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Author(s): Larry Laudan
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A CONFUTATION OF CONVERGENT REALISM*

LARRY LAUDAN†

University of Pittsburgh

This essay contains a partial exploration of some key concepts associated with the epistemology of realist philosophies of science. It shows that neither reference nor approximate truth will do the explanatory jobs that realists expect of them. Equally, several widely-held realist theses about the nature of inter-theoretic relations and scientific progress are scrutinized and found wanting. Finally, it is argued that the history of science, far from confirming scientific realism, decisively confutes several extant versions of avowedly 'naturalistic' forms of scientific realism.

> 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 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

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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;

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.

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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.

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[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).

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. Sections **3** through **5** of this essay deal with the first abductive argument; section **6** deals with the second.

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:

[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.

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).

[4]Whether one utilizes Putnam's earlier or later versions of realism is irrelevant for the central arguments of this essay.

(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 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 counterexamples. 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 the-

[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).

ories 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 'ap-

[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".

proximately 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

[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 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 forthcoming, p. 21); 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.

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" (forthcoming, p. 16), 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 some-

[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" (forthcoming, p. 2).

[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 forthcoming). 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).

one provides a clearer analysis of approximate truth than is now available, it is not even clear whether truth-likeness 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

[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" (forthcoming, p. 15).

[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.

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 maneuvre 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 loathe 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.)

6. Confusions About Convergence and Retention. Thus far, I have discussed only the static or synchronic versions of CER, versions which make absolute rather than relative judgments about truthlikeness. Of equal appeal have been those variants of CER which invoke a notion of what is variously called convergence, correspondence or cumulation. Proponents of the diachronic version of CER supplement the arguments discussed above ((S1)-(S4) and (T1)-(T2)) with an additional set. They tend to be of this form:

C1) If earlier theories in a scientific domain are successful and thereby, according to realist principles (e.g.,(S3)above), approximately true, then scientists should only accept later theories which retain appropriate portions of earlier theories;

C2) As a matter of fact, scientists do adopt the strategy of (C1) and manage to produce new, more successful theories in the process;

C3) The 'fact' that scientists succeed at retaining appropriate parts of earlier theories in more successful successors shows that the earlier theories did genuinely refer and that they were approximately true. And thus, the strategy propounded in (C1) is sound.[14]

Perhaps the prevailing view here is Putnam's and (implicitly) Popper's, according to which rationally-warranted successor theories in a 'mature' science must (a) contain reference to the entities apparently referred to in the predecessor theory (since, by hypothesis, the terms in the earlier theory refer), and (b) contain the 'theoretical laws' and 'mechanisms' of the predecessor theory as limiting cases. As Putnam tells us, a 'realist' should insist that *any* viable successor to an old theory T_1 must "contain the laws of T_1 as a limiting case" (1978, p. 21). John Watkins, a like-minded convergentist, puts the point this way:

> It typically happens in the history of science that when some hitherto dominant theory T is superceded by T^1, T^1 is in the relation of correspondence to T [i.e., T is a 'limiting case' of T^1] (1978, pp. 376–377).

[14]If this argument, which I attribute to the realists, seems a bit murky, I challenge any reader to find a more clear-cut one in the literature! Overt formulations of this position can be found in Putnam, Boyd and Newton-Smith.

Numerous recent philosophers of science have subscribed to a similar view, including Popper, Post, Krajewski, and Koertge.[15]

This form of retention is not the only one to have been widely discussed. Indeed, realists have espoused a wide variety of claims about what is or should be retained in the transition from a once successful predecessor (T_1) to a successor (T_2) theory. Among the more important forms of realist retention are the following cases: (1) T_2 entails T_1 (Whewell); (2) T_2 retains the true consequences or truth content of T_1 (Popper); (3) T_2 retains the 'confirmed' portions of T_1 (Post, Koertge); (4) T_2 preserves the theoretical laws and mechanisms of T_1 (Boyd, McMullin, Putnam); (5) T_2 preserves T_1 as a limiting case (Watkins, Putnam, Krajewski); (6) T_2 explains why T_1 succeeded insofar as T_1 succeeded (Sellars); (7) T_2 retains reference for the central terms of T_1 (Putnam, Boyd).

The question before us is whether, when retention is understood in *any* of these senses, the realist's theses about convergence and retention are correct.

6.1 *Do Scientists Adopt the 'Retentionist' Strategy of CER?* One part of the convergent realist's argument is a claim to the effect that scientists generally adopt the strategy of seeking to preserve earlier theories in later ones. As Putnam puts it:

> preserving the *mechanisms* of the earlier theory as often as possible, which is what scientists try to do . . . That scientists try to do this . . . is a fact, and that this strategy has led to important discoveries . . . is also a fact (1978, p. 20).[16]

In a similar vein, Szumilewicz (although not stressing realism) insists that many eminent scientists made it a main heuristic requirement of their

[15]Popper: "a theory which has been well corroborated can only be superseded by one . . . [which] *contains* the old well-corroborated theory—or at least a good approximation to it" (1959, p. 276).

Post: "I shall even claim that, as a matter of empirical historical fact, [successor] theories [have] always explained the *whole* of [the well-confirmed part of their predecessors]" (1971, p. 229).

Koertge: "nearly all pairs of successive theories in the history of science stand in a correspondence relation and . . . where there is no correspondence to begin with, the new theory will be developed in such a way that it comes more nearly into correspondence with the old" (1973, p. 176–177). Among other authors who have defended a similar view, one should mention (Fine 1967, p. 231 ff.), (Kordig 1971, pp. 119–125), (Margenau 1950) and (Sklar 1967, pp. 190–224).

[16]Putnam fails to point out that it is also a fact that many scientists do *not* seek to preserve earlier mechanisms and that theories which have not preserved earlier theoretical mechanisms (whether the germ theory of disease, plate tectonics, or wave optics) have led to important discoveries is also a fact.

research programs that a new theory stand in a relation of 'correspondence' with the theory it supersedes (1977, p. 348). If Putnam and the other retentionists are right about the strategy which most scientists have adopted, we should expect to find the historical literature of science abundantly provided with (a) proofs that later theories do indeed contain earlier theories as limiting cases, or (b) outright rejections of later theories which fail to contain earlier theories. Except on rare occasions (coming primarily from the history of mechanics), one finds neither of these concerns prominent in the literature of science. For instance, to the best of my knowledge, literally no one criticized the wave theory of light because it did not preserve the theoretical mechanisms of the earlier corpuscular theory; no one faulted Lyell's uniformitarian geology on the grounds that it dispensed with several causal processes prominent in catastrophist geology; Darwin's theory was not criticized by most geologists for its failure to retain many of the mechanisms of Lamarckian 'evolutionary theory'.

For all the realist's confident claims about the prevalence of a retentionist strategy in the sciences, I am aware of *no* historical studies which would sustain as a *general* claim his hypothesis about the evaluative strategies utilized in science. Moreover, insofar as Putnam and Boyd claim to be offering "an explanation of the [retentionist] behavior of scientists" (Putnam 1978, p. 21), they have the wrong explanandum, for if there is any widespread strategy in science, it is one which says, "accept an empirically successful theory, regardless of whether it contains the theoretical laws and mechanisms of its predecessors".[17] Indeed, one could take a leaf from the realist's (C2) and claim that the success of the strategy of assuming that earlier theories do not generally refer shows that it is true that earlier theories generally do not!

(One might note in passing how often, and on what evidence, realists imagine that they are speaking for the scientific majority. Putnam, for instance, claims that "realism is, so to speak, 'science's philosophy of science' " and that "science taken at 'face value' *implies* realism" (1978, p. 37).[18] Hooker insists that to be a realist is to take science "seriously" (1976, pp. 467–472), as if to suggest that conventionalists, instrumentalists and positivists such as Duhem, Poincaré, and Mach did not take science seriously. The willingness of some realists to attribute realist strategies to working scientists—on the strength of virtually no empirical research into the principles which *in fact* have governed scientific practice—raises doubts about the seriousness of their avowed commitment to the empirical character of epistemic claims.)

[17]I have written a book about this strategy, (Laudan 1977).

[18]After the epistemological and methodological battles about science during the last three hundred years, it should be fairly clear that science, taken at its face value, *implies* no particular epistemology.

6.2 *Do Later Theories Preserve the Mechanisms, Models, and Laws of Earlier Theories?* Regardless of the explicit strategies to which scientists have subscribed, are Putnam and several other retentionists right that later theories "typically" entail earlier theories, and that "earlier theories are, very often, limiting cases of later theories".[19] Unfortunately, answering this question is difficult, since "typically" is one of those weasel words which allows for much hedging. I shall assume that Putnam and Watkins mean that "most of the time (or perhaps in most of the important cases) successor theories contain predecessor theories as limiting cases". So construed, the claim is patently false. Copernican astronomy did not retain all the key mechanisms of Ptolemaic astronomy (e.g., motion along an equant); Newton's physics did not retain all (or even most of) the 'theoretical laws' of Cartesian mechanics, astronomy and optics; Franklin's electrical theory did not contain its predecessor (Nollet's) as a limiting case. Relativistic physics did not retain the aether, nor the mechanisms associated with it; statistical mechanics does not incorporate all the mechanisms of thermodynamics; modern genetics does not have Darwinian pangenesis as a limiting case; the wave theory of light did not appropriate the mechanisms of corpuscular optics; modern embryology incorporates few of the mechanisms prominent in classical embryological theory. As I have shown elsewhere,[20] loss occurs at virtually every level: the confirmed predictions of earlier theories are sometimes not explained by later ones; even the 'observable' laws explained by earlier theories are not always retained, not even as limiting cases; theoretical processes and mechanisms of earlier theories are, as frequently as not, treated as flotsam.

The point is that some of the most important theoretical innovations have been due to a willingness of scientists to violate the cumulationist or retentionist constraint which realists enjoin 'mature' scientists to follow.

There is a deep reason why the convergent realist is wrong about these matters. It has to do, in part, with the role of ontological frameworks in science and with the nature of limiting case relations. As scientists use the term 'limiting case', T_1 can be a limiting case of T_2 only if (a) *all* the variables (observable and theoretical) assigned a value in T_1 are assigned a value by T_2 and (b) the values assigned to every variable of T_1 are the same as, or very close to, the values T_2 assigns to the corresponding variable when certain initial and boundary conditions—consistent with T_2[21]—are specified. This seems to require that T_1 can be a limiting case

[19](Putnam 1978, pp. 20, 123).

[20](Laudan 1976, pp. 467–472).

[21]This matter of limiting conditions consistent with the 'reducing' theory is curious. Some of the best-known expositions of limiting case relations depend (as Krajewski has

of T_2 only if *all* the entities postulated by T_1 occur in the ontology of T_2. Whenever there is a change of ontology accompanying a theory transition such that T_2 (when conjoined with suitable initial and boundary conditions) fails to capture T_1's ontology, then T_1 *cannot* be a limiting case of T_2. Even where the ontologies of T_1 and T_2 overlap appropriately (i.e., where T_2's ontology embraces all of T_1's), T_1 is a limiting case of T_2 only if *all* the laws of T_1 can be derived from T_2, given appropriate limiting conditions. It is important to stress that *both* these conditions (among others) must be satisfied before one theory can be a limiting case of another. Where 'closet positivists' might be content with capturing only the formal mathematical relations or only the observable consequences of T_1 within a successor, T_2, any genuine realist must insist that T_1's underlying ontology is preserved in T_2's, *for it is that ontology above all which he alleges to be approximately true*.

Too often, philosophers (and physicists) infer the existence of a limiting case relation between T_1 and T_2 on substantially less than this. For instance, many writers have claimed one theory to be a limiting case of another when only some, but not all, of the laws of the former are 'derivable' from the latter. In other cases, one theory has been said to be a limiting case of a successor when the mathematical laws of the former find homologies in the latter but where the former's ontology is not fully extractable from the latter's.

Consider one prominent example which has often been misdescribed, namely, the transition from the classical aether theory to relativistic and quantum mechanics. It can, of course, be shown that *some* 'laws' of classical mechanics are limiting cases of relativistic mechanics. But there are other laws and general assertions made by the classical theory (e.g., claims about the density and fine structure of the aether, general laws about the character of the interaction between aether and matter, models and mechanisms detailing the compressibility of the aether) which could not conceivably be limiting cases of modern mechanics. The reason is a simple one: a theory cannot assign values to a variable which does not occur in that theory's language (or, more colloquially, it cannot assign properties to entities whose existence it does not countenance). Classical

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observed) upon showing an earlier theory to be a limiting case of a later theory only by adopting limiting assumptions *explicitly denied by the later theory*. For instance, several standard textbook discussions present (a portion of) classical mechanics as a limiting case of special relativity, provided c approaches infinity. But special relativity is committed to the claim that c is a constant. Is there not something suspicious about a 'derivation' of T_1 from a T_2 which essentially involves an assumption inconsistent with T_2? If T_2 is correct, then it forbids the adoption of a premise commonly used to derive T_1 as a limiting case. (It should be noted that most such proofs can be re-formulated unobjectionably, e.g., in the relativity case, by letting $v \rightarrow o$ rather than $c \rightarrow \infty$.)

aether physics contained a number of postulated mechanisms for dealing *inter alia* with the transmission of light through the aether. Such mechanisms could not possibly appear in a successor theory like the special theory of relativity which denies the very existence of an aetherial medium and which accomplishes the explanatory tasks performed by the aether via very different mechanisms.

Nineteenth-century mathematical physics is replete with similar examples of evidently successful mathematical theories which, because some of their variables refer to entities whose existence we now deny, cannot be shown to be limiting cases of our physics. As Adolf Grünbaum has cogently argued, when we are confronted with two incompatible theories, T_1 and T_2, such that T_2 does not 'contain' all of T_1's ontology, then not all the mechanisms and theoretical laws of T_1 which involve those entities of T_1 not postulated by T_2 can possibly be retained—not even as limiting cases—in T_2 (1976, pp. 1–23). This result is of some significance. What little plausibility convergent or retentive realism has enjoyed derives from the presumption that it correctly describes the relationship between classical and post-classical mechanics and gravitational theory. Once we see that even in this *prima facie* most favorable case for the realist (where *some* of the laws of the predecessor theory are genuinely limiting cases of the successor), changing ontologies or conceptual frameworks make it impossible to capture many of the central theoretical laws and mechanisms postulated by the earlier theory, then we can see how misleading is Putnam's claim that "what scientists try to do" is to preserve

> the *mechanisms* of the earlier theory as often as possible—or to show that they are 'limiting cases' of new mechanisms . . . (1978, p. 20).

Where the mechanisms of the earlier theory involve entities whose existence the later theory denies, no scientist does (or should) feel any compunction about wholesale repudiation of the earlier mechanisms.

But even where there is no change in basic ontology, many theories (even in 'mature sciences' like physics) fail to retain all the explanatory successes of their predecessors. It is well known that statistical mechanics has yet to capture the irreversibility of macro-thermodynamics as a genuine limiting case. Classical continuum mechanics has not yet been reduced to quantum mechanics or relativity. Contemporary field theory has yet to replicate the classical thesis that physical laws are invariant under reflection in space. If scientists had accepted the realist's constraint (namely, that new theories must have old theories as limiting cases), neither relativity nor statistical mechanics would have been viewed as viable theories. It has been said before, but it needs to be reiterated over and again: *a proof of the existence of limiting relations between selected com-*

ponents of two theories is a far cry from a systematic proof that one theory is a limiting case of the other. Even if classical and modern physics stood to one another in the manner in which the convergent realist erroneously imagines they do, his hasty generalization that theory successions in all the advanced sciences show limiting case relations is patently false.[22] But, as this discussion shows, not even the realist's paradigm case will sustain the claims he is apt to make about it.

What this analysis underscores is just how reactionary many forms of convergent epistemological realism are. If one took seriously CER's advice to reject any new theory which did not capture existing mature theories as referential and existing laws and mechanisms as approximately authentic, then any prospect for deep-structure, ontological changes in our theories would be foreclosed. Equally outlawed would be any significant repudiation of our theoretical models. In spite of his commitment to the growth of knowledge, the realist would unwittingly freeze science in its present state by forcing all future theories to accomodate the ontology of contemporary ('mature') science and by foreclosing the possibility that some future generation may come to the conclusion that some (or even most) of the central terms in our best theories are no more referential than was 'natural place', 'phlogiston', 'aether', or 'caloric'.

6.3 *Could theories converge in ways required by the realist?* These instances of violations in genuine science of the sorts of continuity usually required by realists are by themselves sufficient to show that the form of scientific growth which the convergent realist takes as his explicandum is often absent, even in the 'mature' sciences. But we can move beyond these specific cases to show in principle that the kind of cumulation demanded by the realist is unattainable. Specifically, by drawing on some results established by David Miller and others, the following can be shown:

a) the familiar requirement that a successor theory, T_2, must both preserve as true the true consequences of its predecessor, T_1, and explain T_1's anomalies is contradictory;

b) that if a new theory, T_2, involves a change in the ontology or conceptual framework of a predecessor, T_1, then T_1 will have true and determinate consequences not possessed by T_2;

c) that if two theories, T_1 and T_2, disagree, then each will have true and determinate consequences not exhibited by the other.

[22] As Mario Bunge has cogently put it: "The popular view on inter-theory relations . . . that every new theory includes (as regards its extension) its predecessors . . . is philosophically superficial, . . . and it is false as a historical hypothesis concerning the advancement of science" (1970, pp. 309–310).

In order to establish these conclusions, one needs to utilize a 'syntactic' view of theories according to which a theory is a conjunction of statements and its consequences are defined *à la* Tarski in terms of content classes. Needless to say, this is neither the only, nor necessarily the best, way of thinking about theories; but it happens to be the way in which most philosophers who argue for convergence and retention (e.g., Popper, Watkins, Post, Krajewski, and Niiniluoto) tend to conceive of theories. What can be said is that if one utilizes the Tarskian conception of a theory's content and its consequences as they do, then the familiar convergentist theses alluded to in (a) through (c) make no sense.

The elementary but devastating consequences of Miller's analysis establish that virtually any effort to link scientific progress or growth to the wholesale retention of a predecessor theory's Tarskian content *or* logical consequences *or* true consequences *or* observed consequences *or* confirmed consequences is evidently doomed. Realists have not only got their history wrong insofar as they imagine that cumulative retention has prevailed in science, but we can see that—given their views on what should be retained through theory change—history could not possibly have been the way their models require it to be. The realists' strictures on cumulativity are as ill-advised normatively as they are false historically.

Along with many other realists, Putnam has claimed that "the mature sciences do converge . . . and that that convergence has great explanatory value for the theory of science" (1978, p. 37). As this section should show, Putnam and his fellow realists are arguably wrong on *both* counts. Popper once remarked that "no theory of knowledge should attempt to explain why we are successful in our attempts to explain things" (1973, p. 23). Such a dogma is too strong. But what the foregoing analysis shows is that an occupational hazard of recent epistemology is imagining that convincing explanations of our success come easily or cheaply.

6.4 *Should New Theories Explain Why Their Predecessors Were Successful?* An apparently more modest realism than that outlined above is familiar in the form of the requirement (R4) often attributed to Sellars—that every satisfactory new theory must be able to explain why its predecessor was successful insofar as it was successful. On this view, viable new theories need not preserve all the content of their predecessors, nor capture those predecessors as limiting cases. Rather, it is simply insisted that a viable new theory, T_N, must explain why, when we conceive of the world according to the old theory T_O, there is a range of cases where our T_O-guided expectations were correct or approximately correct.

What are we to make of this requirement? In the first place, it is clearly *gratuitous*. If T_N has more confirmed consequences (and greater conceptual simplicity) than T_O, then T_N is preferable to T_O even if T_N cannot explain why T_O is successful. Contrariwise, if T_N has fewer confirmed consequences than T_O, then T_N cannot be rationally preferred to T_O even if T_N explains why T_O is successful. In short, a theory's ability to explain why a rival is successful is neither a necessary nor a sufficient condition for saying that it is better than its rival.

Other difficulties likewise confront the claim that new theories should explain why their predecessors were successful. Chief among them is the ambiguity of the notion itself. One way to show that an older theory, T_O was successful is to show that it shares many confirmed consequences with a newer theory, T_N, which is highly successful. But this is not an 'explanation' that a scientific realist could accept, since it makes no reference to, and thus does not depend upon, an epistemic assessment of either T_O or T_N. (After all, an instrumentalist could quite happily grant that if T_N 'saves the phenomena' then T_O—insofar as some of its observable consequences overlap with or are experimentally indistinguishable from those of T_N—should also succeed at saving the phenomena.)

The intuition being traded on in this persuasive account is that the pragmatic success of a new theory, combined with a partial comparison of the respective consequences of the new theory and its predecessor, will sometimes put us in a position to say when the older theory worked and when it failed. But such comparisons as can be made in this manner do not involve *epistemic* appraisals of either the new or the old theory *qua* theories. Accordingly, the possibility of such comparisons provides no argument for epistemic realism.

What the realist apparently needs is an *epistemically* robust sense of 'explaining the success of a predecessor'. Such an epistemic characterization would presumably begin with the claim that T_N, the new theory, was approximately true and would proceed to show that the 'observable' claims of its predecessor, T_O, deviated only slightly from (some of) the 'observable' consequences of T_N. It would then be alleged that the (presumed) approximate truth of T_N and the partially overlapping consequences of T_O and T_N jointly explained why T_O was successful in so far as it was successful. But this is a *non-sequitur*. As I have shown above, the fact that a T_N is approximately true does not even explain why it is successful; how, under those circumstances, can the approximate truth of T_N explain why some theory different from T_N is successful? Whatever the nature of the relations between T_N and T_O (entailment, limiting case, etc.), the epistemic ascription of approximate truth to either T_O or T_N (or both) apparently leaves untouched questions of how successful T_O or T_N are.

The idea that new theories should explain why older theories were successful (insofar as they were) originally arose as a rival to the 'levels' picture of explanation according to which new theories fully explained—because they entailed—their predecessors. It is clearly an improvement over the levels picture (for it does recognize that later theories generally do not entail their predecessors). But when it is formulated as a general thesis about inter-theory relations, designed to buttress a realist epistemology, it is difficult to see how this position avoids difficulties similar to those discussed in earlier sections.

7. 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 warrantedly 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.[23] 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?[24] 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.

[23]I owe the suggestion of this realist response to Andrew Lugg.

[24]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 over-arching 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 criticize 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.

8. 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 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.

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