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Larry Laudan,* “A Confutation of Convergent Realism”

Larry Laudan builds his case against “convergent realism” around traditional problems about the notion of reference (here the relation between theoretical terms of mature science and the world) and by appeal to the historical record, which suggests that many theories have been empirically successful without being true or securing reference for their terms.

The positive argument for realism is that it is the only philosophy that doesn't make the success of science a miracle.

H. Putnam (1975)

1. The Problem

It is becoming increasingly common to suggest that epistemological realism is an empirical hypothesis, grounded in, and to be authenticated by, its ability to explain the workings of science. A growing number of philosophers (including Boyd, Newton-Smith, Shimony, Putnam, Friedman and Niiniluoto) have argued that the theses of epistemic realism are open to empirical test. The suggestion that epistemological doctrines have much the same empirical status as the sciences is a welcome one: for, whether it stands up to detailed scrutiny or not, it marks a significant facing-up by the philosophical community to one of the most neglected (and most notorious) problems of philosophy: the status of epistemological claims.

But there are potential hazards as well as advantages associated with the “scientizing” of epistemology. Specifically, once one concedes that epistemic doctrines are to be tested in the court of experience, it is possible that one's favorite epistemic theories may be refuted rather than confirmed. It is

L. Laudan, “A Confutation of Convergent Realism,” *Philosophy of Science*, 1981, 48: 19–38, 45–9.

the thesis of this paper that precisely such a fate afflicts a form of realism advocated by those who have been in the vanguard of the move to show that realism is supported by an empirical study of the development of science. Specifically, I shall show that epistemic realism, at least in certain of its extant forms, is neither supported by, nor has it made sense of, much of the available historical evidence.

2. Convergent Realism

Like other philosophical *-isms*, the term “realism” covers a variety of sins. Many of these will not be at issue here. For instance, “semantic realism” (in brief, the claim that all theories have truth values and that some theories—we know not which—are true) is not in dispute. Nor shall I discuss what one might call “intentional realism” (i.e., the view that theories are generally intended by their proponents to assert the existence of entities corresponding to the terms in those theories). What I shall focus on instead are certain forms of *epistemological* realism. As Hilary Putnam has pointed out, although such realism has become increasingly fashionable, “very little is said about what realism *is*” (1978). The lack of specificity about what realism asserts makes it difficult to evaluate its claims, since many formulations are too vague and sketchy to get a grip on. At the same time, any efforts to formulate the realist position with greater precision lay the critic open to charges of attacking a straw man. In the course of this paper, I shall attribute several theses to the realists. Although there is probably no realist who subscribes to all of them, most of them have been defended by some self-avowed realist or other; taken together, they are perhaps closest to that version of realism advocated by Putnam, Boyd and Newton-Smith. Although I believe the views I shall be discussing can be legitimately attributed to certain contemporary philosophers (and will frequently cite the textual evidence for such attributions), it is not crucial to my case that such attributions can be made. Nor will I claim to do justice to the complex epistemologies of those whose work I will criticize. My aim, rather, is to explore certain epistemic claims which those who are realists might be tempted (and in some cases have been tempted) to embrace. If my arguments are sound, we will discover that some of the most intuitively tempting versions of realism prove to be chimeras.

The form of realism I shall discuss involves variants of the following claims:

- R1) Scientific theories (at least in the “mature” sciences) are typically approximately true and more recent theories are closer to the truth than older theories in the same domain;

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- R2) The observational and theoretical terms within the theories of a mature science genuinely refer (roughly, there are substances in the world that correspond to the ontologies presumed by our best theories);
- R3) Successive theories in any mature science will be such that they “preserve” the theoretical relations and the apparent referents of earlier theories (i.e., earlier theories will be “limiting cases” of later theories).¹
- R4) Acceptable new theories do and should explain why their predecessors were successful insofar as they were successful.

To these semantic, methodological and epistemic theses is conjoined an important meta-philosophical claim about how realism is to be evaluated and assessed. Specifically, it is maintained that:

- R5) Theses (R1)–(R4) entail that (“mature”) scientific theories should be successful; indeed, these theses constitute the best, if not the only, explanation for the success of science. The empirical success of science (in the sense of giving detailed explanations and accurate predictions) accordingly provides striking empirical confirmation for realism.

I shall call the position delineated by (R1) to (R5) *convergent epistemological realism*, or CER for short. Many recent proponents of CER maintain that (R1), (R2), (R3), and (R4) are empirical hypotheses which, via the linkages postulated in (R5), can be tested by an investigation of science itself. They propose two elaborate abductive arguments. The structure of the first, which is germane to (R1) and (R2), is something like this:

- I
 - 1. If scientific theories are approximately true, they will typically be empirically successful;
 - 2. If the central terms in scientific theories genuinely refer, those theories will generally be empirically successful;
 - 3. Scientific theories are empirically successful.
 - 4. (Probably) Theories are approximately true and their terms genuinely refer.

The argument relevant to (R3) is of slightly different form, specifically:

- II
 - 1. If the earlier theories in a “mature” science are approximately true and if the central terms of those theories genuinely refer, then later

- more successful theories in the same science will preserve the earlier theories as limiting cases;
2. Scientists seek to preserve earlier theories as limiting cases and generally succeed.
 3. (Probably) Earlier theories in a “mature” science are approximately true and genuinely referential.

Taking the success of present and past theories as givens, proponents of CER claim that *if* CER were true, it would follow that the success and the progressive success of science would be a matter of course. Equally, they allege that if CER were false, the success of science would be “miraculous” and without explanation.² Because (on their view) CER explains the fact that science is successful, the theses of CER are thereby confirmed by the success of science and non-realist epistemologies are discredited by the latter’s alleged inability to explain both the success of current theories and the progress which science historically exhibits.

As Putnam and certain others (e.g., Newton-Smith) see it, the fact that statements about reference (R2, R3) or about approximate truth (R1, R3) function in the explanation of a contingent state of affairs, establishes that “the notions of ‘truth’ and ‘reference’ have a causal explanatory role in epistemology” (Putnam 1978, p. 21).³ In one fell swoop, both epistemology and semantics are “naturalized” and, to top it all off, we get an explanation of the success of science into the bargain!

The central question before us is whether the realist’s assertions about the interrelations between truth, reference and success are sound. It will be the burden of this paper to raise doubts about both I and II. Specifically, I shall argue that *four* of the five premises of those abductions are either false or too ambiguous to be acceptable. I shall also seek to show that, even if the premises were true, they would not warrant the conclusions which realists draw from them. . . .

3. Reference and Success

The specifically referential side of the “empirical” argument for realism has been developed chiefly by Putnam, who talks explicitly of reference rather more than most realists. On the other hand, reference is usually implicitly smuggled in, since most realists subscribe to the (ultimately referential) thesis that “the world probably contains entities very like those postulated by our most successful theories.”

If (R2) is to fulfill Putnam’s ambition that reference can explain the success of science, and that the success of science establishes the presumptive truth of (R2), it seems he must subscribe to claims similar to these:

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- S1) The theories in the advanced or mature sciences are successful;
- S2) A theory whose central terms genuinely refer will be a successful theory;
- S3) If a theory is successful, we can reasonably infer that its central terms genuinely refer;
- S4) All the central terms in theories in the mature sciences do refer.

There are complex interconnections here. (S2) and (S4) explain (S1), while (S1) and (S3) provide the warrant for (S4). Reference explains success and success warrants a presumption of reference. The arguments are plausible, given the premises. But there is the rub, for with the possible exception of (S1), none of the premises is acceptable.

The first and toughest nut to crack involves getting clearer about the nature of that “success” which realists are concerned to explain. Although Putnam, Sellars and Boyd all take the success of certain sciences as a given, they say little about what this success amounts to. So far as I can see, they are working with a largely *pragmatic* notion to be cashed out in terms of a theory’s workability or applicability. On this account, we would say that a theory is successful if it makes substantially correct predictions, if it leads to efficacious interventions in the natural order, if it passes a battery of standard tests. One would like to be able to be more specific about what success amounts to, but the lack of a coherent theory of confirmation makes further specificity very difficult.

Moreover, the realist must be wary—at least for these purposes—of adopting too strict a notion of success, for a highly robust and stringent construal of “success” would defeat the realist’s purposes. What he wants to explain, after all, is why science in general has worked so well. If he were to adopt a very demanding characterization of success (such as those advocated by inductive logicians or Popperians) then it would probably turn out that science has been largely “unsuccessful” (because it does not have high confirmation) and the realist’s avowed explanandum would thus be a non-problem. Accordingly, I shall assume that a theory is “successful” so long as it has worked well, i.e., so long as it has functioned in a variety of explanatory contexts, has led to confirmed predictions and has been of broad explanatory scope. As I understand the realist’s position, his concern is to explain why certain theories have enjoyed this kind of success.

If we construe “success” in this way, (S1) can be conceded. Whether one’s criterion of success is broad explanatory scope, possession of a large number of confirming instances, or conferring manipulative or predictive control, it is clear that science is, by and large, a successful activity.

What about (S2)? I am not certain that any realist would or should endorse it, although it is a perfectly natural construal of the realist’s claim

that “reference explains success.” The notion of reference that is involved here is highly complex and unsatisfactory in significant respects. Without endorsing it, I shall use it frequently in the ensuing discussion. The realist sense of reference is a rather liberal one, according to which the terms in a theory may be genuinely referring even if many of the claims the theory makes about the entities to which it refers are false. Provided that there are entities which “approximately fit” a theory’s description of them, Putnam’s charitable account of reference allows us to say that the terms of a theory genuinely refer.⁴ On this account (and these are Putnam’s examples), Bohr’s “electron,” Newton’s “mass,” Mendel’s “gene,” and Dalton’s “atom” are all referring terms, while “phlogiston” and “aether” are not (Putnam 1978, pp. 20–22).

Are genuinely referential theories (i.e., theories whose central terms genuinely refer) invariably or even generally successful at the empirical level, as (S2) states? There is ample evidence that they are not. The chemical atomic theory in the 18th century was so remarkably unsuccessful that most chemists abandoned it in favor of a more phenomenological, elective affinity chemistry. The Proutian theory that the atoms of heavy elements are composed of hydrogen atoms had, through most of the 19th century, a strikingly unsuccessful career, confronted by a long string of apparent refutations. The Wegenerian theory that the continents are carried by large subterranean objects moving laterally across the earth’s surface was, for some thirty years in the recent history of geology, a strikingly unsuccessful theory until, after major modifications, it became the geological orthodoxy of the 1960s and 1970s. Yet all of these theories postulated basic entities which (according to Putnam’s “principle of charity”) genuinely exist.

The realist’s claim that we should expect referring theories to be empirically successful is simply false. And, with a little reflection, we can see good reasons why it should be. To have a genuinely referring theory is to have a theory which “cuts the world at its joints,” a theory which postulates entities of a kind that really exist. But a genuinely referring theory need not be such that all—or even most—of the specific claims it makes about the properties of those entities and their modes of interaction are true. Thus, Dalton’s theory makes many claims about atoms which are false; Bohr’s early theory of the electron was similarly flawed in important respects. Contra-(S2), genuinely referential theories need not be strikingly successful, since such theories may be “massively false” (i.e., have far greater falsity content than truth content).

(S2) is so patently false that it is difficult to imagine that the realist need be committed to it. But what else will do? The (Putnamian) realist wants attributions of reference to a theory’s terms to function in an explanation of that theory’s success. The simplest and crudest way of doing that involves a

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claim like (S2). A less outrageous way of achieving the same end would involve the weaker

(S2') A theory whose terms refer will usually (but not always) be successful.

Isolated instances of referring but unsuccessful theories, sufficient to refute (S2), leave (S2') unscathed. But, if we were to find a broad range of referring but unsuccessful theories, that would be evidence against (S2'). Such theories can be generated at will. For instance, take any set of terms which one believes to be genuinely referring. In any language rich enough to contain negation, it will be possible to construct indefinitely many unsuccessful theories, all of whose substantive terms are genuinely referring. Now, it is always open to the realist to claim that such “theories” are not really theories at all, but mere conjunctions of isolated statements—lacking that sort of conceptual integration we associate with “real” theories. Sadly a parallel argument can be made for genuine theories. Consider, for instance, how many inadequate versions of the atomic theory there were in the 2000 years of atomic “speculating,” before a genuinely successful theory emerged. Consider how many unsuccessful versions there were of the wave theory of light before the 1820s, when a successful wave theory first emerged. Kinetic theories of heat in the seventeenth and eighteenth century, developmental theories of embryology before the late nineteenth century sustain a similar story. (S2'), every bit as much as (S2), seems hard to reconcile with the historical record.

As Richard Burian has pointed out to me (in personal communication), a realist might attempt to dispense with both of those theses and simply rest content with (S3) alone. Unlike (S2) and (S2'), (S3) is not open to the objection that referring theories are often unsuccessful, for it makes no claim that referring theories are always or generally successful. But (S3) has difficulties of its own. In the first place, it seems hard to square with the fact that the central terms of many relatively successful theories (e.g., aether theories, phlogistic theories) are evidently non-referring. I shall discuss this tension in detail below. More crucial for our purposes here is that (S3) is *not strong enough* to permit the realist to utilize reference to explain success. Unless genuineness of reference entails that all or most referring theories will be successful, then the fact that a theory's terms refer scarcely provides a convincing explanation of that theory's success. If, as (S3) allows, many (or even most) referring theories can be unsuccessful, how can the fact that a successful theory's terms refer be taken to explain why it is successful? (S3) may or may not be true; but in either case it arguably gives the realist no explanatory access to scientific success.

A more plausible construal of Putnam's claim that reference plays a role in explaining the success of science involves a rather more indirect argument. It might be said (and Putnam does say this much) that we can explain why a theory is successful by assuming that the theory is true or approximately true. Since a theory can only be true or nearly true (in any sense of those terms open to the realist) if its terms genuinely refer, it might be argued that reference gets into the act willy-nilly when we explain a theory's success in terms of its truth(like) status. On this account, reference is piggy-backed on approximate truth. The viability of this indirect approach is treated at length in section 4 below so I shall not discuss it here except to observe that if the only contact point between reference and success is provided through the medium of approximate truth, then the link between reference and success is extremely tenuous.

What about (S3), the realist's claim that success creates a rational presumption of reference? We have already seen that (S3) provides no explanation of the success of science, but does it have independent merits? The question specifically is whether the success of a theory provides a warrant for concluding that its central terms refer. Insofar as this is—as certain realists suggest—an empirical question, it requires us to inquire whether past theories which have been successful are ones whose central terms genuinely referred (according to the realist's own account of reference).

A proper empirical test of this hypothesis would require extensive sifting of the historical record of a kind that is not possible to perform here. What I can do is to mention a range of once successful, but (by present lights) non-referring, theories. A fuller list will come later (see section 5), but for now we shall focus on a whole family of related theories, namely, the subtle fluids and aethers of 18th and 19th century physics and chemistry.

Consider specifically the state of aetherial theories in the 1830s and 1840s. The electrical fluid, a substance which was generally assumed to accumulate on the surface rather than permeate the interstices of bodies, had been utilized to explain *inter alia* the attraction of oppositely charged bodies, the behavior of the Leyden jar, the similarities between atmospheric and static electricity and many phenomena of current electricity. Within chemistry and heat theory, the caloric aether had been widely utilized since Boerhaave (by, among others, Lavoisier, Laplace, Black, Rumford, Hutton, and Cavendish) to explain everything from the role of heat in chemical reactions to the conduction and radiation of heat and several standard problems of thermometry. Within the theory of light, the optical aether functioned centrally in explanations of reflection, refraction, interference, double refraction, diffraction and polarization. (Of more than passing interest, optical aether theories had also made some very startling predictions, e.g., Fresnel's prediction of a bright spot at the center of the shadow of a circular disc; a

surprising prediction which, when tested, proved correct. If that does not count as empirical success, nothing does!) There were also gravitational (e.g., LeSage’s) and physiological (e.g., Hartley’s) aethers which enjoyed some measure of empirical success. It would be difficult to find a family of theories in this period which were as successful as aether theories; compared to them, 19th century atomism (for instance), a genuinely referring theory (on realist accounts), was a dismal failure. Indeed, on any account of empirical success which I can conceive of, non-referring 19th-century aether theories were more successful than contemporary, referring atomic theories. In this connection, it is worth recalling the remark of the great theoretical physicist, J. C. Maxwell, to the effect that the aether was better confirmed than any other theoretical entity in natural philosophy!

What we are confronted by in 19th-century aether theories, then, is a wide variety of once successful theories, whose central explanatory concept Putnam singles out as a prime example of a non-referring one (Putnam 1978, p. 22). What are (referential) realists to make of this historical case? On the face of it, this case poses two rather different kinds of challenges to realism: (1) it suggests that (S3) is a dubious piece of advice in that *there can be* (and have been) *highly successful theories some central terms of which are non-referring*; and (2) it suggests that *the realist’s claim that he can explain why science is successful is false at least insofar as a part of the historical success of science has been success exhibited by theories whose central terms did not refer*.

But perhaps I am being less than fair when I suggest that the realist is committed to the claim that *all* the central terms in a successful theory refer. It is possible that when Putnam, for instance, says that “terms in a mature [or successful] science typically refer” (Putnam 1978, p. 20), he only means to suggest that *some* terms in a successful theory or science genuinely refer. Such a claim is fully consistent with the fact that certain other terms (e.g., “aether”) in certain successful, mature sciences (e.g., 19th-century physics) are nonetheless non-referring. Put differently, the realist might argue that the success of a theory warrants the claim that at least some (but not necessarily all) of its central concepts refer.

Unfortunately, such a weakening of (S3) entails a theory of evidential support which can scarcely give comfort to the realist. After all, part of what separates the realist from the positivist is the former’s belief that the evidence for a theory is evidence for *everything* which the theory asserts. Where the stereotypical positivist argues that the evidence selectively confirms only the more “observable” parts of a theory, the realist generally asserts (in the language of Boyd) that:

the sort of evidence which ordinarily counts in favor of the acceptance

of a scientific law or theory is, ordinarily, evidence for the (at least approximate) truth of the law or theory as an account of the causal relations obtaining between the entities [“observation or theoretical”] quantified over in the law or theory in question. (Boyd 1973, p. 1)⁵

For realists such as Boyd, either all parts of a theory (both observational and non-observational) are confirmed by successful tests or none are. In general, realists have been able to utilize various holistic arguments to insist that it is not merely the lower-level claims of a well-tested theory which are confirmed but its deep-structural assumptions as well. This tactic has been used to good effect by realists in establishing that inductive support “flows upward” so as to authenticate the most “theoretical” parts of our theories. Certain latter-day realists (e.g., Glymour) want to break out of this holist web and argue that certain components of theories can be “directly” tested. This approach runs the very grave risk of undercutting what the realist desires most: a rationale for taking our deepest-structure theories seriously, and a justification for linking reference and success. After all, if the tests to which we subject our theories only test *portions* of those theories, then even highly successful theories may well have central terms which are non-referring and central tenets which, because untested, we have no grounds for believing to be approximately true. Under those circumstances, a theory might be highly successful and yet contain important constituents which were patently false. Such a state of affairs would wreak havoc with the realist’s presumption (R1) that success betokens approximate truth. In short, to be less than a holist about theory testing is to put at risk precisely that predilection for deep-structure claims which motivates much of the realist enterprise.

There is, however, a rather more serious obstacle to this weakening of referential realism. It is true that by weakening (S3) to only certain terms in a theory, one would immunize it from certain obvious counter-examples. But such a maneuver has debilitating consequences for other central realist theses. Consider the realist’s thesis (R3) about the retentive character of inter-theory relations (discussed below in detail). The realist both recommends as a matter of policy and claims as a matter of fact that successful theories are (and should be) rationally replaced only by theories which preserve reference for the central terms of their successful predecessors. The rationale for the normative version of this retentionist doctrine is that the terms in the earlier theory, *because it was successful, must* have been referential and thus a constraint on any successor to that theory is that reference should be retained for such terms. This makes sense just in case success provides a blanket warrant for presumption of reference. But if (S3) were weakened so as to say merely that it is reasonable to assume that *some* of the

terms in a successful theory genuinely refer, then the realist would have no rationale for his retentive theses (variants of R3), which have been a central pillar of realism for several decades.⁶

Something apparently has to give. A version of (S3) strong enough to license (R3) seems incompatible with the fact that many successful theories contain non-referring central terms. But any weakening of (S3) dilutes the force of, and removes the rationale for, the realist’s claims about convergence, retention and correspondence in inter-theory relations.⁷ If the realist once concedes that some unspecified set of the terms of a successful theory may well not refer, then his proposals for restricting “the class of candidate theories” to those which retain reference for the *prima facie* referring terms in earlier theories is without foundation (Putnam 1975, p. 22).

More generally, we seem forced to say that such linkages as there are between reference and success are rather murkier than Putnam’s and Boyd’s discussions would lead us to believe. If the realist is going to make his case for CER, it seems that it will have to hinge on approximate truth, (R1), rather than reference, (R2).

4. Approximate Truth and Success: the “Downward Path”

Ignoring the referential turn among certain recent realists, most realists continue to argue that, at bottom, epistemic realism is committed to the view that successful scientific theories, even if strictly false, are nonetheless “approximately true” or “close to the truth” or “verisimilar.”⁸ The claim generally amounts to this pair:

- (T1) if a theory is approximately true, then it will be explanatorily successful; and
- (T2) if a theory is explanatorily successful, then it is probably approximately true.

What the realist would *like* to be able to say, of course, is:

- (T1′) if a theory is true, then it will be successful.

(T1′) is attractive because self-evident. But most realists balk at invoking (T1′) because they are (rightly) reluctant to believe that we can reasonably presume of any given scientific theory that it is true. If all the realist could explain was the success of theories which were true *simpliciter*, his explanatory repertoire would be acutely limited. As an attractive move in the direction of broader explanatory scope, (T1) is rather more appealing. After all, presumably many theories which we believe to be false (e.g., Newtonian

mechanics, thermodynamics, wave optics) were—and still are—highly successful across a broad range of applications.

Perhaps, the realist evidently conjectures, we can find an *epistemic* account of that pragmatic success by assuming such theories to be “approximately true.” But we must be wary of this potential sleight of hand. It may be that there is a connection between success and approximate truth; *but if there is such a connection it must be independently argued for*. The acknowledgedly uncontroversial character of (T1') must not be surreptitiously invoked—as it sometimes seems to be—in order to establish (T1). When (T1')'s antecedent is appropriately weakened by speaking of approximate truth, it is by no means clear that (T1) is sound.

Virtually all the proponents of epistemic realism take it as unproblematic that if a theory were approximately true, it would deductively follow that the theory would be a relatively successful predictor and explainer of observable phenomena. Unfortunately, few of the writers of whom I am aware have defined what it means for a statement or theory to be “approximately true.” Accordingly, it is impossible to say whether the alleged entailment is genuine. This reservation is more than perfunctory. Indeed, on the best known account of what it means for a theory to be approximately true, it does *not* follow that an approximately true theory will be explanatorily successful.

Suppose, for instance, that we were to say in a Popperian vein that a theory, T_1 , is approximately true if its truth content is greater than its falsity content, i.e.,

$$Ct_T(T_1) > > Ct_F(T_1).^9$$

(Where $Ct_T(T_1)$ is the cardinality of the set of true sentences entailed by T_1 and $Ct_F(T_1)$ is the cardinality of the set of false sentences entailed by T_1 .) When approximate truth is so construed, it does *not* logically follow that an arbitrarily selected class of a theory's entailments (namely, some of its observable consequences) will be true. Indeed, it is entirely conceivable that a theory might be approximately true in the indicated sense and yet be such that *all* of its thus far tested consequences are *false*.¹⁰

Some realists concede their failure to articulate a coherent notion of approximate truth or verisimilitude, but insist that this failure in no way compromises the viability of (T1). Newton-Smith, for instance, grants that “no one has given a satisfactory analysis of the notion of verisimilitude” (1981, p. 197), but insists that the concept can be legitimately invoked “even if one cannot at the time give a philosophically satisfactory analysis of it.” He quite rightly points out that many scientific concepts were explanatorily useful long before a philosophically coherent analysis was given for them.

But the analogy is unseemly, for what is being challenged is not whether the concept of approximate truth is philosophically rigorous but rather whether it is even clear enough for us to ascertain whether it entails what it purportedly explains. Until someone provides a clearer analysis of approximate truth than is now available, it is not even clear whether truthlikeness would explain success, let alone whether, as Newton-Smith insists, “the concept of verisimilitude is *required* in order to give a satisfactory theoretical explanation of an aspect of the scientific enterprise.” If the realist would de-mystify the “miraculousness” (Putnam) or the “mysteriousness” (Newton-Smith)¹¹ of the success of science, he needs more than a promissory note that somehow, someday, someone will show that approximately true theories must be successful theories.¹²

Whether there is some definition of approximate truth which does indeed entail that approximately true theories will be predictively successful (and yet still probably false) is not clear.¹³ What can be said is that, promises to the contrary notwithstanding, *none* of the proponents of realism has yet articulated a coherent account of approximate truth which entails that approximately true theories will, across the range where we can test them, be successful predictors. Further difficulties abound. Even if the realist had a semantically adequate characterization of approximate or partial truth, and even if that semantics entailed that most of the consequences of an approximately true theory would be true, he would still be without any criterion that would *epistemically* warrant the ascription of approximate truth to a theory. As it is, the realist seems to be long on intuitions and short on either a semantics or an epistemology of approximate truth.

These should be urgent items on the realists’ agenda since, until we have a coherent account of what approximate truth is, central realist theses like (R1), (T1) and (T2) are just so much mumbo-jumbo.

5. Approximate Truth and Success: the “Upward Path”

Despite the doubts voiced in section 4, let us grant for the sake of argument that if a theory is approximately true, then it will be successful. Even granting (T1), is there any plausibility to the suggestion of (T2) that explanatory success can be taken as a rational warrant for a judgment of approximate truth? The answer seems to be “no.”

To see why, we need to explore briefly one of the connections between “genuinely referring” and being “approximately true.” However the latter is understood, I take it that *a realist would never want to say that a theory was approximately true if its central theoretical terms failed to refer*. If there were nothing like genes, then a genetic theory, no matter how well confirmed it was, would not be approximately true. If there were no entities

similar to atoms, no atomic theory could be approximately true; if there were no sub-atomic particles, then no quantum theory of chemistry could be approximately true. In short, a necessary condition—especially for a scientific realist—for a theory being close to the truth is that its central explanatory terms genuinely refer. (An *instrumentalist*, of course, could countenance the weaker claim that a theory was approximately true so *long* as its directly testable consequences were close to the observable values. But as I argued above, the realist must take claims about approximate truth to refer alike to the observable and the deep-structural dimensions of a theory.)

Now, what the history of science offers us is a plethora of theories which were both successful and (so far as we can judge) non-referential with respect to many of their central explanatory concepts. I discussed earlier one specific family of theories which fits this description. Let me add a few more prominent examples to the list:

- the crystalline spheres of ancient and medieval astronomy;
- the humoral theory of medicine;
- the effluvial theory of static electricity;
- “catastrophist” geology, with its commitment to a universal (Noachian) deluge;
- the phlogiston theory of chemistry;
- the caloric theory of heat;
- the vibratory theory of heat;
- the vital force theories of physiology;
- the electromagnetic aether;
- the optical aether;
- the theory of circular inertia;
- theories of spontaneous generation.

This list, which could be extended *ad nauseam*, involves in every case a theory which was once successful and well confirmed, but which contained central terms which (we now believe) were non-referring. Anyone who imagines that the theories which have been successful in the history of science have also been, with respect to their central concepts, genuinely referring theories has studied only the more “whiggish” versions of the history of science (i.e., the ones which recount only those past theories which are referentially similar to currently prevailing ones).

It is true that proponents of CER sometimes hedge their bets by suggesting that their analysis applies exclusively to “the mature sciences” (e.g., Putnam and Krajewski). This distinction between mature and immature sciences proves convenient to the realist since he can use it to dismiss any *prima facie* counter-example to the empirical claims of CER on the grounds that

the example is drawn from an “immature” science. But this insulating maneuver is unsatisfactory in two respects. In the first place, it runs the risk of making CER vacuous since these authors generally define a mature science as one in which correspondence or limiting case relations obtain invariably between any successive theories in the science once it has passed “the threshold of maturity.” Krajewski grants the tautological character of this view when he notes that “the thesis that there is [correspondence] among successive theories becomes, indeed, analytical” (1977, p. 91). Nonetheless, he believes that there is a version of the maturity thesis which “may be and must be tested by the history of science.” That version is that “every branch of science crosses at some period the threshold of maturity.” But the testability of this hypothesis is dubious at best. There is no historical observation which could conceivably *refute* it since, even if we discovered that no sciences yet possessed “corresponding” theories, it could be maintained that eventually every science will become corresponding. It is equally difficult to *confirm* it since, even if we found a science in which corresponding relations existed between the latest theory and its predecessor, we would have no way of knowing whether that relation will continue to apply to subsequent changes of theory in that science. In other words, the much-vaunted empirical testability of realism is seriously compromised by limiting it to the mature sciences.

But there is a second unsavory dimension to the restriction of CER to the “mature” sciences. The realists’ avowed aim, after all, is to explain why science is successful: that is the “miracle” which they allege the non-realists leave unaccounted for. The fact of the matter is that parts of science, including many “immature” sciences, have been successful for a very long time; indeed, many of the theories I alluded to above were empirically successful by any criterion I can conceive of (including fertility, intuitively high confirmation, successful prediction, etc.). If the realist restricts himself to explaining only how the “mature” sciences work (and recall that very few sciences indeed are yet “mature” as the realist sees it), then he will have completely failed in his ambition to explain why science in general is successful. Moreover, several of the examples I have cited above come from the history of mathematical physics in the last century (e.g., the electromagnetic and optical aethers) and, as Putnam himself concedes, “*physics* surely counts as a ‘mature’ science if any science does” (1978, p. 21). Since realists would presumably insist that many of the central terms of the theories enumerated above do not genuinely refer, it follows that none of those theories could be approximately true (recalling that the former is a necessary condition for the latter). Accordingly, cases of this kind cast very grave doubts on the plausibility of (T2), i.e., the claim that nothing succeeds like approximate truth.

I daresay that for every highly successful theory in the past of science which we now believe to be a genuinely referring theory, one could find half a dozen once successful theories which we now regard as substantially non-referring. If the proponents of CER are the empiricists they profess to be about matters epistemological, cases of this kind and this frequency should give them pause about the well-foundedness of (T2).

But we need not limit our counter-examples to non-referring theories. There were many theories in the past which (so far as we can tell) were both genuinely referring and empirically successful which we are nonetheless loath to regard as approximately true. Consider, for instance, virtually all those geological theories prior to the 1960s which denied any lateral motion to the continents. Such theories were, by any standard, highly successful (and apparently referential); but would anyone today be prepared to say that their constituent theoretical claims—committed as they were to laterally stable continents—are almost true? Is it not the fact of the matter that structural geology was a successful science between (say) 1920 and 1960, even though geologists were fundamentally mistaken about many—perhaps even most—of the basic mechanisms of tectonic construction? Or what about the chemical theories of the 1920s which assumed that the atomic nucleus was structurally homogenous? Or those chemical and physical theories of the late 19th century which explicitly assumed that matter was neither created nor destroyed? I am aware of no sense of approximate truth (available to the realist) according to which such highly successful, but evidently false, theoretical assumptions could be regarded as “truthlike.”

More generally, the realist needs a riposte to the *prima facie* plausible claim that there is no necessary connection between increasing the accuracy of our deep-structural characterizations of nature and improvements at the level of phenomenological explanations, predictions and manipulations. It *seems* entirely conceivable intuitively that the theoretical mechanisms of a new theory, T_2 , might be closer to the mark than those of a rival T_1 and yet T_1 might be more accurate at the level of testable predictions. In the absence of an argument that greater correspondence at the level of unobservable claims is more likely than not to reveal itself in greater accuracy at the experimental level, one is obliged to say that the realist’s hunch that increasing deep-structural fidelity must manifest itself pragmatically in the form of heightened experimental accuracy has yet to be made cogent. (Equally problematic, of course, is the inverse argument to the effect that increasing experimental accuracy betokens greater truthlikeness at the level of theoretical, i.e., deep-structural, commitments.)

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6. The Realists’ Ultimate “*Petitio Principii*”

It is time to step back a moment from the details of the realists’ argument to look at its general strategy. Fundamentally, the realist is utilizing, as we have seen, an abductive inference which proceeds from the success of science to the conclusion that science is approximately true, verisimilar, or referential (or any combination of these). This argument is meant to show the sceptic that theories are not ill-gotten, the positivist that theories are not reducible to their observational consequences, and the pragmatist that classical epistemic categories (e.g., “truth”, “falsehood”) are a relevant part of metascientific discourse.

It is little short of remarkable that realists would imagine that their critics would find the argument compelling. As I have shown elsewhere (1978), ever since antiquity critics of epistemic realism have based their scepticism upon a deep-rooted conviction that the fallacy of affirming the consequent is indeed fallacious. When Sextus or Bellarmine or Hume doubted that certain theories which saved the phenomena were warrantable as true, their doubts were based on a belief that the exhibition that a theory had some true consequences left entirely open the truth-status of the theory. Indeed, many non-realists have been non-realists precisely because they believed that false theories, as well as true ones, could have true consequences.

Now enters the new breed of realist (e.g., Putnam, Boyd and Newton-Smith) who wants to argue that epistemic realism can reasonably be presumed to be true by virtue of the fact that it has true consequences. But this is a monumental case of begging the question. The non-realist refuses to admit that a *scientific* theory can be warrantably judged to be true simply because it has some true consequences. Such non-realists are not likely to be impressed by the claim that a *philosophical* theory like realism can be warranted as true because it arguably has some true consequences. If non-realists are chary about first-order abductions to avowedly true conclusions, they are not likely to be impressed by second-order abductions, particularly when, as I have tried to show above, the premises and conclusions are so indeterminate.

But, it might be argued, the realist is not out to convert the intransigent sceptic or the determined instrumentalist.¹⁴ He is perhaps seeking, rather, to show that realism can be tested like any other scientific hypothesis, and that realism is at least as well confirmed as some of our best scientific theories. Such an analysis, however plausible initially, will not stand up to scrutiny. I am aware of no realist who is willing to say that a *scientific* theory can be reasonably presumed to be true or even regarded as well confirmed just on the strength of the fact that its thus far tested consequences are true. Realists have long been in the forefront of those opposed to *ad hoc* and *post hoc*

theories. Before a realist accepts a scientific hypothesis, he generally wants to know whether it has explained or predicted more than it was devised to explain; he wants to know whether it has been subjected to a battery of controlled tests; whether it has successfully made novel predictions; whether there is independent evidence for it.

What, then, of realism itself as a “scientific” hypothesis?¹⁵ Even if we grant (contrary to what I argued in section 4) that realism entails and thus explains the success of science, ought that (hypothetical) success warrant, by the realist’s own construal of scientific acceptability, the acceptance of realism? Since realism was devised in order to explain the success of science, it remains purely *ad hoc* with respect to that success. If realism has made some novel predictions or been subjected to carefully controlled tests, one does not learn about it from the literature of contemporary realism. At the risk of apparent inconsistency, the realist repudiates the instrumentalist’s view that saving the phenomena is a significant form of evidential support while endorsing realism itself on the transparently instrumentalist grounds that it is confirmed by those very facts it was invented to explain. No proponent of realism has sought to show that realism satisfies those stringent empirical demands which the realist himself minimally insists on when appraising scientific theories. The latter-day realist often calls realism a “scientific” or “well-tested” hypothesis, but seems curiously reluctant to subject it to those controls which he otherwise takes to be a *sine qua non* for empirical well-foundedness.

7. Conclusion

The arguments and cases discussed above seem to warrant the following conclusions:

1. The fact that a theory’s central terms refer does not entail that it will be successful; and a theory’s success is no warrant for the claim that all or most of its central terms refer.
2. The notion of approximate truth is presently too vague to permit one to judge whether a theory consisting entirely of approximately true laws would be empirically successful; what is clear is that a theory may be empirically successful even if it is not approximately true.
3. Realists have no explanation whatever for the fact that many theories which are not approximately true and whose “theoretical” terms seemingly do not refer are nonetheless often successful.
4. The convergentist’s assertion that scientists in a “mature” discipline usually preserve, or seek to preserve, the laws and mechanisms of earlier theories in later ones is probably false; his assertion that when such laws

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are preserved in a successful successor, we can explain the success of the latter by virtue of the truthlikeness of the preserved laws and mechanisms, suffers from all the defects noted above confronting approximate truth.

5. Even if it could be shown that referring theories and approximately true theories would be successful, the realists' argument that successful theories are approximately true and genuinely referential takes for granted precisely what the non-realist denies (namely, that explanatory success betokens truth).
6. It is not clear that acceptable theories either *do* or *should* explain why their predecessors succeeded or failed. If a theory is better supported than its rivals and predecessors, then it is not epistemically decisive whether it explains why its rivals worked.
7. If a theory has once been falsified, it is unreasonable to expect that a successor should retain either all of its content *or* its confirmed consequences *or* its theoretical mechanisms.
8. Nowhere has the realist established—except by fiat—that non-realist epistemologists lack the resources to explain the success of science.

With these specific conclusions in mind, we can proceed to a more global one: it is not yet established—Putnam, Newton-Smith and Boyd notwithstanding—that realism can explain *any* part of the success of science. What is very clear is that realism *cannot*, even by its own lights, explain the success of those many theories whose central terms have evidently not referred and whose theoretical laws and mechanisms were not approximately true. The inescapable conclusion is that insofar as many realists are concerned with explaining how science works and with assessing the adequacy of their epistemology by that standard, they have thus far failed to explain very much. Their epistemology is confronted by anomalies which seem beyond its resources to grapple with.

It is important to guard against a possible misinterpretation of this essay. *Nothing* I have said here refutes the possibility in principle of a realistic epistemology of science. To conclude as much would be to fall prey to the same inferential prematurity with which many realists have rejected in principle the possibility of explaining science in a non-realist way. My task here is, rather, that of reminding ourselves that there *is* a difference between wanting to believe something and having good reasons for believing it. All of us would like realism to be true; we would like to think that science works because it has got a grip on how things really are. But such claims have yet to be made out. Given the *present* state of the art, it can only be wish fulfilment that gives rise to the claim that realism, and realism alone, explains why science works.

Notes

- * I am indebted to all of the following for clarifying my ideas on these issues and for saving me from some serious errors: Peter Achinstein, Richard Burian, Clark Glymour, Adolf Grünbaum, Gary Gutting, Allen Janis, Lorenz Krüger, James Lennox, Andrew Lugg, Peter Machamer, Nancy Maull, Ernan McMullin, Ilkka Niiniluoto, Nicholas Rescher, Ken Schaffner, John Worrall, Steven Wykstra.
- 1 Putnam, evidently following Boyd, sums up (R1) to (R3) in these words:
 - 1) Terms in a mature science typically *refer*.
 - 2) The laws of a theory belonging to a mature science are typically approximately true . . . I will only consider [new] theories . . . which have this property—[they] contain the [theoretical] laws of [their predecessors] as a limiting case (1978, pp. 20–21).
 - 2 Putnam insists, for instance, that if the realist is wrong about theories being referential, then “the success of science is a miracle” (Putnam 1975, p. 69).
 - 3 Boyd remarks: “scientific realism offers an *explanation* for the legitimacy of ontological commitment to theoretical entities” (Putnam 1978, Note 10, p. 2). It allegedly does so by explaining why theories containing theoretical entities work so well: because such entities genuinely exist.
 - 4 Whether one utilizes Putnam’s earlier or later versions of realism is irrelevant for the central arguments of this essay.
 - 5 See also p. 3: “experimental evidence for a theory is evidence for the truth of even its non-observational laws.” See also Sellars (1963, p. 97).
 - 6 A caveat is in order here. *Even if* all the central terms in some theory refer, it is not obvious that every rational successor to that theory must preserve all the referring terms of its predecessor. One can easily imagine circumstances when the new theory is preferable to the old one even though the range of application of the new theory is less broad than the old. When the range is so restricted, it may well be entirely appropriate to drop reference to some of the entities which figured in the earlier theory.
 - 7 For Putnam and Boyd both “it will be a constraint on T_2 [i.e., any new theory in a domain] . . . that T_2 must have this property, the property that *from its standpoint* one can assign referents to the terms of T_1 [i.e., an earlier theory in the same domain]” (Putnam 1978, p. 22). For Boyd, see (1973, p. 8): “new theories should, *prima facie*, resemble current theories with respect to their accounts of causal relations among theoretical entities.”
 - 8 For just a small sampling of this view, consider the following: “The claim of a realist ontology of science is that the only way of explaining why the models

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of science function so successfully . . . is that they approximate in some way the structure of the object” (McMullin 1970, pp. 63–64); “the continued success [of confirmed theories] can be *explained* by the hypothesis that they are in fact close to the truth . . . ” (Niiniluoto 1980, p. 448); the claim that “the laws of a theory belonging to a mature science are typically approximately *true* . . . [provides] an *explanation* of the behavior of scientists and the success of science” (Putnam 1978, pp. 20–21). Smart, Sellars, and Newton-Smith, among others, share a similar view.

- 9 Although Popper is generally careful not to assert that actual historical theories exhibit ever-increasing truth content (for an exception, see his (1963, p. 220)), other writers have been more bold. Thus, Newton-Smith writes that “the historically generated sequence of theories of a mature science is a sequence in which succeeding theories are increasing in truth content without increasing in falsity content.” [See Newton-Smith 1981, p. 184. Laudan is quoting from a manuscript of Newton-Smith’s book. The wording of the quotation differs slightly from the published version.]
- 10 On the more technical side, Niiniluoto has shown that a theory’s degree of corroboration co-varies with its “estimated verisimilitude” (1977, pp. 121–147 and 1980). Roughly speaking, “estimated truthlikeness” is a measure of how closely (the content of) a theory corresponds to *what we take to be* the best conceptual systems that we so far have been able to find (1980, pp. 443ff.). If Niiniluoto’s measures work it follows from the above-mentioned co-variance that an empirically successful theory will have a high degree of estimated truthlikeness. But because estimated truthlikeness and genuine verisimilitude are not necessarily related (the former being parasitic on existing evidence and available conceptual systems), it is an open question whether—as Niiniluoto asserts—the continued success of highly confirmed theories can be *explained* by the hypothesis that they in fact are close to the truth at least in the relevant respects. Unless I am mistaken, this remark of his betrays a confusion between “true verisimilitude” (to which we have no epistemic access) and “estimated verisimilitude” (which is accessible but non-epistemic).
- 11 Newton-Smith claims that the increasing predictive success of science through time “would be totally mystifying . . . if it were not for the fact that theories are capturing more and more truth about the world” (1981, p. 196).
- 12 I must stress again that I am *not* denying that there *may* be a connection between approximate truth and predictive success. I am only observing that until the realists show us what that connection is, they should be more reticent than they are about claiming that realism can explain the success of science.

- 13 A *non-realist* might argue that a theory is approximately true just in case all its *observable* consequences are true or within a specified interval from the true value. Theories that were “approximately true” in this sense would indeed be demonstrably successful. But, the realist’s (otherwise commendable) commitment to taking seriously the theoretical claims of a theory precludes him from utilizing any such construal of approximate truth, since he wants to say that the theoretical as well as the observational consequences are approximately true.
- 14 I owe the suggestion of this realist response to Andrew Lugg.
- 15 I find Putnam’s views on the “empirical” or “scientific” character of realism rather perplexing. At some points, he seems to suggest that realism is both empirical and scientific. Thus, he writes: “If realism is an explanation of this fact [namely, that science is successful], realism must itself be an overarching scientific *hypothesis*” (1978, p. 19). Since Putnam clearly maintains the antecedent, he seems committed to the consequent. Elsewhere he refers to certain realist tenets as being “our highest level empirical generalizations about knowledge” (p. 37). He says moreover that realism “could be false,” and that “facts are relevant to its support (or to criticizing it)” (pp. 78–79). Nonetheless, for reasons he has not made clear, Putnam wants to deny that realism is either scientific or a hypothesis (p. 79). How realism can consist of doctrines which 1) explain facts about the world, 2) are empirical generalizations about knowledge, and 3) can be confirmed or falsified by evidence and yet be neither scientific nor hypothetical is left opaque.

References

- Boyd, R. (1973), “Realism, Underdetermination, and a Causal Theory of Evidence,” *Noûs* 7: 1–12.
- Krajewski, W. (1977), *Correspondence Principle and Growth of Science*. Dordrecht: Reidel.
- Laudan, L. (1978), “Ex-Huming Hacking,” *Erkenntnis* 13: 417–435.
- McMullin, Ernan (1970), “The History and Philosophy of Science: A Taxonomy,” Stuewer, R. (ed.), *Minnesota Studies in the Philosophy of Science V*: 12–67. Minneapolis: University of Minnesota Press.
- Newton-Smith, W. (1981), *The Rationality of Science*. London: Routledge & Kegan Paul.
- Niiniluoto, Ilkka (1977), “On the Truthlikeness of Generalizations,” Butts, R. and Hintikka, J. (eds.), *Basic Problems in Methodology and Linguistics*: 121–147. Dordrecht: Reidel.
- Niiniluoto, Ilkka (1980), “Scientific Progress,” *Synthese* 45: 427–462.
- Popper, K. (1963), *Conjectures and Refutations*. London: Routledge & Kegan Paul.
- Putnam, H. (1975), *Mathematics, Matter and Method, Vol. 1*. Cambridge: Cambridge University Press.

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- Putnam, H. (1978), *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul.
- Sellars W. (1963), *Science, Perception and Reality*. New York: The Humanities Press.