the way the world is; the definition would *not* commit him to a belief in the truth of any of our actual scientific theories. But this view of scientific realism is just the sort of weak metaphysical realist view which we find in Popper and which, as I have demonstrated, is unproblematic for falsificationism.

But Hacking, in fact, is not a realist about theories. To understand why he is not, we must look closely at his discussion of W. Newton-Smith's analysis of scientific realism. Hacking notes that, according to Newton-Smith, scientific realism has three ingredients:

> 1 An *ontological* ingredient: scientific theories are either true or false, and that which a given theory is, is in virtue of how the world is.
> 2 A *causal* ingredient: if a theory is true, the theoretical terms of the theory denote theoretical entities which are causally responsible for the observable phenomena.
> 3 An *epistemological* ingredient: we can have warranted belief in theories or in entities (at least in principle). (Hacking 1983, p. 28)

Now Hacking defines theory realism as follows: "My realism about theories is, then, roughly (1) and (3) …" (Hacking 1983, p. 29). Thus, Hacking's theory realism roughly has two ingredients. Now the first ingredient is not at all surprising; it is just the metaphysical realist view once again. At this point there is no conflict with falsificationism. The difficulty arises with the second ingredient, the epistemological one, for this would require Hacking to accept that at least some theories are true. But, like Popper, Hacking is not prepared to accept that conclusion.

Hacking's refusal to ascribe truth to scientific theories is evident in a number of statements. For example, he says, "… we have good reason to suppose that electrons exist, although no full-fledged description of electrons has any likelihood of being true. Our theories are constantly revised; for different purposes we use different and incompatible models of electrons which one does not think are literally true, but there are electrons, nonetheless" (Hacking 1983, p. 27). In addition, he states, "There is an important experimental contrast between realism about entities and realism about theories. Suppose we say that the latter is belief that science aims at true theories. Few experimenters will deny that. Only philosophers doubt it. Aiming at the truth is, however, something about the indefinite future. Aiming a beam of electrons is using present electrons. … There is in contrast no present set of theories that one has to believe in" (Hacking 1983, p. 263). Thus, in agreement with Popper, Hacking's view of theories entails that they aim at truth but not that any *are* actually true.

This is, though, not the end of the story with regard to Hacking's treatment of theory. He also has a good deal to say about the relationship between theory and experiment. Here, too, there are both similarities and dissimilarities with falsificationism. Hacking expresses a similarity when he says, "Science is said to have two aims: theory and experiment. Theories try to say how the world is. Experiment and subsequent technology change the world. We represent and we intervene. We represent in order to intervene, and we intervene in the light of representations" (Hacking 1983, p. 31). What is significant here is that Hacking says that theories are necessary for us to be able to intervene. Hacking expresses this idea even more forcefully when he says, "Thus, I make no claim that experimental work could exist independently of theory. That would be the blind work of those whom Bacon mocked as 'mere empirics'" (Hacking 1983, p. 158).

Yet Hacking rejects the Popperian view that all experiments are tests of theories (Hacking 1983, pp. 154–57). Contra Popper, Hacking asserts that although "you must have some ideas about nature and your apparatus before you conduct an experiment," you need not be "testing a theory about the phenomena under scrutiny" (Hacking 1983, pp. 153–54). Hacking thinks that the relationship between theory and experiment is far more

complex. As he puts it, "Some profound experimental work is generated entirely by theory. Some great theories spring from pre-theoretical experiment. Some theories languish for lack of mesh with the real world, while some experimental phenomena sit idle for lack of theory" (Hacking 1983, p. 159).

Another important topic which is featured in entity realism and which bears on its relationship to falsificationism is causality. Both Hacking and Popper emphasize the search for causes, and in the work of both authors, the notion of causality is related to that of explanation. It will therefore be necessary to discuss these two notions together. Popper asserts "that it is the aim of science to find *satisfactory explanations,* of whatever strikes us as being in need of explanation" (Popper 1972, p. 191). And he connects explanation with causes in the following manner: "Given some conjectured regularity and some initial conditions which permit us to deduce predictions from our conjecture, we can call the conditions the (conjectured) cause and the predicted event the (conjectured) effect. And the conjecture which links them by logical necessity is the long-searched-for (conjectured) necessary link between cause and effect. (The whole can be called a 'causal explanation' …" (Popper 1972, p. 91). And Popper thinks that the direction of scientific explanation is generally from the known to the unknown (Popper 1972, p. 191). That is, we explain some well known event by offering a conjecture about the "structural properties of nature" (i.e., about hidden causes) (Popper 1972, p. 196).

Thus, for Popper, our knowledge of causes is embedded in theoretical explanations. He says, "To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws,* together with certain singular statements, the *initial conditions*" (Popper 1968, p. 59). Now it is important to realize that "It is the theory or the [universal] law which constitutes the *logical link* between cause and effect …" (Popper 1972, p. 352). Thus, on Popper's account, causes and effects have to be linked by *universal* laws.

Popper claims that, strictly speaking, initial conditions, rather than any conjectured entities, are the cause of an event. Any conjectured entities or deep structures merely link one set of conditions (initial) with another (the effect). Thus,

causation is reduced to a regularity existing between one set of conditions and another. This is, of course, consistent with Popper's nomic realism. Yet, as I indicated in my discussion of Popper's realism, Popper also claims to be a realist about theoretical entities. Thus, he would have to admit that not only are the laws linking causes and effects real, so also are the theoretical entities contained in those laws.

Although agreeing with Popper about the importance in science of seeking causes, Hacking rejects Popper's account. Popper's account of causality is actually similar to Newton-Smith's causal ingredient of scientific realism. Recall that the causal ingredient says that "… if a theory is true, the theoretical terms of the theory denote theoretical entities which are causally responsible for the observable phenomena." But this "implies that belief in such entities depends on belief in a theory in which they are embedded" (Hacking 1983, p. 28). Contrary to Popper and Newton-Smith, Hacking thinks that knowledge of causes need not be parasitic upon knowledge of theories or universal laws.

Ironically, Hacking picks up a very different strand in Popper's thought to lend support to his own radically different account of causality. According to Popper, "… the entities which we conjecture to be real should be able to exert a causal effect upon the *prima facie* real things; that is, upon material things of an ordinary size: that we can explain changes in the ordinary material world of things by the causal effects of entities conjectured to be real" (quoted in Hacking 1983, p. 146). Hacking thinks that here "… Popper points in the right direction. Reality has to do with causation and our notions of reality are formed from our abilities to change the world" (Hacking 1983, p. 146). Thus, according to Hacking, "… one can believe in some entities without believing in any particular theory in which they are embedded. One can even hold that no general deep theory about the entities could possibly be true, for there is no such truth" (Hacking 1983, p. 28). For Hacking, then, causal explanation does not require universal laws; we can instead have individual low-level causal explanations. He says, for example, that we are convinced of the reality of structures seen under the microscope "not by a high powered deductive theory about the cell—there is none—but because of a large number of interlocking low level generalizations that enable us to control and create phenomena in

the microscope. In short, we learn to move around in the microscopic world" (Hacking 1983, p. 209). Hacking concludes that we do not need some true general theory about an entity in order to provide a causal explanation involving that entity. If asked, for instance, to explain why the charge on a niobium ball changes when sprayed by electrons, Hacking would simply say that electrons do, *in fact,* change the charge under appropriate conditions. No deeper explanation is required. We know that one of the causal properties of electrons is that they alter the charge on anything to which they adhere, and we know this specific causal property because of experimentation, not theory. Our knowledge of causes is fully captured by hard-won, low-level experimental generalizations of this sort.

As is clear from these last remarks, Hacking does not think that explanation is really that important. He says, "… explanation may play a less central a role in scientific reasoning than some philosophers imagine" (Hacking 1983, p. 53). He also says, "There are times when we feel a great gain in understanding by the organization of new explanatory hypotheses. But that feeling is not a ground for supposing that the hypothesis is true. … the ground for believing the theory is its predictive success, and so forth, not its explanatory power" (Hacking 1983, p. 53). Now, although Popper does not think we really have *any* grounds for believing a theory, he would have to agree with Hacking that what is most important from a falsificationist standpoint is a theory's predictive success, i.e., how well it has withstood severe tests.

There is a final point of agreement between Hacking and Popper regarding explanation, which has to do with explanations of the success of science in general. Various abductive arguments, or inferences to the best explanation, can be offered to account for the success of science, and, thus, to argue for scientific realism. Hacking, however, is sceptical of all such arguments and, in fact, of *all* abductive arguments (Hacking 1983, pp. 52–53). As we have seen in our discussion of Boyd, support for such scepticism can be drawn from Popper. And, in fact, Hacking does solicit this support, for he asserts, "Popper, I think, is a wiser self-professed realist than most when he writes that it never makes sense to ask for the explanation of our success" (Hacking 1983, p. 57). Thus, Hacking and Popper both reject abductive arguments for scientific realism.

Entity realism and falsificationism do not agree, however, with respect to what Hacking calls "the creation of phenomena." According to Hacking, we not only discover phenomena, we also create them. He says, "Traditionally scientists are said to explain phenomena that they discover in nature. I say that often they create the phenomena which then become the centrepieces of theory" (Hacking 1983, p. 220). And Hacking thinks that "… the creation of phenomena more strongly favours a hard-headed scientific realism" (Hacking 1983, p. 220). But there is nothing in Popper's description of falsificationism or realism about creating phenomena.

Finally, there seems to be a fundamental disagreement between entity realism and falsificationism concerning induction. Hacking's argument for scientific realism apparently requires induction. According to entity realism, we justify our low-level empirical generalizations on the basis of a limited number of interventions. To the extent that entity realism makes our knowledge of causes depend on such justification, it clearly violates falsificationism's proscription of induction. I believe, however, that entity realism can be reframed in such a way that knowledge of causes would not require inductive justification. I will have more to say about this in the next chapter.

Let me now summarize the major points of agreement and disagreement between Hacking's entity realism and Popper's falsificationism. I begin with the points of agreement. First, there is good evidence that entity realism, like falsificationism, entails only that theories aim at truth, not that they *are* true. Second, although Hacking emphasizes experimentation, he also admits the value of theory. Third, both Hacking and Popper emphasize the search for causes. Fourth, both reject abduction. Now, as I have demonstrated, the two philosophers also *disagree* over the following: 1) whether all experiments are tests of theories, 2) the nature of causality and explanation, 3) the creation of phenomena, and 4) whether induction is necessary. Thus, although entity realism and falsificationism appear to be consistent in some respects, there are also significant inconsistencies between them which would have to be remedied before any truly compatible synthesis of these two positions could be achieved.

Chapter 3: A Possible Solution

It is clear from my analyses of three forms of realism in the preceding chapter that none of them quite succeeds in being truly compatible with Popper's falsificationism. All three forms of realism fall short of compatibility at particular points. We are in a position now to see what can be learned from these failures, and what progress, if any, may have been made towards a solution of the problem described in chapter one. How far have we gotten towards achieving a convincing conjunction of falsificationism and realism? Is there a solution to this problem? And, if there is, what form of realism does it require? In this chapter, I take stock of my analyses of the three versions of realism discussed in chapter two, and I draw upon those analyses in order to propose a possible solution. Since, as I stated in chapter one, I am not interested here in criticizing falsificationism, my solution focuses on realism instead. More specifically, my solution consists in the elaboration of an alternative form of realism, one that incorporates elements of Hacking's entity realism and Boyd's methodological realism. I do not claim that my proposed solution is without difficulties of its own. Where such difficulties arise, they will be noted as places for further research and investigation.

I. Assessment of Results

Let us now see what can be learned from the results of my analyses in chapter two. Why do the versions of realism put forth by Putnam, Boyd, and Hacking fail to be compatible with falsificationism? To be more precise, where does each form of realism run afoul of falsificationism? Well, Putnam's internal realism fails because it dispenses with the very notions of approximate truth and absolute truth that give falsificationism its point. Falsificationism simply does not make sense without the idea that we strive for closer and closer approximations to absolute truth. Boyd's methodological realism, on the other hand, fails primarily for just the opposite reason: it allows us to ascribe *absolute* approximate truth, rather than relative or comparative approximate truth, to theories. But such an ascription is specifically prohibited by the scepticism inherent in Popper's

falsificationism. Boyd's methodological realism also fails because it argues for scientific realism via abduction, a logical method not countenanced by falsificationism. Finally, Hacking's entity realism fails because it rejects the falsificationist view that all experiments are tests of theories and because it apparently requires the use of induction. The former problem, though, is really just due to a quirk on Popper's part. There is no legitimate reason why a falsificationist would have to insist that *all* experiments be tests of theories. Instead, he could readily concede that some experiments were not, while still maintaining that the primary role of experiments is to test theories. The latter problem— i.e., the one having to do with induction—is admittedly more serious, but there may be a way around it, which I discuss in laying out my proposed solution. If this problem could be overcome, then the falsificationist picture of science could be contained as a component in the more comprehensive picture presented by Hacking's entity realism.

Now if we look closely at this summary of reasons for failure, we can discern certain common criteria for any solution to the problem of persuasively combining falsificationism with realism. Following the diagnosis of Putnam's failure, the first criterion for any solution is that it must be able to accommodate approximate truth and absolute truth. From Boyd's failure we can discern a second criterion, which is that any solution must *not* grant *absolute* approximate truth to theories. A third criterion for any solution is that it must not rely on abduction. We may also add a fourth criterion, which is just the rather obvious point that any solution to this problem cannot employ induction. Thus, any version of realism put forth as a solution to this problem must meet these criteria.

Before developing my version of realism, I would first like to point out a peculiar feature of the problem addressed here. I have been using Popper's philosophy of science as a way of exploring the general tension between falsificationism and realism. Now, given the way that falsificationism and realism are formulated by Popper, there is no way, strictly speaking, of successfully combining the two positions. That is, strictly speaking, there is no solution to the problem of eliminating the tension between falsificationism and realism. This is due to the way that Popper has, perhaps unknowingly, set up the

problem. According to Popper, scientific theories are universal statements. Moreover, induction is an invalid form of argument. Thus, scientific theories can never be proven or shown to be true; they can only be falsified. Yet, Popper claims also to be a realist about scientific theories. Now, if being a realist about theories consists merely in the metaphysical realist belief that theories aim at the truth or that theories have truth-values, then there is no real problem here at all. But then, as discussed in chapter one, this is a very weak and unconvincing form of realism. We desire a much stronger version of realism about theories that says that our scientific theories *are* true or, at least, approximately true. If, however, being a realist about theories takes the form of this stronger version, then there is apparently *no* way to be both a falsificationist *and* a realist about theories in Hacking's sense. What this means is that any form of realism offered as a solution to this problem must meet an additional criterion: it must not be a realism about theories.

II. Proposed Solution

Might there be a way of making falsificationism compatible with a strong version of realism by adopting entity realism instead? I suggest that there is such a way, one which incorporates important features of both Boyd's methodological realism and Hacking's entity realism. My suggestion should not be surprising, since my analyses of these two forms of realism revealed a number of broad areas of agreement with falsificationism. I draw upon these areas in developing my proposed solution. Specifically, I draw upon those features of methodological realism and entity realism that actually explain why falsificationism works.

In describing my solution, I must first trace the steps leading to its creation. This tracing should, in turn, reveal the structure of the argument in support of my proposal. I begin with Boyd. Boyd's form of realism is consistent with falsificationism in a number of ways. For instance, both falsificationism and methodological realism embody a trial and error approach; both also portray knowledge as dynamic; and both assert knowledge of causal factors. Furthermore, Boyd's methodological realism fleshes out Popper's

account of falsificationism by showing how theoretical considerations actually play crucial roles both in the acceptance of basic statements and in the possibility of rigorous testing. In fact, Boyd's methodological realism can be seen as employing the notion of the approximate truth of scientific theories to support the sort of theoretical, deductive approach found in falsificationism.

Unfortunately, as we have seen, Boyd tries to prove too much with his abductive argument for scientific realism. He tries to prove that our actual scientific theories possess absolute approximate truth. This move, however, is ruled out explicitly by Popper and by our criterion that forbids realism about theories as a possible solution.

What Boyd should have tried to prove instead was just that our actual theoretical tradition has allowed us to have knowledge of real theoretical entities or underlying causes. In short, Boyd should have argued merely for entity realism rather than theory realism. Of course, he actually argues for both. His main argument for entity realism is his causal and naturalistic epistemic access theory of reference. Recall that, according to this theory, there are complex causal interactions that bring it about that the real properties of a theoretical entity actually reliably regulate the usage of the term used to refer to that entity. Moreover, this process is supposed to exhibit the same kind of successive approximations characteristic of scientific theories. Now, we may reasonably suppose that this process would still operate even if the scientific theories describing these entities were not approximately true. That is, there is no reason to think that the accuracy of reference to theoretical entities is dependent upon the approximate truth of theories. Another argument for the reality of theoretical entities is expressed in Boyd's second methodological principle. According to this principle, theoretical criticism of procedures of measurement and detection can only be instrumentally reliable if the theories employed actually describe real entities. Finally, Boyd offers what I take to be a third argument for entity realism. This argument explains the reliability of instrumental scientific knowledge on the assumption that our theories describe the real, unobservable causal factors responsible for observable phenomena.

There are, though, two problems at this point. The first problem is that the latter two arguments are themselves abductive. Falsificationism, however, only accepts deductive arguments. Thus, a falsificationist cannot accept or use these two arguments to support entity realism. The second problem has to do with Boyd's epistemic access theory of reference. While it is an appealing account of reference, its truth is anything but certain. Thus, it is simply too slim and uncertain a basis upon which to build a strong argument for entity realism. This fact actually points to a more general difficulty with Boyd's argument for scientific realism: it is almost entirely based on abductive arguments. Boyd claims that *if* such and such features of scientific realism were true, then they would account for instrumental knowledge in science. These features of scientific realism are simply offered as the best explanations of instrumental knowledge. But, as Hacking points out (Hacking 1983, p. 53), the fact that such explanations satisfy our intellects is a very weak basis upon which to assert the truth of those explanations. Yet, Boyd's explanations, particularly those concerning the reality of theoretical entities, might still be true.

What we clearly need is some independent way of supporting Boyd's claim that we do, in fact, possess knowledge of theoretical entities. This is where Hacking comes in. Hacking presents compelling evidence, drawn from actual scientific experimentation, of knowledge of theoretical entities and their causal properties. Most importantly, I contend that his entity realism can be reinterpreted so that knowledge of such causes does not require inductive justification. This reinterpretation requires that we downplay the importance of low-level generalizations and the inferential moves required to generate them. We must instead focus on direct, non-inferential knowledge of causes as reflected in actual instances of intervening in the world. That is, since Hacking's entire approach to the question of realism is to move away from representation and theory, we must regard his argument for scientific realism as resting primarily not on inferential knowledge of causes (as represented in generalizations), but instead on non-inferential knowledge, the kind of knowledge one has each time he successfully uses such causes.

There is certainly a sense in which Hacking simply enumerates examples from experimental science in which so-called 'theoretical' entities and their causal properties are reliably used. In such instances, no argument, theory, or representation of any kind is required in order to know that a 'theoretical' entity or cause is real. An argument is no more needed for knowledge of a 'theoretical' cause than it is to know that the pain in my toe was caused by stubbing it against the chair. Thus, Hacking's way of supporting the claim that we have knowledge of theoretical entities and causes is in agreement with Boyd's assertion that there can be no philosophical "*analysis* of causation," only "an *assay* of causation, an account of the sorts of causal factors there are in the world and of how they interact" (Boyd 1981, p. 653).

Let us examine more closely what my reinterpretation of entity realism requires. On one reading of Hacking's realism, knowledge of causes is clearly inferential. From descriptions of particular cases involving a given cause, we infer a low-level generalization about the cause. We move from a statement or set of statements to another statement. As Wilfrid Sellars (1963, p. 329) points out, inferences like these are moves within a language game from one position to another; both the stimulus and the response are positions in the language game. In order to develop my reinterpretation of entity realism, however, we need an account of how the language game hooks up with the world. Fortunately, Sellars provides such an account. He claims that there is another kind of move which is represented by moves into and out of the language game. He calls the former "language entry transitions," and they occur whenever the stimulus for the linguistic response (i.e., an observation sentence) is not itself part of the language game. The latter, on the other hand, are dubbed by Sellars "language departure transitions" and occur whenever one moves from a linguistic stimulus (a command, for example) to a response which is not part of the language game (for instance, raising one's hand) (Sellars 1963, p. 329). Now what is significant here is that Sellars contends that these transitions are *not* moves in a language game. In particular, he claims that language entry transitions cannot be moves within a language game since the role of sensations "in bringing about the occupation of an observation sentence position is not that of a thought

serving as a premise in an inference" (Sellars 1963, p. 329). Following Sellars then we can reframe Hacking's entity realism so that statements about unobservable causes represent language entry transitions rather than inferences within a language game. This is crucial because it means that these transitions are non-inferential and thus non-inductive. On Hacking's account, then, our capacity to effect language entry and language departure transitions would be dependent upon our ability to successfully interact with, and thus know, unobservable causes. Thus, my reinterpretation of entity realism draws upon Sellars's work in a way that allows us to claim knowledge of unobservable causes without having to employ induction and, therefore, run afoul of falsificationism.

Support for this sort of direct knowledge of unobservable causes can be found in Hacking's own account of entity realism. In its focus on experimentation, Hacking's entity realism incorporates the claim that we can have pre-theoretical knowledge of causes. That is, according to Hacking, we can gain knowledge of causes via experimentation *before* we have any relevant theory whatsoever. Knowledge of causes can simply be contained in pre-theoretical observation statements, i.e., statements resulting from language entry transitions. This claim, however, certainly seems to run counter to Popper's whole conjectural approach. Yet, recall that Hacking cites Popper's statement that we should count as real those conjectured entities which have causal effects on everyday objects. Popper's statement seems to implicitly accept pre-theoretical knowledge of causes and, thus, to lend support to my proposal to combine falsificationism with my reinterpretation of entity realism. Further support for my proposal can be obtained from Popper's treatment of basic statements as conventional. The conventions we actually adopt could be based on pre-theoretical knowledge of causes. That is, the fact that observers *are* able to come to agreement over which statements to accept as basic could be explained by the observers' common possession of pre-theoretical knowledge of causes.

There may, however, still be a lingering worry that Hacking does, after all, use induction in some primary way to support his entity realism. This worry stems from

Hacking's own talk of low-level generalizations and low-level causal properties. Recall that Hacking asserts, "The 'direct' proof of electrons and the like is our ability to manipulate them using well-understood low-level causal properties" (Hacking 1983, p. 274). It is also helpful here to recall the following statement by Hacking concerning the question of the reality of cellular structures seen under a microscope: "We are convinced not by a high powered deductive theory about the cell—there is none—but because of a large number of interlocking low level generalizations that enable us to control and create phenomena in the microscope" (Hacking 1983, p. 209).

Do such generalizations require induction for their support? As it turns out, whether they are supported deductively or inductively does not really matter. On Hacking's view, it is really irrelevant *how* we justify these low-level generalizations; what is important is that we can use them. Our knowledge of the theoretical entities and causes described by such generalizations depends only on our ability to use the generalizations, not on the method of their derivation or justification. For example, we know that electrons are real because we can employ their causal properties as described in the low-level generalization, "Under appropriate experimental conditions, spraying electrons on a niobium ball decreases its charge." Our knowledge of the reality of electrons is *not* based on our ability to infer generalizations of this sort from repeated observations.

There is a connection here, I think, between Hacking's low-level generalizations and Popper's falsifying hypotheses. In my discussion of Popper's notion of corroboration, I claimed that corroboration played something of an instrumental role in falsificationism proper. According to Popper, the falsification of a highly universal theory requires the simultaneous corroboration of a low-level empirical hypothesis (i.e., a falsifying hypothesis) that describes some reproducible effect which is clearly in conflict with predictions deduced from the universal theory (Popper 1968, pp. 86–87). Now it is significant that Popper asserts that "The falsifying hypothesis can be of a *very low level of universality* (obtained, as it were, *by generalising the individual co-ordinates of a result of observation*" (Popper 1968, p. 87, n. 1; italics added). And he cites as an

instance of such a generalization Mach's statement that "… the velocity of the levelling out of temperature differences, provided these differences of temperature are small, is proportional to these differences themselves" (Popper 1968, p. 75). But "generalising the individual co-ordinates of a result of observation" to create this sort of generalization is nothing other than induction! Thus, here we have some, albeit slight, evidence that Popper accepts a limited role for induction. That is, he seems to grant it a role in the justification of low-level falsifying hypotheses even though he explicitly denies it any role in the justification of high-level universal theories. Moreover, in the light of Hacking's discussion of experimentation, I should point out that the process of observing and generalizing described here by Popper would not require any universal theory. What we have here then is a passage in Popper's seminal work that is consistent with Hacking's use of low-level generalizations to support our claims to have knowledge of theoretical entities, even if such generalizations are derived inductively.

Admittedly, though, Hacking's low-level generalizations and Popper's low-level falsifying hypotheses are not the same thing. Popper's falsifying hypotheses are themselves tested during an attempt to falsify a universal theory. Hacking's low-level generalizations, on the other hand, are not tested; they are used in the course of experiments. But Hacking's low-level generalizations actually help to explain how falsificationism is possible. In the course of attempting to falsify a theory, we have to make use of low-level generalizations about theoretical entities. For example, if we attempt to falsify a theory about quarks, we have to utilize low-level generalizations concerning the causal properties of other theoretical entities such as electrons. On a full-blooded realist reinterpretation, falsificationism would not be possible without being able to utilize the causal properties of real 'theoretical' entities.

This, though, is just the argument that Boyd makes via abduction. Of course, as we have seen, Boyd goes too far in arguing for the approximate truth of the theories which make up our theoretical tradition. In the light of Hacking's work, Boyd could, however, make a compelling argument that our theoretical tradition does embody knowledge of real 'theoretical' entities and that it is *this* knowledge that accounts for the

instrumental reliability of scientific methodology. Our possession of this knowledge, rather than of knowledge of theories, would be the key that allows us to successfully design the kinds of experiments that severely test theories. We would design experiments that employed entities and causes that we already at least partially understood. Furthermore, such knowledge of real 'theoretical' entities would allow us to choose which new theories to take seriously. Recall that, according to Boyd, we take seriously only those theories "which relatively closely resemble our existing theories in respect of their ontological commitments and the laws they contain" (Boyd 1981, p. 618). Now we must reject the latter requirement concerning laws. But we can preserve the former requirement if we reinterpret it in the light of Hacking's entity realism. That is, our ontological commitments will not be bound up in any particular theories but will instead be based on the kinds of entities we can successfully manipulate in experimentation. So we will choose to test only those theories which have some articulation with real 'theoretical' entities and causes about which we already have some knowledge. Thus, we can use Hacking's entity realism to support a limited or restricted version of Boyd's methodological realism. And we can do this in a way that is consistent with falsificationism.

Being able to retain some of Boyd's methodological realism is an important result because it allows us to preserve his understanding of scientific discovery as a dialectical process. As we have seen, the idea of a dialectical, trial-and-error approach of successive approximations to the truth is also a key element in falsificationism. But which of the dialectical components of methodological realism can we preserve? Well, we obviously cannot claim that there is a dialectical process with regard to theories, since we cannot be realists about theories. But what about methodology and language? I have already discussed in the preceding paragraph how our possession of knowledge of real 'theoretical' entities would provide the basis for a dialectical scientific methodology. That is, our knowledge of real entities would influence scientific methodology in a way that would expand our knowledge of those entities and allow us to gain knowledge of other entities. I think that the same sort of dialectical process would also still operate

with regard to scientific language. As I mentioned before, there is no reason to think that being able to refer successfully to theoretical entities is dependent upon any one particular theory or upon any theory which describes such entities actually being true. Moreover, Boyd presents an independent argument, his epistemic access theory of reference, to account for the dialectical nature of scientific language. Other causal theories of reference, such as that put forth by Putnam, might be elicited for support as well.

III. Conclusion

I have proposed that by abandoning theory realism for a hybrid form of entity realism we may be able to alleviate the tension between falsificationism and realism found in Popper's philosophy of science. This hybrid form of realism draws primarily on Hacking's entity realism but also incorporates key elements of Boyd's methodological realism. Most importantly, my proposed version of realism is a strong one. It claims more than that we strive for knowledge of theoretical entities; it claims that we already have such knowledge, at least with respect to entities in their capacity as causal agents. Moreover, my hybrid form of realism, like Boyd's methodological realism, also explains why a falsificationist methodology is successful in attaining instrumental knowledge.

As I stated in the beginning of this chapter, I do not claim that my solution of this problem is definitive or that it is without certain difficulties. Concerning the latter, I believe there is a major difficulty having to do with induction. I have argued that we should read Hacking as emphasizing direct, non-inferential knowledge of causes as reflected in particular experimental interventions. This kind of knowledge clearly would not require induction. But how would we use it in the process of falsifying theories? In order to apply such knowledge to new cases, wouldn't we need the sort of low-level empirical generalizations mentioned by Hacking? But on Hacking's rich account of experimental activity, these generalizations would usually have to be justified inductively. Even though, as I have pointed out, there is some slight evidence that

Popper would accept this sort of relatively harmless low-level induction, it seems better, from a strictly falsificationist standpoint, to avoid induction altogether.

There may be a way around this difficulty, however. Instead of treating low-level generalizations as inductively valid statements, we could instead regard them as instrumental conjectures or hypotheses. Although such conjectures themselves might never be tested, we would still be able to use them in the course of experiments; the conjectures just would not represent certain knowledge of causes. And this seems plausible according to falsificationism: non-conjectural knowledge of causes could not be reflected in any sort of generalization or universal statement, but only in particular observation statements. It remains to be seen, however, whether such a sceptical attitude toward low-level generalizations is compatible with actual, everyday experimental practice in mature scientific disciplines (i.e., those possessing well-developed theories). Does relying on these generalizations require knowing them to be justified or merely treating them as useful conjectures? If the former, then my proposed version of realism might have to substantially narrow the range of permitted experimentation: any experiment would have to be a test of some theory or other, and our use of low-level generalizations would have to be limited to those that had actually been tested (i.e., subjected to attempts at falsification). What effect would such limitations have on our stock of knowledge of theoretical entities and causes? It seems to me that more research into the history of experimental science is necessary in order to answer this question. We need to find out, for example, how experimenters actually regard low-level generalizations. Thus, determining whether or not my proposed solution is plausible is to some extent an empirical question.

There is another remaining difficulty, which is to spell out more clearly how exactly we are able to have direct, non-inferential knowledge of causes. At first blush, this idea sounds rather occult and suspicious. We certainly do not want to wind up back at a mysterious Platonic grasp of forms. But if we hold, with Popper and others, to a naturalistic, evolutionary view of knowledge in which there is continuity between animal knowledge and our own, then we must concede that the idea has some plausibility. In

order to make it more plausible, however, we need to learn more about non-propositional knowledge and its relationship to propositional knowledge. We also need a better understanding of how closely the survival of species is tied to knowledge of causes in the world. There are almost certainly other important questions pertaining to my proposed solution that would need to be answered as well. Nonetheless, I believe that in this thesis I have taken an important first step in the explication of a plausible conjunction of falsificationism and a hearty form of realism.

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Vita

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