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Realism, Conventionality and "Realism About"

0. Realism and Conventionality.

0.0. Realism. Scientific realists hold (against social constructivism) that the characteristic product of successful scientific research involves knowledge of causal structures whose existence and whose properties are independent of of the adoption of the theories and conceptual frameworks which describe them, and (against empiricism) that this remains true even when the causal structures in question would have to be unobservable. In general, the case for scientific realism depends on the observation that many apparently central features of scientific concepts and practices seem to involve reference to such theory-independent and unobservable structures; the concepts appear to theoretically defined and the practices to be theory-dependent (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c, 1988, 1989a, 1989b; Hacking 1984; Glymour 1984; McMullin 1984; Putnam 1972, 1975a, 1975b; for a general account of arguments for realism which appeal this observation see Boyd 1983, 1988, 1989b).

01. Anti-realism and Conventionalism. A variety of anti-realist responses to arguments for realism from the theory-dependence of scientific practice are possible but one characteristic and pervasive anti-realist strategy has been to acknowledge that various scientific concepts and practices implicate theoretical knowledge but to provide an interpretation of scientific language and concepts according to which the relevant knowledge is grounded in linguistic convention or social construction rather than knowledge of theory-independent unobservable phenomena.

Thus, for example, according to the operationalism and phenomenalism of Carnap 1928 theoretical claims apparently referring to unobservables are knowable because they are translatable into the physical thing language, and ultimately into the sense-datum language. Crucially, the rules of translation, which certainly look as though they embody claims about the observable effects of unobservable phenomena and about the sensory effects of medium-sized physical objects, are to be rationally reconstructed as truths by convention. The culmination of this tendency within logical empiricism is surely the position of Carnap 1950 according to which the methodological role of unreduced theoretical laws is acknowledged but according to which those laws themselves are to be understood as reflections of linguistic conventions establishing the relevant scientific languages.

Quite similar treatments of the theoretical commitments which govern scientific research were of course advanced by philosophers in the constructivist tradition (see, e.g., Hanson 1958, Kuhn 1960); indeed the treatment of the semantics of theoretical terms in Kuhn 1960 owes so much to the tradition embodied in Carnap 1950 that it is an interesting question how to tell late Carnap from Kuhn (For an answer see Boyd 1989a, 1989b). Thus the strategy of treating certain methodologically significant theoretical doctrines as reflections of linguistic convention has been a central component of anti-realist philosophical arguments within each of the important anti-realist traditions in the philosophy of science.

0.2. Realism and Conventionalism. The dialectical situation established by the conventionalist argumentative strategy within anti-realist philosophy of science led to the establishment within the realist tradition of a strong anti-conventionalist outlook, bolstered by supporting realist conceptions of the semantics of scientific terms. One consequence is that, almost always, philosophers in the realist tradition diagnose a much lower level of linguistic or social conventionality in a given body of scientific discourse than do their empiricist or constructivist colleagues; indeed one of the most significant recent contribution of scientific realism to philosophy generally has been the articulation of the naturalistic conceptions of natural kinds and of reference which underwrite the realist suspicion of appeals to conventionality (Putnam 1972, 1975a, 1975b; Kripke 1971, 1972; Boyd 1979, 1982). As we shall see, however, this deep antipathy to conventionalism also poses problems for scientific realism; these problems are the subject of the present essay.

1. Realism and "Realism About".

1.0. Realism and What There Isn't. In an influential passage Putnam (1975c, p. 290), following Boyd 1971, presents a realist conception of science as embodying the principle that "(t)erms in a mature scientific theory typically refer; Laws of a mature scientific theory are typically approximately true." Surely scientific realism, if it is to be thought of as a systematic conception of science, must be committed to something like this principle. There are, however, some terms in some mature sciences which pretty unproblematically don't refer. There was (and there is) no ether, so the term "ether" in the theory of electromagnetic radiation didn't refer. Arguably, "caloric" didn't either, nor did "phlogiston". Perhaps most of the terms in Ptolemaic astronomy failed to refer to features of the natural world, however apt they may have been for the calculation of the relative position of the earth and various other bodies. Scientific realism must account for occasional cases of reference failure--and perhaps for some wholesale failures of reference, unless a non-question-begging defense of the non-maturity of Ptolemaic astronomy is available.

There are other thing there aren't (or perhaps aren't) for which realists must account. As Putnam (1972, 1975a, 1975b) emphasized, it is an intended consequence of contemporary realist conceptions of kinds properties,and other natural categories that they usually possess natural, real, or "essential"--as opposed to conventional or "nominal"--definitions (see also Kripke 1971, 1972; Boyd 1979, 1988, 1989a, 1989b). Of course it is unproblematical that some aspects of the definitions of scientifically important categories are arbitrary or conventional (the sign of the charge of the electron, the unit of measurement for mass,...). What is interesting is that there are. at least arguably, cases of unexpected conventionality of definition--cases in which we find nominal definitions where realists would ordinarily expect to find real ones. For example there certainly are species and higher taxa, but some systematists (cladists) insist that although species are "real" higher taxa aren't "real". By this they mean that the definitions of higher taxa are largely conventional. Although the reasons cladists make explicit for this surprising conclusion are usually laced with simplistic positivist principles which no sophisticated empiricist could accept, their position can best be understood as ultimately and plausibly grounded in a particular theoretical conception of the mechanisms of macroevolution (for a defense of this non-positivist "rational reconstruction" see Guyot 1987). They are thus the sorts of reasons which a scientific realist must prima facie take seriously. Among the paradigm cases of apparent natural kinds, then, are some which a realist might have to admit are nominal rather than natural.

Once the possibility of unexpected conventionality of definition is acknowledged, it can be seen that there is the closely related possibility of unexpected conventionality of laws or generalizations: cases in which the truth (or approximate truth) of laws or empirical generalizations turn out to depend in unexpected ways on conventions in scientific practice--either conventional definitions of particular terms or broader conventions of methodological and linguistic practice. I am inclined to think that examples of this sort are commonplace, but it's hard to find one that's altogether uncontroversial. Here is a plausible example: I.Q. test scores in typical populations exhibit a normal distribution. Plainly this is an empirical generalization; it is conceivable that it should turn out to be significantly wrong about the results of some methodologically appropriate I.O. testing. It has, moreover, been cited as providing evidence bearing on an important scientific question: the question of the genetic contribution to intelligence differences. According to Lewontin 1976, Jensen 1968 reasons that, since a normal distribution is characteristic of certain sorts of polygenetically determined traits, the normal distribution of I.Q, scores provides some evidence that intelligence is such a trait.

In response one might quite plausibly suggest that the normality of the distribution of I.Q. scores is an artifact of conventions of test design and test standardization. While normality of distribution is not, strictly, part of a nominal definition of I.Q. (so that the normality of actually obtained measurements is an empirical fact), normality may well obtain largely because of conventions and practices in psychometrics rather than because of any underlying structure (genetic or otherwise) in the relevant experimental subjects. If this is right then here is unexpected conventionality in a law or empirical generalization, one which renders the generalization evidentially irrelevant with respect to the question of the genetics of intelligence differences.

1.1 "Realism About" and the Integrity of Scientific Realism. When the issue is raised of whether some apparently referring expression in some scientific theory really referred, or of whether some theoretical expression has a real rather than a nominal definition it is common (if, as I shall argue, misleading) to describe the issue as the issue of "realism about x", where x is the relevant expression. Thus the question of the existence of the ether is the question of "realism about the ether" and the question of whether or not higher taxa have non-conventional definitions is the question of "realism about higher taxa". Presumably questions about the absence or presence of unexpected conventionality in laws or generalizations would receive a similar formulation: Jensen and Lewontin differ, I suppose, regarding "realism about the normality of I.Q. score distributions".

This terminology encourages and, more importantly, reflects a certain fragmented conception of scientific realism, one according to which realism is deeply topic specific. One may be a realist about some sorts of alleged natural phenomena (or natural definitions, or laws or generalizations) and an anti-realist about others, picking and choosing as one's philosophical inclinations dictate. Such a conception weakens the case for scientific realism in several respects. First, part of the attraction of scientific realism is that it appears to offer a distinctive and coherent conception of scientific knowledge--one which, for example, preserves a certain common sense (or perhaps common science) conception of the way in which scientists exploit causal interactions with natural phenomena in order to obtain new knowledge. If instead there is no coherent overall realist picture but instead a piecemeal amalgamation of realist and anti-realist conceptions of various components of scientific theorizing, the philosophical attractiveness of realist positions is significantly reduced.

The absence of a coherent overall realist position weakens the case for "realism about" any particular set of alleged entities or definitions in another way. The philosopher with realist inclinations will, presumably, be a "realist about" those (alleged) entities or definitions or laws with respect to which defending a realist position is easiest, and an "anti-realist" about those regarding which the defense of realism is most difficult. If there is no coherent realist conception of scientific knowledge to rationalize or underwrite this picking and choosing, there will be the reasonable suspicion that the realist is only apparently winning even the easy battles: that eventually these too will provide victories for the systematic anti-realist (or, perhaps, will prove to be cases which the systematically anti-realist empiricist or constructivist need not contest).

This latter difficulty is compounded by another consideration. It is reasonable to argue that the history of recent philosophy of science is a history of concessions by anti-realist philosophers to scientific realism (I develop this theme in Boyd 1988 and 1989a). Thus, for example, the development of theories of the semantics of scientific language seems to be driven almost entirely by the necessity to accommodate the apparent growth of knowledge about unobservable (and/or naturalistically defined) entities. Surely it is a significant part of the *prima facie* case for realism that such concessions have been characteristic of the development of the opposing positions. Of course if realism is the fragmented position suggested by the "realism about" terminology then this case is undermined. Not only is there no coherent realist position to which concessions have been made but each case of reference failure, or of unexpected conventionality of a definition or of a law or generalization will count as a failure of "realism about" and it's acknowledgment will count as a concession to systematic anti-realism.

1.2. Towards Unfragmented Realism. Several considerations suggest that the situation may not be so bad for systematic realism as the "realism about" terminology suggests. In the first place, it seems plain that there are some kinds of conventionality actually present in scientific practice whose recognition poses no threat to scientific realism, supposing there to be such a philosophical position. No one worries about "realism about the choice of units of length" or about "realism about the (exact) number of levels in the Linnean hierarchy". If we had an adequate understanding of why explicit and near-explicit conventional features of scientific language and practice pose no special problems for the integrity of scientific realism, we might expect to see that actual cases of unexpected conventionality pose no problems either.

That this should be so is suggested by the fact that scientific realism--presuming that there is such a coherent and systematic position--seem to predict the occurrence of unexpectedly conventional features of scientific theories. Recall that, on contemporary realist and naturalistic accounts of definition, the establishment of natural definitions for scientific terms arises under circumstances in which there are reasons (typically "theoretical reasons") to believe that certain sorts of similarity and difference in (often unobservable) properties are causally relevant to the behavior of systems under study. Terms are introduced and natural definitions proposed in order to "map" these presumed similarities and differences. Central to he realist's conception of definition is the understanding that particular definitional proposals may be, and often are, mistaken and that naturalistic definitions may thus be revised in the light of the growth of theoretical knowledge. It is precisely this conception of scientific definitions as *a posteriori* which underwrites the realist's response to incommensurability claims of constructivist philosophers of science like Kuhn (1970) (see Putnam 1975a).

If particular definitional proposals often reflect mistaken theoretical commitments, so too might broader definitional projects. Suppose that researchers justifiably believe that certain sorts of similarity and difference and causally important but that they are mistaken, not with respect to individual definitions, but with respect to all or most of their conception of the sorts of similarity and difference which matter causally. Suppose that they introduce (apparently) appropriate terminology and establish tentative definitions which govern the use of that terminology. When (and if) their fundamental error is discovered, it is plausible that the terminology in question should be understood as possessing wholly or largely nominal definitions (Putnam 1975a). If the terminology has been central in the development of the relevant literature, it may be appropriate to retain it, acknowledging its largely arbitrary conventional character. Retrospectively it will be seen that the terminology in question exhibited unexpected conventionality.

Nothing in the scenario just sketched appears incompatible with a systematic realist conception of the growth of scientific knowledge. Indeed some such scenario should be expected on a realist conception of inquiry about a sufficiently complex world. The scenario just sketched is precisely what, according to (an appropriate interpretation of) cladism, has happened with respect to taxonomic terminology above the species level. It hardly seems that a realist should find this possibility difficult to accommodate; no concession to anti-realism seems involved. Instead, it would appear that it is precisely because we have a workable realist conception of how definitions of scientific terms ordinarily work that we can understand what, according to cladists, is peculiar about higher taxa.

Nevertheless it seem obvious that there are certain alleged cases of reference failure or of unexpected conventionality which a realist cold not acknowledge without making a genuine concession to systematic anti-realism. In order to assess the prospects for non-fragmented realism we need to ascertain whether historical and scientific facts and sound philosophical arguments ever dictate acknowledgment of such cases. Of course there is a prior question, "How much conventionality (and how much reference failure) can a systematic scientific realist consistently acknowledge?". It is with this question that we shall be primarily concerned in the rest of the present essay.

2. Realism and the Limits of Conventionality.

2.0. Towards Unfragmented Realism: Dialectics and Philosophical Packages. In Part 2 I'll try to formulate and defend a workable answer to the question "what must a realist be a 'realist about' ?". I'll focus primarily on the question of the sorts of conventionality in science which a systematic realist can acknowledge, and I'll indicate how the answer to that question generalizes naturally to the corresponding question about reference failure.

Elsewhere (Boyd 1988, 1989b) I address the general question of how a particular component of a realist treatment scientific knowledge is to be assessed with regard to the question of its appropriateness vis a vis competing anti-realist conceptions. I argue that, in general, such components are not properly assessed in isolation but instead in terms of the extent to which they contribute to cogency of (one or more versions of) a broader realist "philosophical package" which presents a systematic treatment of epistemological, semantic and metaphysical issues and which incorporates relevant findings from the various special sciences which are the objects of philosophical investigation and from such disciplines as the history and sociology of science, psychology, social theory, etc. The cogency of such packages is itself to be assessed dialectically in terms of their relation to the best available anti-realist philosophical packages of similar scope.

Thus, for example, a proposal by a realist to treat a particular feature of scientific theorizing as largely conventional (or to treat a particular theoretical terms as non-referring) is to be assessed in terms of the contribution which that proposal makes (or fails to make) to the cogency of available realist philosophical packages vis a vis anti-realist alternatives. If a proposal of the sort at issue contributes to their cogency (or, perhaps, is makes no difference to their cogency) then no concession to systematic anti-realism is involved in being "anti-realist" about the relevant phenomenon. If, on the other hand, the adoption of the proposal weakens available realist philosophical packages relative to (some of) their anti-realist competitors then a concession has been made and the "anti-realist" expression is not misleading.

What I propose to argue in the rest of Part 2 is that developments in the philosophy of science have proceeded (I think advanced) to a point at which, in consequence of the resulting dialectical situation vis-a-vis realism and systematic anti-realism, we can identify with some precision a constraint on realist philosophical packages which provides a quite clear answer to the question "what must a realist be a 'realist about'?".

2.1. Recent Philosophy of Science: Two Fixed Points. As I indicated in Section 0.0, arguments for realism and against empiricism in the philosophy of science have almost always proceeded from the observation that some aspect or other of scientific theorizing or practice is dependent on "theoretical" considerations in a way which would be surprising if knowledge of "unobservables" were impossible. Argument for realism grounded in this sort of observation have taken many different forms. Thus, for example, it was often held that various methodologically central scientific practices are unintelligible if the concepts they employ are interpreted according to a verifiability criterion of meaningfulness or that they refute some other empiricist proposal regarding the semantics of scientific terms (see, e.g., Feigl 1956; Hempel 1965; Putnam 1975d; Quine 1961b). Various empiricist proposals for eliminating reference to unobservables were held to be incompatible with the logic of the quantificational structure of scientific theories (Quine 1961a), and it was held that any but a realist understanding of scientific theories would render the predictive success of some of them an inexplicable "miracle" (Putnam 1978).

Against these and other realist arguments, it was for some time common for empiricists in the philosophy of science to deny that--on a proper understanding--scientific practices and concepts are so theory-dependent as they at first appear. Examples of empiricist responses embodying such denials were the defense of operationalism and related eliminativist analyses of theoretical terms in science, and the articulation of an alleged sharp distinction between the "context of theory invention" (where theoretical considerations could play an epistemically harmless role) and the "context of confirmation" (which was to be free of theoretical commitments). The classic example (and the most durable) was the contemporary version of the Humean conception of causation and the associated deductive-nomological account of explanation which, if successful, eliminate reference to unobservable causal powers or underlying mechanisms from the methodologically crucial notions of causation and explanation.

The developments within recent philosophy of science which, I shall argue, permit us to say with some certainty what a realist must be a ":realist about" concern the variety of pro-realist philosophical arguments and the available anti-realist responses. In the first place, there has emerged a near consensus affirming the ineliminability of theoretical commitments from the rational methodological and linguistic practices of science. The anti-realist responses rehearsed above do not work, or at any rate they do not work well enough to eliminate wholesale theoretical commitments from the most clearly rational practices of even the most unproblematically scientific work.

In describing this position as near consensus I mean, of course, to indicate that it is widely accepted by those anti-realists to whom it might seem troubling, as well as by realists and constructivists for whom it is grist for their respective philosophical mills. Thus, for example, van Fraassen (1980) and Fine (1984) join the later person-stages of Putnam (1979, 1981) in acknowledging the ineliminable theory-dependence of scientific methods while, of course, dissenting from a realist explanation for its rationality. Theoretical commitments may be understood realistically (Boyd 1982, 1983; McMullin 1984), according to "internal realism" (Putnam 1979, 1981; Fine 1984), as the social construction of reality (Kuhn 1970), or as a matter of rational acceptance without belief (van Fraassen 1980). They cannot be made to go away. Moreover, and this is important in what follows, not only is there near consensus about the ineliminability of theoretical considerations in science, there is very substantial descriptive, if not philosophical, agreement (due, I believe, largely to the persuasiveness of Kuhn 1970) about how such considerations influence theory choice, experimental design, assessment of evidence, improvements in instrumentation, etc. for both actual cases and a significant range of philosophically relevant counterfactual cases.

The second important development in recent philosophy of science has been the recognition of the centrality of a class of "abductive" arguments for realism: arguments which exhibit a realist understanding of scientific theories as part of the best naturalistic explanation for the success of various features of scientific methods (Putnam 1975c; Boyd 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c; for important critiques see especially Fine 1884; Laudan 1981; van Fraassen 1980). According to these arguments the instrumental reliability of scientific methods (their reliability as a guide to (approximate) truth about observables) is parasitic upon their reliability with respect to (approximate) truth about unobservables.

The arguments in question rely on the observation just discussed that the methods of science are profoundly theory dependent. If they are successful, what they show--about all of the central methodological practices of science--is that their reliability even with respect to observational knowledge is not explicable, nor is their application in seeking such knowledge justifiable, except on the assumption that they typically operate against a background of approximate theoretical knowledge and are reliable in the production of new theoretical knowledge. The methods of science work because they employ available approximate theoretical knowledge to establish an appropriate "fit" between the actual (and often unobservable) causal structures of the relevant phenomena and the methods and practices by which scientists gain additional knowledge (both observational and theoretical) about those phenomena. The selective skepticism of empiricist philosophy of science--accepting scientific knowledge of observables while denying it with respect to unobservable--is thus shown to be untenable, and realism to be the only alternative to extreme skepticism. Once the centrality of these arguments is understood and once it is recognized that the doctrine of the ineliminability of theoretical commitments upon which they depend is near consensus, we are in a position to identify the crucial dialectical constraints on realist philosophical packages and assess their implications for the question of "realism about".

2.2. The Central Core of Scientific Realism. We are concerned to distinguish those cases of denying "realism about" some phenomena or other which are harmless to, or required by, a systematically developed realist philosophical package from those cases whose incorporation in a realist philosophical package would be genuine concessions to systematic anti-realism. In the light of the developments in the literature just rehearsed, I propose that we can identify two philosophical doctrines which define the essential central core of any realist philosophical package. The systematic realist, I suggest, is compelled to accept "realism about" in just those cases in which "realism about" is a necessary component in any defensible philosophical package which treats the relevant scientific cases and which incorporates the two central core realist doctrines.

The central arguments for realism are the abductive arguments for realism as a component in the best explanation for the instrumental reliability of various (uncontroversially) theory-dependent methods. It will be central to any realist philosophical package, then, that the relevant realist explanations are, almost always, the correct ones. The first central core component of scientific realism is the doctrine of the *epistemic centrality of theoretical knowledge*: when reliable methodological practices which contribute to the criteria for theory choice, experimental design, assessment of evidence, judgments of projectability, etc. in successful scientific research are theory-dependent in the now familiar ways, their reliability is (almost always) explained by the approximate truth (as accounts of the causal structure of the relevant phenomena) of the background theories upon which they depend, and their application is (almost always) justified by the approximate knowledge thus embodied in those theories. The epistemic centrality doctrine differs from the almost uncontroversial doctrine of the ineliminability of theoretical commitments in that it entails that the success of theory-dependent methodological practices is explained by background theoretical knowledge, and thus that knowledge of unobservables is possible.

The astute reader will have observed that what has been said thus far about the dialectical situation of realism in the current literature does not obviously apply more to realist than to constructivist conceptions of scientific knowledge. The ineliminability of theoretical commitments is every bit as central to constructivist philosophy of science as to realist philosophy of science; indeed it may be largely through the efforts of philosophers and historians influenced by constructivism that it has emerged as almost uncontroversial. Likewise, although the term "abductive" seems much too naturalistic, the central abductive arguments for scientific realism against empiricism would seem equally available to the constructivist; indeed, Kuhn's (1970) arguments that the world scientists study must be one in which the most fundamental laws in the relevant paradigm are true has much in common with abductive arguments for realism. In consequence, the doctrine of the epistemic centrality of theoretical knowledge may be as central to constructivist as to realist philosophical packages. We thus have yet to see upon what principle we can distinguish viable realist philosophical packages from those whose rejection of various instances of "realism about" represents an untenable concession to constructivist anti-realism.

The answer is ultimately provided, I believe, by a recognition that the realist denies, while the constructivist affirms, that the adoption of theories, paradigms, research interests, conceptual frameworks, or perspectives in some way constitutes, or contributes to the constitution of, the causal powers of and the causal relations between the objects scientists study in the context of those theories, frameworks, etc. Of course the realist does not deny that the adoption of theories, frameworks etc, is a causal phenomena and thus will contribute *causally* to the establishment of, for example, those causal factors which are explanatory in the history, philosophy and sociology of science. [Thus in particular the adoption of a theory in such a discipline could contribute causally to the causal powers and relations which are the subject matter of the theory itself.] What the realist denies is that there is some further sort of contribution (logical, conceptual, socially constructive, or the like) which the adoption of theories, conceptual frameworks, and the like makes to the establishment of causal powers and relations. Realists affirm, and constructivists deny, the *no non-causal contribution doctrine*: the doctrine that the adoption of theories, frameworks, paradigms, projects, intellectual or pratical interests etc. makes no non-causal contribution to the causal structure of the world scientists study. This is the second central core doctrine of scientific realism, the one whose successful incorporation into a philosophical package in the philosophy of science assures us that it makes no crucial concessions to constructivist anti-realism. [In the immediately preceding discussion I borrow heavily from material appearing in Boyd 1989b.] It remains to see how the identification of these two central core realist doctrines permits a solution to the problem of "realism about".

2.3. *What Must a Realist be a "Realist About"?, Part One: How Much Conventionality Can a Realist Accept?* Even those, like social constructivists, who adopt a deeply conventionalist conception of scientific theories ordinarily do not hold that the content of theories is entirely conventional: at least in the empirical science conventionality affects some features of theories and not others. If we are to investigate the acceptable levels of conventionality in realist philosophical packages, we need some terminology to reflect this fact. Let us say that the choice between one or another of two theories or descriptive schemes (henceforth: conceptions) is *arbitrary from a realist perspective* (henceforth: arbitrary; but recall that this abbreviated terminology might be unacceptable to the constructivist: world-constituting theories will hardly seem arbitrary) just in case the correctness of such a choice would depend on facts about linguistic conventions or about the particular history of language use within the scientific community rather than upon a difference in how well the conceptions reflect casual structures which are themselves independent of such choices. Our question then is this: under what circumstances may the systematic realist acknowledge that the choice between two conceptions is arbitrary, and under what conditions must she not, on pain of having made a concession to systematic anti-realism?

We have identified the two central core doctrines of scientific realism. Let us restate the second of these using the terminology just introduced: If the choice between two conceptions is arbitrary (in particular if it is conventional) then they reflect the causal structure of the world exactly equally well (or badly). [Note that this is a distinctly anti-constructivist claim, as required; it might be acceptable to an empiricist.]

Consider now how this principle interacts with the other central core realist principle: the doctrine of the epistemic centrality of theoretical knowledge. According to the latter, methods dictated by theoretical conceptions are reliable because, and to the extent that, the background theories they depend on provide a relevantly accurate account of causal structures; it is the "fit" of theory-dependent methods to the actual causal structure of the world which explains their reliability. Two theories between which the choice is arbitrary reflect the relevant causal structure exactly equally well. What should we then say about the case in which the arbitrariness of the choice is not recognized--about the case in which scientists mistakenly take two such theories to reflect different or competing conceptions of causal structure?

Well, where there are features of the two theories which *appear* (by the best prevailing standards) to be reflections of competing or different conceptions of causal structures but are in a matter of arbitrary choice between the theories, scientists will warrantedly take those features to be methodologically significant--to be relevant to methodological judgements about, e.g., the assessment of the import of experimental evidence or about the explanatory power of other theoretical proposals. They will see the two theories as underwriting different or competing methodological standards. What the realist must hold, in the light of the doctrine of the epistemic centrality of theoretical knowledge, is that in such cases the scientists in question are (non-culpably) mistaken. *The methodological judgments which are peculiar to one rather than another of two theories between which the choice is arbitrary will be reliable, if at all, only accidentally*: two such theories are *epistemically equipotent*--exactly equally reliable as guides to the identification of reliable methods. This equipotency doctrine is an important corollary to the two central core doctrines of scientific realism.

Here's another: When it is concluded about two theories that the choice between them is, by realist standards, arbitrary, it must be held that all prior methodological judgments which reflected commitment to one as opposed to the other of the theories must have been (if at all) only accidentally reliable, and it must be held that subsequent methodological applications of the theories (if they are still, or come to be, well confirmed) must reflect the arbitrariness of the choice between them by being insensitive to which is chosen.

We are now, I think, in a position to answer the question: how much conventionality can a scientific realist acknowledge? Notice that we are not asking how much conventionality a scientific realist should ideally acknowledge--how much conventionality would be acknowledged in the best possible realist philosophical package. We're asking what sorts of conventionality, if acknowledged, would be significant concessions to systematic anti-realism (and thus support the implicatures of the "realism about" idiom) and what sorts would not. Let us consider what constraints respect for the central core realist doctrines puts on the assembling of a realist philosophical package.

In the first place, the doctrine of the epistemic centrality of theoretical knowledge commits the realist *prima facie* to holding, about every background theoretical principle which contributes to the instrumental success of theory-dependent methods in a successful science, that it so contributes because it is relevantly approximately true. Moreover, except for background theoretical claims appearing in the earliest stages in the construction of a successful research program, the realist will need to present the relevant background theoretical claims as *themselves* a reflection of approximate knowledge, and will thus *prima facie* need to explain the reliability of the theory-dependent methods by which those claims were obtained by appealing to the approximate truth of the background theories which determined *those* methods, and so forth. *Prima facie* the realist must accept the approximate truth of all those background theoretical principles which are thus directly or indirectly implicated in the methods by which instrumental knowledge is obtained in well developed successful sciences. [For a treatment of the issue of the earliest stages in successful research traditions see Boyd 1982, 1988, 1989b.]

What the equipotency principles (and the no non-causal contribution doctrine which underwrites them) tell us is that, in the relevant realist explanations of the success of methods, respects of (approximate) truth which are merely conventional (or otherwise arbitrary) don't count. The realist must hold that the distinction between theories between which the choice is arbitrary is irrelevant to questions of justification and method. Thus the realist can successfully incorporate the claim that such a choice is arbitrary into a successful realist philosophical package only when, in the light of the equipotency principles, that claim does not compromise her commitments arising from the doctrine of the epistemic centrality of theoretical knowledge .

What I propose is that this is the only fundamental constraint on realist attributions of conventionality. Realist acknowledgment of conventionality which don't conflict, given the actual conduct of science and in the light of the equipotency principles, with the doctrine of the epistemic centrality of theoretical knowledge may be mistakes, but they preserve the central doctrines upon which the defense of realism against empiricism and constructivism depend. They should not be viewed as concessions to anti-realism.

What does this mean in practice? Where features of theoretical claims are central to the methodological judgments directly or indirectly implicated in the methods by which apparent instrumental knowledge is obtained in an established science, the burden of proof is strongly on the realist who claims that those features are conventional or otherwise arbitrary. That burden can be discharged *only* (as in the case of the cladist assessment of higher taxa, if, and to the extent that, it succeeds) by a *scientific* critique of the scientific community's assent to the relevant features of the theoretical claims in question--one which results in a rebuttal to the causal claim that the methods associated with those features are systematically reliable. With respect to features of theoretical claims which are not so implicated in the establishment of instrumental knowledge, the realist can affirm conventionality without thus being "anti-realist" in any interesting sense and certainly without making concessions to systematic anti-realism.

What must a realist be a "realist about"? In so far as the issue of conventionality is concerned: Only about what is implicated in instrumentally reliable methodology.

2.4. *What Must a Realist be a "Realist About"?, Part Two: How Much Reference Failure Can a Realist Accept?* Let us turn now to the question of how much reference failure the systematic scientific realist can acknowledge. Here the answer is considerably easier than in the case of the question of conventionality since concessions to constructivist anti-realism will not ordinarily be at issue. I propose that, as before, we take the realist be *required* to be a "realist about" when such "realism about" is required in order to permit the articulation of a defensible philosophical package incorporating the two central core doctrines. The no non-causal contribution doctrine will not ordinarily be at issue, so our concern will be only with the doctrine of the epistemic centrality of theoretical knowledge. As before, where features of theoretical claims are central to the methodological judgments directly or indirectly implicated in the methods by which apparent instrumental knowledge is obtained in an established science, the realist must *prima facie* portray those features as approximate reflections of actual causal structures. Again as before, the realist can justifiably avoid this obligation with respect to a particular feature of the relevant theoretical claims only if, and to the extent that, she can offer a justifiable *scientific* critique of those features and of the methodological judgements in which they are implicated.

Perhaps the most common way for a body of theory to provide approximate truth about causal relations is for all of its constituent terms to refer to real phenomena, about which the relevant theoretical principles say things that are approximately true. But this is by no means the only way. Some terms in a body of approximately true theories may partially denote (Field 1973). Some terms may fail to enter into any reference-like relation whatsoever; their introduction may represent deeply mistaken theoretical commitment. Even in such cases, statements embodying those terms may reflect important approximations to the truth; consider, for example, a deeply Platonist early 19th century biological work which discourses about "specific forms" but which uses that terminology to present some significant information about the differences between various species of birds.

Hence the realist, in portraying methodologically central theories as relevantly approximately true need not treat all of their constituent terms as (even partially) referring. What she must do is to portray them as being approximately true in respects suitable to explain the reliability of the methods they underwrite. The standards for assessing realist explanations of the reliability of particular methods are just those of ordinary science (see Boyd 1989b, especially sections 3,3, 3.4). Thus the realist must treat a theoretical term as referring (or partially denoting) only when such a treatment is required, by ordinary scientific standards, in order to causally explain the instrumental reliability of some particular scientific methods.

What must a realist be a "realist about"? With respect to the issue of reference failure, as with respect to the issue of conventionality: Only about what is implicated in instrumentally reliable methodology.

3. *Applications*.

3.0. *Interest-Dependence of Kinds*. Scientific language, according to realists. must be employed to "cut the world at its joints" where the appeal to joints is an appeal to the notion of causally significant similarity and difference. What we have just seen is that the realist may be faithful to this naturalistic conception of the semantics of scientific language while still acknowledging even inexplicit and unexpected conventionality in the definitions of scientific terms. In particular, where the no non-causal contribution doctrine is honored no concession to constructivist anti-realism or related conceptions is involved in the acknowledgment of conventionality.

Sometimes it is held that realist treatments of natural definitions must make a fatal concession to constructivism on a related point. It is an interesting but uncontroversial fact is that the location of the "joints" at which the world must be cut must be thought of as depending on the particular sort of natural phenomena under study: respects of similarity and difference may be causally significant with respect to one sort of phenomenon and insignificant with respect to another. Sometimes this point is put by saying that natural kinds (or the naturalness of natural kinds, or the reality of natural kinds) are interest-dependent: which sorting procedure is appropriately natural will depend on the interests of the investigating parties. Some philosophers appear to hold that these phenomena of interest-dependence by themselves constitute a refutation of the realist's conception that scientists study a largely mind-independent reality and that they thus favor some sort of social constructivist conception.

The considerations rehearsed in the preceding section suggest that the "interest-dependence" of natural kinds just discussed is unproblematically compatible with a realism. To describe either the definitions of natural kinds, magnitudes, etc. or their "reality" as "interest-dependent" is potentially misleading. It is fruitful to talk about possible intellectual or practical projects--sets of questions and problems together with some specification of the form of the anticipated answers or solutions. According to the realist conception, for the problems and questions set by a project to be answered and solved the terms in which the solutions and answers are formulated would have to be defined a posteriori in terms of the relevant sorts of (similarities and differences in) causal powers. That this is so does not, according to the realist, depend at all on whether the project in question is one in which humans (or others) are actually interested or engaged. Neither the causal powers (differences, similarities) of the possible objects of study, nor the appropriatenes of methods for studying them, depend non-causally on actual study or actual interest or on any other candidate for "social construction". Or, at any rate, nothing in the unproblematical "interest-dependence" of natural kinds suggests otherwise. There is no reason to suppose that the no non-causal contribution doctrine would have to be abandoned in a coherent philosophical package which acknowledged the "interest-dependence" of scientific definitions.

3.1. Ontological Pluralism. Once we have seen that the interest-dependence of natural definitions does not threaten systematic realism we are in a position to employ the resources of Part 2 to examine a related issue about the philosophical plausibility of realism. Some philosophers (see, e.g., Putnam 1983b) have suggested that realism is committed to the highly implausible view that there is a single true theory--in a sense of that notion which implies that there is a single true way of "cutting the world at its joints" and thus a single true conceptual scheme. I have argued elsewhere (Boyd 1989a) that insofar as this conclusion rests on the (correct!) assumption that the contemporary realist should be a materialist (as it does in Putnam 1983b) it rests as well on a reductionist conception of materialism which the realist can, and indeed must, reject. We have just seen that another line of argument to the same conclusion is mistaken: it would be inappropriate to hold that the realist must deny the plurality of conceptual schemes which arises from the interest-dependence of natural definitions.

One final reason for thinking that realism is in trouble with respect to the question of the plurality of conceptual schemes is the following: Suppose that realist's are right in this: that the dictates of a particular scientific project require that scientists use a conceptual scheme which "fits" the world in some special way which is suitable to the project in question. Still it seems plausible that there may be a large, perhaps infinite, number of different ways of "carving up the world" which would equally satisfy the demands of any particular scientific project. Even the realist will have to acknowledge that the choice between these alternative conceptions is arbitrary or conventional; she must therefore abandon realism about natural kinds and other scientific categories, thereby defeating the broader realist project.

What we have seen in Section 2.3 is that the realist must be a "realist about" only those features of scientific theories which are central to reliable methodology. Thus the realist can quite coherently accept the pluralistic conception of scientific categories even within a single scientific discipline. There are "hairy' issues in analytical metaphysics raised by the pluralistic proposal we are considering, but this is clear: without abandoning anything central to coherent scientific realism the realist could acknowledge that for every particular scientific program there is an infinite plurality of appropriate conceptual schemes which fit the casual structure of the world equally well and between which the choice is arbitrary.

3.2. "Realism About", One More Time. Scientific Realism is apparently not the fragmented position which the "realism about" terminology would suggest. Why not? The answer suggested by the discussion on Part 2 is that two factors are responsible. First, an identifiable naturalistic account of methodology, independently identifiable as central to the case for scientific realism, affords us a standard by which to assess the acceptability from a realist point of view of acknowledgements of conventionality or reference failures in scientific theorizing. In the light of that standard, being a non-realist, on scientific grounds, about the ether, for example, or about higher taxa is no concession to anti-realism. Second, the naturalistic account of methodology and the arguments for realism which it underwrites are applicable across the range of the natural sciences. We are thus not faced with the serious prospect of there being "realism about physics" but not, e.g., "realism about chemistry" or "realism about biology". The wide applicability of the naturalistic account of methodology and the associated arguments for realism arises because of deep methodological similarities between the natural sciences, and because in each there is the history of unproblematical instrumental reliability of methods upon which the crucial argument for realism depends.

I propose a reform in the use of the expression "realism about". By "realism about" a subject area I propose to mean the doctrine that the characteristic intellectual achievement in that area involves the acceptance of statements which are, when understood literally, approximately true of a reality which is largely logically independent of the theories, conceptual schemes, research interests., etc. which one adopts. If we accept the largely uncontroversial doctrine that contemporary scientific theories are often literally about putative unobservable phenomena, then realism in this sense about the natural sciences is just scientific realism. Let us ask, in the sense of this reformed definition, "What must the (scientific) realist be a realist about?" The answer suggested by our discussion of the integrity of scientific realism is, "About those subject areas which (1) unproblematically share a common methodology with the natural sciences, and (2) unproblematically exhibit a level of instrumental reliability of method appropriate to the abductive argument for realism." For subject areas which fail to meet these two conditions, there may be deep considerations favoring realism but. prima facie, there is no reason why scientific realists are obliged to take these considerations much more seriously than other philosophers must.

I think that the considerations just rehearsed explain several features of the current dialectical situation with respect to "realisms about" (in the reformed sense). They explain, for example, why it seem possible to cogently accept realism about the natural sciences while denying it about at least some of the social sciences, where both the methodological similarities to the natural sciences and the level of instrumental success are controversial. They explain, as well, why scientific realists find it harder to deny realism about "cognitive science" than about other social sciences whose methods and records of instrumental success less closely resemble those of the natural sciences, and why the temptation to realism about mathematics is often greater when one focuses on mathematical theories in their scientific application than when one focuses on their more "pure" development. It likewise explains why scientific realist rarely feel compelled to be moral realists.

In saying that prima facie scientific realists need be realists only about those subject areas satisfying the two conditions above I mean to discuss the current dialectical situation of scientific realism vis-a-vis realism of other sorts. If certain naturalistic and anti-foundationalist features of much recent scientific realist philosophy come to be seen as central to scientific realism, as I think they should be, then scientific realists might be obliged assess realism about other subject areas in a more favorable light than (scientific) anti-realists. In particular, it is plausible that acceptance of certain naturalistic and anti-foundationalist principles which are arguably central to scientific realism greatly enhance the plausibility of moral realism (Boyd 1988; see also Brink 1984, forthcoming; Sturgeon 1984a, 1984b). But even when those principles are accepted moral realism emerges as a controversial empirical hypothesis about the history of moral discourse, one which a scientific realist could reject on empirical grounds without compromise.

3.3. Methodological Spectra. Arbitrariness or conventionality of theories comes in respects and degrees, and it has been fruitful here to specify the extent to which a theory is conventional by considering the range of alternatives to it with respect to which choice would be arbitrary or conventional. This "measure" of arbitrariness doe not, by itself, answer all of the the questions which might be put by asking, "how arbitrary is this theory?". It does not indicate what the methodological import is of the theory's respects of arbitrariness or non-arbitrariness. The epistemological equipotency doctrines discussed earlier suggest a way of assessing that import. By the methodological spectrum of a theory let us mean the class of methodological judgments which (given prevailing background theories) it properly underwrites. If the equipotency doctrines are right then two theories between which the choice is arbitrary by realist standards will, if properly understood, have the same methodological spectrum. In consequence, the claim that a theory is unexpectedly arbitrary in particular respects entails that its methodological spectrum is narrower than prevailing methods would suggest; competing claims regarding respects of arbitrariness will thus entail different conceptions of a theory's methodological spectrum

I think that it will prove important to applied philosophy of science to make explicit the connection between claims about arbitrariness and claims about methodological spectra. It will help, I believe, in formulating the methodology appropriate for assessing arbitrariness claims as they arise in actual scientific practice. For example, I have referred to cladist claims that higher taxa are unexpectedly arbitrary, and I have indicated that the theoretical reasons which appear to underwrite those claims are the sorts of considerations which are worthy of serious consideration. It seems to me, however, that it would help cladists and others to formulate those claims more perspicuously (and, I believe, more modestly) if the consequences of the equipotency doctrines were acknowledged.

Cladists often claim that the only non-arbitrary constraint on higher taxa is that they be monophyletic (that they consist of all the species which are the descendents of some particular species) and that their definitions should conform to the formal structure of the Linnean hierarchy. Should they claim this level of arbitrariness? Well, the theoretical claims which appear to underwrite cladism are claims about macroevolution (Guyot 1987). The literature on macroevolution is centrally concerned with the explanation of facts about the pace and tempo of evolution, and with the explanation of apparent evolutionary trends. Cladism apparently rests on a critique of standard macroevolutionary explanations which emphasize the role of natural selection, and upon the defense of a class of alternative explanations which place much less emphasis on selection.

One feature of the literature on macroevolution is that in assessing evidence about pace and tempo of evolution and about possible evolutionary trends evolutionary biologists routinely employ statistics defined in terms of higher taxa--comparing, for example, the rate of emergence of new classes or orders at different intervals in evolutionary history. It is a consequence of the equipotency doctrines that, if higher taxa are as arbitrary as the strongest cladist claims suggest, then these statistics are methodologically irrelevant. It is by no means clear that the case for cladism can survive so deep a methodological critique of the current literature. There are special reasons for cladists to formulate and defend their claims about the arbitrariness of higher taxa with much greater care, and the equipotency doctrines indicate just where the greatest care is needed.

This conclusion is, I hope, plausible on scientific grounds independently of any special philosophical reflections. This is so because many instances of the equipotency doctrines are uncontroversial methodological principles in everyday successful science. For the realist, of course, all of its instances are acceptable. The constructivist must somehow pick and choose. Whether that constructivist picking and choosing can be suitably justified is topic for another paper (Boyd 1988a).

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