practices in psychometrics rather than because of any underlying structure. Definitions of I.Q. (so that the normality of actually obtained measurements is an empirical generalization) provides some evidence that intelligence is such a trait. This is so because it can best be understood as ultimately and plausibly grounded in a particular theoretical conception of the mechanisms of macroevolution (for a defense of this position see Kipre 1971, 1972a, 1972b; Popper 1959). 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.

Quite similar treatments of the theoretical commitments which govern scientific research were of course achieved by philosophers in the constructivist tradition (see, e.g., Hanson 1899, Kuhn 1962); indeed the treatment of the special status of theoretical terms in Kuhn 1899 was so much in the manner embodied in Carnap 1950 that it is an interesting question how to tell Carnap from Kipre on an answer see Boyd 1899a, 1899b. 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.

Scientific realists hold (against social constructivism) that the normality of the theoretical structures whose existence and whose properties are independent 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 unknowable. 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 intentionally independent entities. According to the anti-realist strategy, however, such theoretical practices to be theory-independent (Boyd 1872, 1873, 1879, 1899, 1899a, 1899b, 1899c, 1899d, 1899e, 1899f, 1899g; Kipre 1899, Gomris 1899–1900–1904; Putnam 1897, 1897a, 1897b); for a general account of arguments for realism which appeal the observation see Boyd 1899, 1899a, 1899b).

1. Anti-realist and Conventionalism. A variety of anti-realist responses to arguments in favor of scientific realism are possible but one characteristic and pervasive anti-realist strategy has been to acknowledge that various scientific concepts and practices implicate theoretical knowledge but provide an interpretation of scientific language and concepts in terms of methodologically appropriate I.Q. testing. It has, moreover, been cited as 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 conventional (the sign of the charge of the electron, the unit of measurement for mass). As Putnam (1972, 1975a, 1975b) emphasized, it is an intended consequence of contemporary realist conceptions of kinds properties, and other theoretical aspects of the definitions of scientifically important categories are arbitrary or conventional (see also Kripke 1971, 1972; for a general account of arguments for realism which 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 (perhaps for some wholesale failures of reference). A non-realist reconciling defense of the non-reality of Planckian atomism is available.

There are other things than aren’t (by perhaps aren’t) for which realists must account. As Putnam (1897a, 1897b) 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"-notations (see also Kipre 1897, 1897b; Boyd 1899, 1899a, 1899b). 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 Planckian atomism). However, unless a non-realist can account for occasional cases of reference failure (perhaps for some wholesale failures of reference), a non-realist reconciling defense of the non-reality of Planckian atomism is available.

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 apparent truth) of some scientific generalization turns out to depend in unexpected ways on conventional conventions of methodological and linguistic practice). I am inclined to think that examples of such sorts of conventions are rather common, 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.Q. 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, conventional - non-positivist "rational reconstruction" see Guyot 1987). They are thus the sorts of paradigm cases of apparent natural kinds, then, are some which a realist might reason which a scientific realist must acknowledge but according to which these laws themselves are to be understood as reflections of linguistic conventions establishing the relevant scientific languages.

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indicate how the answer to that question generalizes naturally to the
of conventionality in science which a systematic realist can acknowledge, and I'll
2.
primarily concerned in the rest of the present essay.

much conventionality (and how much reference failure) can a systematic scientific
dictate acknowledgment of such cases. Of course there is a prior question, "How
whether historical and scientific facts and sound philosophical arguments ever
fragmented realism we need to ascertain
‑
order to assess the prospects for non
‑
realism. In
reference failure or of unexpected conventionality which a realist cold not
that we can understand what, according to cladists, is peculiar about higher taxa.
workable realist conception of how definitions of scientific terms ordinarily work
to (an appropriate interpretation of) cladism, has happened with respect to
such scenario should be expected on a realist conception of inquiry about a
character. Retrospectively it will be seen that the terminology in question exhibited
be appropriate to retain it, acknowledging its largely arbitrary conventional
discovered, it is plausible that the terminology in question should be understood as
(apparently) appropriate terminology and establish tentative definitions which
causally important but that they are mistaken, not with respect to individual

predict the occurrence of unexpectedly conventional features of scientific theories.
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conventionality pose no problems either.
scientific language and practice pose no special problems for the integrity of
explicit conventional features of
adequate understanding of why explicit and near

anti
realism.
‑
‑
realism about" and it's acknowledgment will count as a concession to systematic
concession by anti
realist empiricist
battles: that eventually these too will provide victories for the systematic anti
realism is most difficult. If there is no coherent realist conception of scientific
position suggested by the "realism about" terminology then this case is
unreal. Not only is there no coherent realist position to which concessions have
been made but each case of reference failure, or of unexpected
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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
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That this should be so is suggested by the best that scientific
realism-presuming that there is such a coherent and systematic position—seem to
pester the occurrence of unexpectedly conventional features of scientific theories. Realists, in
contrast, recognize no methods or techniques of definition, the

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This latter difficulty is compounded by another consideration. It is
reason will be the history of realist philosophy of science in a history of attempt of
any realist philosophers to scientific realism (I develop this Theme in Boyd 1988 and 1989b). 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 gap knowledge about observable (and/or naturalistic defined) entities. Surely it is a significant part of the
value of the fact that the particular cases in which 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
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particular component of a realist treatment scientific knowledge is to be assessed with regard to the question of its appropriateness (and competing anti-realist conceptions). I argue that, in general, such conceptions are not properly assessed in isolation but instead in terms of the extent to which they contribute to our ability to make predictions about scientific theories which are not reducible to empirical laws or relations, i.e., to the central methodological practices of science. The systematic realist, I suggest, is compelled to accept these arguments for realism about unobservable observables and to respond to them in a way which incorporates the two central core realist doctrines.

Thus, for example, a proposal by a realist to treat a particular feature of scientific theorizing as largely conventional (or to treat superficially theoretical terms as non-referential) 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. (The systematic realist, I suggest, is compelled to make genuine concessions to systematic anti-realism without the contribution of theoretical commitments upon which they depend is near ineliminable. The systematic realist, I suggest, is compelled to make genuine concessions to systematic anti-realism without the contribution of theoretical commitments upon which they depend is near ineliminable.

Against these and other realist arguments, it was for some time common for empiricists in the philosophy of science to deny that, as in a proper understanding—scientific practices and concepts are no-theory-dependent as they at first appear. Examples of empiricist responses explaining such devices were the defense of operationalism and related eliminativist analyses of theoretical terms in science, and the articulation of an alleged deep 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 understood in terms of the predictive success of some of them or deleterious "miracles" (Putnam 1978).

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 good for their respective philosophical mills. Thus, for example, van Fraassen (1984; 1985c; for important critiques see especially Fine 1984; Laudan 1981; 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 commitments from the methodologically central 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 good for their respective philosophical mills. Thus, for example, van Fraassen (1984) and Fine (1984) join the near consensus about the ineliminability of theoretical commitments from the methodologically central practices of even the most unproblematically scientific work. 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 the most unproblematically scientific work.
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 explorations 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 criterion for theory choice, 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 encoded in those theories. The epistemic centrality doctrine differs from the almost uncontroversial doctrine of the irreversibility of theoretical commitments in that it entails that the success of theory-dependent methodological practice is explained by background theoretical knowledge, and thus that knowledge of irreversibility 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 to constructivist conceptions of scientific knowledge. The irreversibility of theoretical commitments is every bit as central to constructive philosophy of science as to realist philosophy of science. Indeed it may be largely through the efforts of philosophers and historians of science that such a commitment has emerged as almost uncontroversial. Likewise, although the term “abductivist” seems much too naturalistic; the central abductive arguments for scientific realism against empiricism could seem equally available to the realist. This is due to the fact that the realist must be one in which the most fundamental laws in the relevant paradigms are the causal structure of the relevant phenomena. The discovery of the irreversibility of theoretical knowledge may as be central to constructive as to realist philosophical packages. We have yet to see upon what principle we can distinguish viable realist philosophical packages from those whose replenishment of various instances of “realism about” represents an ultimate 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 strategies, empirical statements, 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 the notion of theories, frameworks, etc. is a causal phenomenon and thus will contribute equally to it. The establishment of, for example, those causal factors which are explanatory in the history, philosophy and sociology of science. [Thus in the adoption of the scientific method and epistemological tradition, it is a revision of my former point which is the subject matter of the theory itself. All the realist denies is that there is sum further of contribution (empirical, societal, sociological, constructivist) or the legal adoption of theories, frameworks, and the like is made to the establishment of causal powers and relations. Realists affirm, and constructivists deny, the non-epistemic contribution. In the doctrine that the adoption of theories, paradigms, experimental or technical strategies, intellectual or practical interests, etc. makes no non-causal contribution to the causal structure of the world. We see in 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 non-causal contribution to the causal structure of the world. This is the second central core doctrine of scientific realism. The ineliminability of theoretical commitments is every bit as central to the philosophical centrality of theoretical knowledge may be as central to constructive as to realist philosophical packages. We thus have to see upon what principle we can distinguish viable realist philosophical packages from those whose replenishment of various instances of “realism about” represents an ultimate concession to constructivist anti-realism.

In the first place, the doctrine of the epistemic centrality of theoretical knowledge may as be central to constructive as to realist philosophical packages. We thus have to see upon what principle we can distinguish viable realist philosophical packages from those whose replenishment of various instances of “realism about” represents an ultimate concession to constructivist anti-realism.

In the second place, the doctrine of the ineliminability of theoretical commitments is every bit as central to the philosophical centrality of theoretical knowledge. The doctrine of the irreversibility of theoretical commitments is every bit as central to constructive as to realist philosophical packages. We thus have to see upon what principle we can distinguish viable realist philosophical packages from those whose replenishment of various instances of “realism about” represents an ultimate concession to constructivist anti-realism.

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 reliably accurate account of causal structures. In the “W” of theory-dependent methods to the actual causal structure of the world which explains their reliability. Two theories which between which the choice is arbitrary reflect the relevant causal structure in exactly equal and opposite ways.

The second core realist principle, the ineliminability of theoretical commitments which are themselves independent of such choices. Our question then is this: What principle we can distinguish viable realist philosophical packages from those whose replenishment of various instances of “realism about” represents an ultimate concession to constructivist anti-realism. The ineliminability of theoretical commitments is every bit as central to the philosophical centrality of theoretical knowledge. The doctrine of the irreversibility of theoretical commitments is every bit as central to constructive as to realist philosophical packages. We thus have to see upon what principle we can distinguish viable realist philosophical packages from those whose replenishment of various instances of “realism about” represents an ultimate concession to constructivist anti-realism.


We are now, I think, in a position to answer the question: How much conventionally can a scientific realist acknowledge? Notice that we are not asking how much conventionally a scientific realist should acknowledge—how much conventionally would it acknowledge in the best possible realist philosophical package. Nor are we asking what sort of conventionality, if acknowledged, would be significant concessions to systematic anti-realism (and thus support the implications of the “realism about” claim) and what sorts would not. We are asking what constraints respect for the central core realist doctrines puts on the assemblment of a realist philosophical package.

In the first place, the doctrine of the epistemic centrality of theoretical knowledge
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 stage in the construction of a successful research program, the realist will need to present the relevant background theoretical claims as *hypotheses* in a reflection of approximate knowledge, and will thus *stop short of* attempting to thereby commit to the approximate truth of those background theoretical claims. These background theoretical claims were obtained by appealing to the approximate truth of the background theories which determined large-scale 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; 1989a.]

What the equipollency principles (and the no non-causal contribution doctrine which underlies them) tell us is that, in the relevant realist explanations of the success of methods, respects of approximately 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/natural philosophical package only when, in the light of the equipollency 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 constant on realist attributes of conventionality. Realist acknowledgment of conventionality which don't conflict, given the actual conduct of science and in the light of the equipollency principles, with the doctrine of the epistemic centrality of theoretical knowledge may in principle as 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 in the case of the clearest assessment of higher laws, and to the extent that it succeeds by a *scientific* critique of the scientific community's acceptance to the relevant features of the theoretical claims in question—which results in a rebutation to the causal claim that the methods associated with those features are systematically reliable. With respect to features of theoretical claims which are not central to the methodological judgments directly or indirectly implicated in the methods by which apparently instrumental knowledge is obtained, the realist can affirm conventionality, provided that the burden is satisfied in an interesting but certainly without making concessions to systematic relativism.

What must a realist be a "realist about"? 1 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 One. How Does Reference Fail? How Does Reference Succeed? Let us turn now to the question of how much reference the scientific realist can acknowledge. Here the central question is: How is it that the realist can avoid the supposed concession to constructivism and anti-realism without being 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 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 apparently instrumental knowledge is obtained, the realist must *stop short of* portraying those features as approximate reflections of actual causal structures. Again as before, the realist can plausibly 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 the features and of the methodological judgments 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 not necessarily the only way. Some terms in a body of theoretical knowledge may be required to refer to only approximately true things, and may fail to enter into reference-free relation whatever: their introduction may represent deeply mistaken theoretical commitment. Even in such cases, statements-employing those terms may reflect important approximations to the truth; consider, for example, a deeply pluralistic 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 pursuing methodologically central theories as reliably approximately true that need not treat all of their constituent terms as given partially referring. What she must do is to portray them as being approximately true in respect 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 1989a, especially sections 3.3.3.4). This 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.

5. Interpretation 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 mean by a "kind" or "natural kind" can be seen as a subclassificatory program that treats the semantics of scientific language while still acknowledging even implicit and unexpressed conventionality in the definitions of scientific terms. In particular, where the no non-causal contribution doctrine holds, 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 false claim that in order to be an adequate, *scientific*, definition explanation of the methods by which apparent instrumental knowledge is obtained in well-developed successful sciences, one must think of as depending on the particular sort of natural phenomena under study, respects of similarity and difference may be causally significant. This rule is thus much more broad and relative than is realist conception that scientific study is largely non-independent reality and that they 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 epistemically compatible with a realist. To describe either the definitions of natural kinds, magnitude, etc., as 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 *spectra* in terms of the relevant sorts of similarity and difference (causal powers). That this is so does not, according to the realist, depend on all 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 appropriateness of methods for studying them, depend non-causally on actual study or actual interest or on any other candidate for "social reality." If this is so, then a "natural definition" of natural kinds suggests otherwise. There is no reason to suppose that the non-causal contribution doctrine would have to be abandoned in a coherent philosophical package which acknowledged the "intrinsic dependence" of scientific definitions.

3.1. *Dialectical Realism*. Once we have seen that the interest-dependence of natural definitions does not threaten systemic realism we are in a position to employ the resources of Part II to examine a related issue about the philosophically coherent package of realism. We 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 (Björk 1996) that the question on the *correct* assumption that the contemporary realism should be a materialist (as it does in Putnam 1983). It needs as well on a relativist conception of materialism which the realist can—indeed, must—reject. We have just seen that another line of argument to the same conclusion is maintain: it would be inapposite 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 realists are right in this view that the dictates of a particular scientific project require that scientists see 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 "cutting 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 conceptual schemes may depend as much on methodological grounds as on metaphysical grounds. It seems, for example, that the conceptual schemes which philosophers like to equate with natural kinds are "hairy" issues in analytical metaphysics raised by the pluralistic proposal we have made above is an instance of what I have called the "realism about the natural sciences". In particular, it is

3.2. *Relevant* About*. One More Type. Scientific realism is apparently not the manglement of the term "realism about" terminology would suggest. Why not? The answer suggested by the discussion on Part II is that two factors are responsible. First, an identifiable naturalistic account of methodology independently defendable as central to the case for scientific realism, whereas a "realist interpretation" of this 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 (Björk 1996) that the question on the correct assumption that the contemporary realism should be a materialist (as it does in Putnam 1983). It needs as well on a relativist conception of materialism which the realist can—indeed, must—reject. We have just seen that another line of argument to the same conclusion is maintain: it would be inapposite to hold that the realist must deny the plurality of conceptual schemes which arises from the interest-dependence of natural definitions.

I propose a reform in the use of the expression "realism about". By "realism about" a subject area (implying to mean the doctrine that the characteristic intellectual achievement in that area involves the acceptance of statements which are, understood literally, approximately true of a reality which is largely logically independent of the theories, conceptual schemes, research programmes etc. which one adopts. If one accepts the logically independent doctrine that contemporary science involves the acceptance of some reasonable standards of knowledge as a test of the rationality of the realist in the realist's sense of the contextual objectivism of scientific realism. Let us, in the sense of this reformed definition. "What must be the realist project?" The answer suggested by our discussion of the integrity of scientific realism is. About "those subject areas which (1) unproblematically share a common method 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, *see here*, there is no reason why scientific realists are obliged to take these areas into consideration also. It seems much more realistic to tell otherwise.

I think that the considerations just rehearsed explain several features of the current statistical situation with respect to "realism about" (in the reformed sense). They explain, for example, why it seems possible to cogently accept realism about the natural sciences while denying it about at least some of the social sciences, where both the methodological and formal similarities to the natural sciences and the level of instrumental success are controversial. They explain, as well, why scientific realists tend to deny realism about "social science" than about "natural science". It may be that the relative success of the former is a consequence of the fact that the social sciences are closely related to those of the natural sciences, and why the temptation to realism about mathematics or other sciences when one focuses on the several "pure" development. It likewise explains why scientific realism need feel compelled to be moral realism.

In saying that current scientific realism need be realists only about those subject areas which (1) unproblematically share a common method with the natural sciences, and (2) unproblematically exhibit a level of instrumental reliability of method appropriate to the abductive argument for realism one would be, the realist might be obliged accept realism about other subject areas in a more humble light than scientific anti-realism. In particular, it is plausible that acceptance of certain naturalistic and anti-naturalistic principles which are argued to be crucial to scientific realism greatly enhance the plausibility of moral realism (Björk 1988; see also Brink 1984, forthcoming; Skjevig 1984; 1986). But even when those principles are accepted realism emerges as a controversial empirical hypothesis about the history of moral dissonance, one which a scientific realist could reject on empirical grounds without compromising realism about the natural sciences.

3.3. *Methodological Spheres*. Arbritrariness or conventionality of theories comes in many degrees (Spectrum A), 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 does not, by itself, answer any of the five questions which might be put by asking "how arbitrary is this theory?". It does not indicate what the methodological import of the theory's respects of arbitrariness or non-arbitrariness. The methodological arbitrariness doctrine discussed earlier suggest a way of measuring the extent to which the realist is committed to the class of methodological principles which give presentations background features (spectrum A). The degree of arbitrariness is right that some theories between which the choice is arbitrary by realist standards 4. It 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 entail different conceptions of a theory's methodological spectrum.

I think that it will prove important to applied philosophy of science to make
exploited the connection between claims about arbitrariness and claims about empirical notions of equipotency. 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 rigid claims that higher taxonomic levels are unequivocally arbitrary, and have indicated that the theoretical reasons which appear to underlie those claims are the sorts of considerations which are worthy of serious consideration. It seems to me, however, that it would help cladists and cladistic critics to be more explicit about their assumptions, and to give them the same sort of methodological scrutiny to which the equipotency doctrines were acknowledged. Clausen and Lang claim that the only non-arbitrary constraint on higher taxonomic levels 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 cladist claims about macroevolution (Guyer 1987). The literature on macroevolution is certainly concerned with the exploration of facts about the pace and tempo of evolutionary change and with 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 taxonomic levels, for example, the rate of emergence of new classes or orders at different times in evolutionary history. It is in consequence of the equipotency doctrine that if higher taxa are as arbitrary as the cladistic cladist claims them to be, then the case for cladism can survive even a methodological critique of the cladistic methodology. The cladist must somehow pick and choose, whether that construction picking and choosing can be suitably justified is a topic for another paper (Boyd 1968b).

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Bibliography


