What Realism Implies and What it Does Not

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Summary

This paper addresses the question of what scientific realism implies and what it does not when it is articulated so as to provide the best defense against plausible philosophical alternatives. A summary is presented of "abductive" arguments for scientific realism, and of the epistemological and semantic conceptions upon which they depend. Taking these arguments to be the best current defense of realism, it is inquired what, in the sense just mentioned, realism implies and what it does not. It is concluded that realism implies the strong rejection of epistemological foundationalism, a non-Humean conception of causation and of explanation, and a causal rather than conceptual account of the unity of natural definitions. It is denied that realism implies bivalence or the existence of one true theory, one preferred vocabulary or one distinctly privileged science. It is further denied that realism implies that there are no unrecognized conventional aspects to scientific theorizing and it is denied that realism implies that scientists routinely do good experimental metaphysics.

Résumé

Ce papier traite de la question «Ce que le réalisme implique et ce qu'il n'implique pas» lorsque celui-ci est présenté comme la meilleure défense face à des alternatives philosophiques plausibles. Les arguments «abductifs» en faveur du réalisme scientifique sont passés en revue, ainsi que les conceptions épistémologiques et sémantiques dont ils dépendent. Prenant ces arguments comme la meilleure défense du réalisme, on examine ce que, dans le sens qui vient d'être précisé, le réalisme implique et ce qu'il n'implique pas. La conclusion est que le réalisme implique a) le net rejet du fondationalisme épistémologique, b) une conception non-huméenne de la causalité et de l'explication et c) une justification causale plutôt que conceptuelle de l'unité des définitions naturelles. On nie d) que le réalisme suppose la bivalence ou l'existence d'une seule théorie vraie, d'un vocabulaire préférable ou d'une science privilégiée. On nie également que le réalisme implique e) l'inexistence d'aspects conventionnels non reconnus dans la construction de théories scientifiques ou f) la pratique routinière, par les savants, d'une bonne métaphysique expérimentale.

Zusammenfassung

Dieses Papier behandelt die Frage, was wissenschaftlicher Realismus impliziert und was nicht, wenn er so dargestellt wird, dass er gegen plausible philosophische Alternativen geschützt ist. Es wird eine Zusammenfassung präsentiert von «entführten» Argumenten für den wissenschaftlichen Realismus und von den erkenntnistheoretischen und semantischen Konzeptionen, von welchen sie abhängen. Indem diese Argumente als beste gegenwärtige Verteidigung des Realismus aufgefasst werden, wird untersucht, was — im eben erwähnten Sinn — Realismus impliziert und was nicht. Es wird gefolgert, dass Realismus die starke Verwerfung des erkenntnistheoretischen Fundamentalismus, eine nicht-humesche Konzeption von Verursachung und Erklärung

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und eine eher kausale als konzeptuelle Beschreibung der Einheit natürlicher Definitionen impliziert. Es wird verneint, dass Realismus Bivalenz oder die Existenz einer einzigen wahren Theorie, eines einzigen ausgezeichneten Vokabulars oder einer deutlich privilegierten Wissenschaft impliziert. Es wird zudem verneint, dass Realismus impliziert, dass es keine unerkannten konventionellen Gesichtspunkte für wissenschaftliche Theoretisieren gebe, und es wird verneint, dass Realismus impliziert, dass Wissenschafter routinemässig gute experimentelle Metaphysik betreiben.

1. *Preliminaries*

1.1 *Realism* — Scientific realists hold that the characteristic product of successful scientific research is knowledge of largely theory-independent phenomena and that such knowledge is possible (indeed actual) even in those cases in which the relevant phenomena are not, in any non-question-begging sense, observable (Boyd 1983). The characteristic philosophical arguments for scientific realism involve a defense of the claim that a realist conception of scientific inquiry is required in order to justify, or to explain the reliability of, various unproblematically successful methodological practices which themselves reflect apparent commitments to knowledge claims regarding unobservables (Boyd 1983, 1985a, 1985b). It is with the implications of scientific realism that this paper is concerned.

1.2 *The Implications of Realism in a Dialectical Context* — Often when philosophers inquire about the implications of some doctrine, philosophical or otherwise, they are concerned to ascertain just what propositions are logically entailed by the doctrine in question considered in isolation. We have, however, a broader and more dialectical notion of implication. On this broader notion to inquire what a doctrine implies it to inquire what conclusions a defender of the doctrine must accept in order to offer an adequate defense of it against its plausible rivals. It is in this latter sense that I propose to explore the implications of scientific realism: I will ask what philosophical doctrines a scientific realist need (and need not) accept in order to fully develop the best available arguments for scientific realism.

Any exploration of the implications, in the broader dialectical sense, of any philosophical (or non-philosophical) doctrine must depend on some estimate of its dialectical setting, and in particular on some assessment of the relative merits of various argumentative strategies for defending it. I have argued elsewhere (Boyd 1982, 1983, 1985a, 1985b, 1985c, 1986, 1988) that the appropriate form for a defense of scientific realism involves arguing, about crucial features of scientific methodology, that their contribution to *instrumental* knowledge can be neither justified nor explained except on the presumption of a realist conception of *theoretical* knowledge. My strategy in the present paper will be to assume, for the sake of argument, that abductive arguments of that form are the most appropriate for the defense of realism, given the current dialectical setting, and to explore the implications, in the broader sense, of accepting them. Of course this will mean that someone who has a significantly different view of how scientific realism is best defended might reach a different conclusion regarding its implications, but such a possibility is built into the broader conception of implication upon which I am proposing to rely. Before exploring the implications of scientific realism it remains to specify, in very broad outline, the nature of the abductive arguments in question.

1.3 *The Abductive Argument for Scientific Realism*

1.3.1 *Objective Knowledge From Theory Dependent Methods* — By the "instrumental reliability" of a scientific theory I mean the extent of its capacity to make approximately true observational predictions — the extent of its approximate empirical adequacy. By the "instrumental reliability" of some body of methods I mean the extent to which their practice is conducive to the acceptance of instrumentally reliable theories. The abductive arguments for scientific realism take place in a dialectical situation in which scientific realists and their philosophical opponents largely agree that the methods of actual recent scientific practice are significantly instrumentally reliable. The disputants agree as well that — subject to appropriate philosophical reconstruction — we may *prima facie* accept as instrumentally reliable those methodological principles which the scientific community treats as central in practice (which is quite definitely not to say that we may accept whatever philosophical glosses scientists may put on those methods).

The abductive arguments for realism are in the first instance directed against the empiricist who denies the possibility of "theoretical" knowledge — knowledge of "unobservables". Against the empiricist the realist argues that only by accepting the reality of approximate theoretical knowledge can we adequately explain the (uncontested) instrumental reliability of scientific methods. Such an argumentative strategy, I suggest, reflects the central insights about scientific methods which have underwritten the philosophical tendency towards scientific realism in recent times.

Over the past three decades or so, philosophers of science within the empiricist tradition have been increasingly sympathetic towards scientific realism and increasingly inclined to alter their views of science in a realist direction. The reasons for this realist tendency lie largely in the recognition of the extraordinary role which theoretical considerations play in actual (and patently successful) scientific practice. To take the most striking example, scientists routinely modify or extend operational "measurement" or "detection" procedures for "theoretical" magnitudes or entities on the basis of new theoretical developments. The reliability of this sort of methodology is perfectly explicable on the realist assumption that the operational procedures in question really are procedures for the measurement or detection of unobservable entities, and that the relevant theoretical developments reflect increasingly accurate approximate knowledge of such "theoretical" entities. Accounts of the revisability of operational procedures which are compatible with an empiricist position appear inadequate to explain the way in which theory-dependent revisions of "measurement" and "detection" procedures make a positive methodological contribution to the progress of science.

If we look more closely at the example of the realist's explanation for the reliability of the methodology which permits theory-determined changes in measurement and detection procedures, we can recognize two important features of the realist's explanations for the reliability of scientific methods generally. In the first place, the realist's account of the theoretical revisability of measurement and detection procedures rests upon a conception of scientific research, when it is successful, as *cumulative by successive (but not necessarily convergent) approximations to the truth.* Approximately true theories dictate approximately reliable measurement and detection procedures for "theoretical entities" and the employment of those procedures leads (if all goes well) to the establishment of still more accurate (but typically only approximate) theories.

Secondly, this cumulative development is possible because *there is a dialectical relationship between current theory and the methodology for its improvement.* The approximate truth of current theories explains why our existing measurement procedures are (approximately) reliable. That reliability, in turn, helps to explain why our experimental or observational investigations are successful in uncovering new theoretical knowledge, which, in turn, may produce improvements in measurement techniques, etc.

These features of scientific methodology are entirely general: all aspects of scientific methodology — principles of experimental design, choices of research problems, standards for the assessment of experimental evidence and for assessing the quality and methodological import of explanations, principles governing theory choice, and rules for the use of theoretical language — are highly dependent upon current theoretical commitments (Boyd 1972, 1973, 1979, 1980, 1982, 1983, 1985a, 1985b, 1985c; Kuhn 1970; Putnam 1972, 1975a, 1975b; van Fraassen 1980). No aspect of scientific method involves the "presupposition-free" testing of individual laws or theories. Instead, the methods of science are profoundly theory-dependent and there is a pattern of dialectical interaction between accepted theories and associated methods of just the sort exemplified in the case of the theory-dependence of measurement and detection procedures. Moreover, this pattern of theory-dependence contributes to the reliability of scientific methodology rather than detracting from it.

According to the scientific realist's abductive argument, the only scientifically plausible explanation for the reliability of a scientific methodology which is so theory-dependent is that scientific methodology, dictated by currently accepted theories, is reliable at producing further knowledge *precisely because currently accepted theories are relevantly approximately true.* For example, it is because our current theories are approximately true that the canons of experimental design which they dictate are appropriate for the rigorous testing of new (and potentially more accurate) theories. What the scientific method provides is a paradigm-dependent paradigm-modification strategy: a strategy for modifying or amending our existing theories in the light of further research, which is such that its methodological principles at any given time will themselves depend upon the theoretical picture provided by the currently accepted theories. If the body of accepted theories is itself relevantly sufficiently approximately true, then this methodology operates to produce a subsequent dialectical improvement both in our knowledge of the world and in our methodology itself. Both our new theories and the methodology by which we develop and test them depend upon previously acquired theoretical knowledge. It is not possible, according to the realist, to explain even the instrumental reliability of actual scientific practice without invoking this explanation and without adopting a realistic conception of scientific knowledge (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c) and it is consequently impossible to adequately justify the use of scientific methods, even with respect to instrumental knowledge, if one rejects realism.

1.3.2 *Projectability, Evidence and Theoretical Plausibility* — The way in which scientific methodology is theory-dependent requires, if the realist's abductive argument is correct, rethinking our notion of scientific evidence. To a very good first approximation a theory receives significant evidential support from a body of successful predictions (or other evidentially favorable observations) just in case (a) the theory is itself projectable (in the sense of Goodman 1973), (b) the observations in question pit the theory's predictions (or, in other contexts, its explanations) against those of its projectable rivals, and (c) in the relevant experiments or observational settings there have been suitable controls for those possible artifactual influences which are themselves suggested by projectable theories of the relevant experimental or observational settings (Boyd 1982, 1983, and especially 1985a).

The realist's abductive argument depends crucially on the by now largely uncontroversial observation (see e.g., Kuhn 1970, van Fraassen 1980, Boyd 1972) that the methodologically relevant projectability judgments are, in fact, judgements of theoretical plausibility in the light of the received background theories. Projectability judgments reflect a strong methodological preference for new theories (and for accounts of possible experimental artifacts) which are plausible in the light of existing theoretical commitments: scientists prefer new theories which relevantly resemble existing theories (where the determination of the relevant respects of resemblance is itself a theoretical issue). According to the realist the reliability of this sort of preference is explained by the approximate truth of existing theories, and one consequence of this explanation is that *judgments of theoretical plausibility are evidential.* The fact that a proposed theory is itself plausible in the light of previously confirmed theories is evidence for its (approximate) truth (Boyd 1972, 1973, 1979, 1982, 1983, 1985a, 1985b, 1985c, 1988, 1989). Judgments of theoretical plausibility are best seen as matters of inductive inference from (partly) theoretical premises to theoretical conclusions (Boyd 1985a, 1985b) and it is precisely inferences of this sort which justify, and explain the reliability of, the typical pattern in science of "inductive inference to the best explanation" (Boyd 1985b).

1.3.3 *Natural Definitions* — One feature of the theory-dependent methods of science which is especially puzzling from an empiricist perspective and especially important for the abductive argument for realism is the apparent tendency of scientists to take the definitions of scientific kinds, properties and relations to be *a posteriori* and subject to theory-determined revisions rather than conventionally *a priori.* With respect to the reliability of this practice the realist's explanation involves a radical departure from received empiricist philosophy of language.

Locke speculates at several places in Book IV of the *Essay* (see, e.g., IV, iii, 25) that when kinds of substances are defined by purely conventional "nominal essences", as he thinks they must be, it will be impossible to have a general science of, say, chemistry. The reason is this: nominal essences define kinds of substance in terms of sensible properties, but the factors which govern the behavior (even the observable behavior) of substances are the fundamental properties of the insensible corpuscules of which they are composed. Since there is no reason to suppose that our conventional nominal essences correspond to categories which reflect uniformities in microstructure, there is no reason to believe that kinds defined by nominal essences will be apt for the formulation or confirmation of general knowledge of substances. Only if we are able to sort substances according to their hidden real essences will systematic general knowledge of substances be possible.

Realists agree. Only when kinds are defined by natural rather than conventional definitions is it possible to obtain the theory-dependent solutions to the problem of projectability just described (Putnam 1975a; Quine 1969a; Boyd 1979, 1982, 1983). It is thus central to the realist's abductive argument that most scientific terms be seen as possessing natural and *a posteriori* rather than conventional definitions. These terms are defined in terms of the (perhaps unobservable) properties, relations, etc. which render them appropriate to particular sorts of scientific or practical reasoning and the realist will appeal to the approximate accuracy of background theoretical beliefs in order to explain the reliability of the methods by which scientists establish and revise the definitions of scientific terms to achieve theoretical or practical appropriateness. [For more on naturalistic definitions see section 2.3]. Naturalistic theories of reference for such terms (Kripke 1971, 1972; Putnam 1979a, 1979b; Boyd 1979) complete the realist's conception of the semantics of scientific language.

With the outline of the abductive argument for scientific realism in hand, we are in a position to explore the question of what realism, so defended, implies. I'll consider here three philosophical doctrines which realism does imply and four that it does not.

2. *What Realism Implies*

2.1 *Realism Implies that Foundationalism is Profoundly False* — Modern epistemology has been largely dominated by positions which can be characterized as "foundationalist": all knowledge is seen as ultimately grounded in certain foundational beliefs which have an epistemically privileged position — they are *a priori,* self-warranting, incorrigible, or something of the sort. Other true beliefs are instances of knowledge only if they can be justified by appeals to foundational knowledge. Whatever the nature of the foundational beliefs, or whatever their epistemic privilege is supposed to consist in, it is an *a priori* question which beliefs fall in the privileged class. Similarly, the basic inferential principles which are legitimate for justifying non-foundational knowledge claims can themselves be shown *a priori* to be rational; it is moreover an *a priori* question about a given inference whether it meets the standards set by those principles or not.

We may fruitfully think of foundationalism as consisting of two parts, *premise foundationalism* which holds that all knowledge is justifiable from an *a priori* specifiable core of epistemically privileged foundational beliefs, and *inference foundationalism* which holds that the principles of justifiable inference are ultimately reducible to inferential principles which are *a priori justifiable* and whose application is *a priori checkable*.

Recent work in "naturalistic epistemology" or "causal theories of knowledge" (see, e.g., Armstrong 1973; Goldman 1967, 1976; Quine 1969b) strongly suggests that the foundationalist conception of knowledge is fundamentally mistaken. For the crucial case of perceptual knowledge, there seem to be (in typical cases at least) neither premises (foundational or otherwise) nor inferences; instead perceptual knowledge obtains when perceptual beliefs are produced by epistemically reliable mechanisms. Even where premises and inferences occur, it seems to be the reliable production of belief that distinguishes cases of knowledge from other cases of true belief. A variety of considerations suggests that there are no beliefs which are epistemically privileged in the way traditional foundationalism seems to require.

I have argued (see Boyd 1982, 1983, 1985a, 1985b, 1985c) that the abductive defense of scientific realism requires an even more thorough-going naturalism in epistemology and, consequently, an even more thorough-going rejection of foundationalism. In the first place, the fact that scientific knowledge grows cumulatively by successive approximations and the fact that the evaluation of theories is an ongoing social phenomenon require that we take the crucial causal notion in epistemology to be reliable *regulation* of belief rather than its reliable *production*. The relevant conception of belief regulation must reflect the approximate, social and dialectical character of the growth of scientific knowledge. It will thus be true that the causal mechanisms relevant to knowledge will include mechanisms, social and technical as well as psychological, for the criticism, testing, acceptance, modification and transmission of scientific theories and doctrines. For that reason, an understanding of the role of social factors in science may be relevant, not only for the sociology and history of science, but for the epistemology of science as well. The epistemology of science is in this respect dependent upon empirical knowledge.

There is an even more dramatic respect in which the epistemology of science rests upon empirical foundations. All of the significant methodological principles of scientific inquiry (except, perhaps, the rules of deductive logic, but see Boyd 1985c) are profoundly theory-dependent. They are a reliable guide to the truth *only* because, and to the extent that, the body of background theories which determines their application is relevantly approximately true. The rules of rational scientific inference are not reducible to some more basic rules whose reliability as a guide to the truth is independent of the truth of background theories. Since it is a contingent empirical matter which background theories are approximately true, the reliability and the justifiability of scientific principles of inference rests ultimately on a contingent matter of empirical fact, just as the epistemic role of the senses rests upon the contingent empirical fact that the senses are reliable detectors of external phenomena. Thus the first component of inference foundationalism is radically false; there are no *a priori* justifiable rules of non-deductive inference. The epistemology of empirical science is an empirical science. (Boyd 1982, 1983, 1985a, 1985b, 1985c). Similarly, assessment of the application of scientific methodology in particular instances is *a posteriori*: particular instances of scientific reasoning are sound just in case the background theories upon which they are based are themselves approximately true and the approximate truth of such theories is itself an *a posteriori* matter. Thus the second component of inference foundationalism is also false: the legitimate inferential principles in science are not *a priori checkable*.

One consequence of this radical contingency of scientific methods is that the emergence of scientific rationality as we know it depended upon the logically, epistemically, and historically contingent emergence of a relevantly approximately true theoretical tradition. It is not possible to understand the initial emergence of such a tradition as the consequence of some more abstractly conceived scientific or rational methodology which itself is theory-independent. There is no such methodology. We must think of the establishment of the corpuscular theory of matter in the 17th century as one component of the beginning of rational methodology in chemistry (its later temporary eclipse not withstanding), not as a consequence of it (for a further discussion see Boyd 1982).

2.2 *Realism Implies a Non-Humean Conception of Causation and of Explanation and a Non-Reductionist Conception of Materialism* — The Humean conception of causal relations according to which they are analyzable in terms of regularity, correlation or deductive subsumability under laws (where "lawlikeness" is syntactically or otherwise non-causally characterized) is defensible only from a verificationist position. If verificationist criticisms of talk about unobservables are rejected — as the realist conception of science requires — then there is nothing more problematical about talk of causal powers than there is about talk of electrons or electromagnetic fields. There is no reason to believe that causal terms have definitions (analytic or natural) in non-causal language. Instead, "cause" and its cognates refer to natural phenomena whose analysis is a matter for physicists, chemists, psychologists, historians, etc. rather than a matter of conceptual analysis. In particular, it is perfectly legitimate — as a naturalistic conception of epistemology requires — to employ un-reduced causal notions in philosophical analysis. (Boyd, 1982, 1985b, Shoemaker 1980).

One crucial example of the philosophical application of such notions lies in the analysis of "reductionism". Realism implies that there is no bar in principle to an empirical solution to many outstanding issues in metaphysics. In particular, given currently available evidence, realism implies that there is good reason to accept materialism. If materialism is correct, then *in some sense* all natural phenomena are "reducible" to basic physical phenomena. The (pre-philosophically) natural way of expressing the relevant sort of reduction is to say that all substances are composed of purely physical substances, all forces are composed of physical forces, all causal powers or potentialities are realized in physical substances and their causal powers, etc. This sort of analysis freely employs un-reduced causal notions. If it is "rationally reconstructed" according to the Humean analysis of such notions, we get the classic analysis of reduction in terms of the syntactic reducibility of the theories in the special sciences to the laws of physics, which in turn dictates the conclusion that all natural properties must be definable in the vocabulary of physics. Such an analysis is entirely without justification from the realistic and naturalistic perspective we are considering. Unreduced causal notions are philosophically acceptable, and the Humean reduction of them mistaken. The pre-philosophically natural analysis of reduction is also the philosophically appropriate one. In particular, purely physical objects, states, properties, etc. need not have definitions in "the vocabulary of physics" or in any other reductive vocabulary (see Boyd 1980, 1982).

The nomological-deductive account of causal explanation is just the Humean account of causation applied to the special case of causal statements involved in explanations. It is without merit even as a first approximation to an account of the paradigm cases of causal explanation in the sciences. In particular, our preference for explanatory theories is not justified (as the N-D account would have it) on the grounds that successful explanations are simply successful retrodictions from the explanatory theory, and thus logically indistinguishable from cases of experimental confirmation. Instead, typical explanations are theoretical accounts of the causal processes or mechanisms which brought or bring about the explained phenomena. A more general theory is said to be explanatory when its theoretical resources play a role in an independently confirmed account of this sort. It is rational to count the explanatory power of a theory as evidentially relevant in assessing its (approximate) truth because the independent confirmation of a theoretical account which employs the resources of a more general theory provides the basis for an inductive inference at the theoretical level to the conclusion that the more general theory is approximately true. Thus "inductive explanation to the best explanation" (Harman 1965) is simply a special case of the sort of theory-mediated inductive reasoning characteristic of scientific methodology. Like all cases of such reasoning, inductions to the best explanations depend for their justification and for their reliability on the relevant approximate truth of the background theories with respect to which the evidence for the accounts of causal mechanisms is judged and with respect to which the evidential import of independently confirmed explanatory accounts is assessed (Boyd 1985b).

2.3 *Realism Implies that the Unity of Natural Definitions may be Causal Rather then Conceptual and their Individuation Conditions may be Non-Extensional* — The sort of natural definition of substances in terms of corpuscular real essences anticipated by Locke is reflected in the currently accepted natural definitions of chemical kinds by molecular formulas ("water = H_2O"). Definitions of this sort specify necessary and sufficient conditions for membership in the kind in question. Recent *non*-naturalistic semantic theories in the "ordinary language" tradition have examined the possibility of definitions which do not provide necessary and sufficient conditions. According to various property-cluster or criterial attribute theories, some terms have definitions which are provided by a collection of properties such that the possession of an adequate number of these properties is sufficient for falling within the extension of the term. It is supposed to be a conceptual (and thus an *a priori*) matter what properties belong in the cluster and which combinations of them are sufficient for falling under the term. In so far as different properties in the cluster are differently "weighted" in such judgments, the weighting is determined by our concept of the kind or property being defined. It is characteristically insisted, however, that our concepts of such kinds are "open textured" so that there is some indeterminacy in extension legitimately associated with property-cluster or criterial attribute definitions. The "imprecision" or "vagueness" of such definitions is seen as a perfectly appropriate feature of ordinary linguistic usage, in contrast to the artificial precision suggested by rigidly formalistic positivist conceptions of proper language use.

I shall argue that — despite the philistine anti-scientism often associated with "ordinary language" philosophy — the property-cluster conception of definitions provides an extremely deep insight into the possible form of natural definitions — an insight which a realist conception of the methodological role of kind definitions implies we must accept. I shall argue that there are a number of scientifically important kinds, properties, relations, etc. whose natural definitions are very much like the property-cluster definitions postulated by ordinary-language philosophers (for the record, I doubt that there are any terms whose definitions actually fit the ordinary-language model, because I doubt that there are any significant "conceptual truths" at all). The natural definitions of these "homeostatic property cluster" kinds (etc.) involve a kind of property-cluster *together with* an associated indeterminacy in extension. Both the property-cluster form of such definitions and the associated indeterminacy are dictated by the (realistically understood) scientific task of employing categories which correspond to inductively and explanatorily relevant causal structures. In particular, the indeterminacy in extension of such natural definitions could not be remedied without rendering the definitions un-natural in the sense of being scientifically misleading. What I believe is that the following sort of situation is commonplace in the special sciences which study complex structurally or functionally characterized phenomena (I formulate the homeostatic property-cluster account of definition for monadic property terms for simplicity; the account is intended to apply in the obvious way to the cases of terms for polyadic relations, magnitudes, etc.):

(i) There is a family F of properties which are contingently clustered in nature in the sense that they co-occur in an important number of cases.

(ii) Their co-occurrence is, at least typically, the result of what may be metaphorically (sometimes literally) described as a sort of *homeostasis*. Either the presence of some of the properties in F tends (under appropriate conditions) to favor the presence of the others, or there are underlying mechanisms or processes which tend to maintain the presence of the properties in F, or both.

(iii) The homeostatic clustering of the properties in F is causally important: there are (theoretically or practically) important effects which are produced by a conjoint occurrence of (many of) the properties in F together with (some or all of) the underlying mechanisms in question.

(iv) There is a kind term *t* which is applied to things in which the homeostatic clustering of most of the properties in F occurs.

(v) *t* has no analytic definition; rather all or part of the homeostatic cluster F together with some or all of the mechanisms which underlie it provide the natural definition of *t*. The question of just which properties and mechanisms belong in the definition of *t* is an *a posteriori* question — often a difficult theoretical one.

(vi) Imperfect homeostasis is nomologically possible or actual: some thing may display some but not all of the properties in F; some but not all of the relevant underlying homeostatic mechanisms may be present.

(vii) In such cases, the relative importance of the various properties in F and of the various mechanisms in determining whether the thing falls under *t* — if it can be determined at all — is a theoretical rather than a conceptual issue.

(viii) Moreover, there will be many cases of extensional "vagueness" which are such that they are not resolvable even given all the relevant facts and all the true theories. There will be things which display some but not all of the properties in F (and/or in which some but not all of the relevant homeostatic mechanisms operate) such that no rational considerations dictate whether or not they are to be classed under *t*, assuming that a dichotomous choice is to be made.

(ix) The causal importance of the homeostatic property cluster F together with the relevant underlying homeostatic mechanisms is such that the kind or property denoted by *t* is a natural kind (see section 1.3.3).

(x) No refinement of usage which replaces *t* by a significantly less extensionally vague term will preserve the naturalness of the kind referred to. Any such refinement would either require that we treat as important distinctions which are irrelevant to causal explanation or to induction, or that we ignore similarities which are important in just these ways.

(xi) The homeostatic property cluster which serves to define *t* is not individuated extensionally. Instead, the property cluster is individuated like a (type or token) historical object or process: certain changes over time (or in space) in the property cluster or in the underlying homeostatic mechanisms preserve the identity of the defining cluster. In consequence, the properties which determine the conditions for falling under *t* may vary over time (or space), *while t continues to have the same definition*. The historicity of the individuation criterion for the definitional property cluster reflects the explanatory or inductive significance (for the relevant branches of theoretical or practical inquiry) of the historical development of the property cluster and of the causal factors which produce it, and considerations of explanatory and inductive significance determine the appropriate standards of individuation for the property cluster itself. The historicity of the individuation conditions for the property cluster is thus essential for the naturalness of the kind to which *t* refers.

The paradigm cases of natural kinds — biological species — are homeostatic cluster kinds. The appropriateness of any particular biological species for induction and explanation in biology depends upon the imperfectly shared and homeostatically related morphological, physiological and behavioral features which characterize its members. The definitional role of mechanisms of homeostasis is reflected in the role of interbreeding in the modern species concept; for sexually reproducing species, the exchange of genetic material between populations is thought by some evolutionary biologists to be essential to the homeostatic unity of the other properties characteristic of the species and it is thus reflected in the species definition which they propose (see Mayr 1970). The *necessary* indeterminacy in extension of species terms is a consequence of evolutionary theory, as Darwin observed: speciation depends on the existence of populations which are intermediate between the parent species and the emerging one. Any "refinement" of classification which artificially eliminated the resulting indeterminacy in classification would obscure the central fact about heritable variations in phenotype upon which biological evolution depends. More determinate species categories would be scientifically inappropriate and misleading.

Similarly, the property cluster and homeostatic mechanisms which define a species must be individuated non-extensionally as a process-like historical entity. This is so because the mechanisms of reproductive isolation which are fundamentally definitional for many sexually reproducing species may vary significantly over the life of a species. Indeed, it is universally recognized that selection for characters which enhance reproductive isolation from related species is a significant factor in phyletic evolution, and it is one which necessarily alters over time the species' defining property cluster and homeostatic mechanisms (Mayr 1970).

Equipped with an understanding of the abductive argument for realism and some of its important implications we are now in a position to see some of the things that realism does not imply.

3. *Some Things Realism Does Not Imply*

3.1 *Realism Does Not Imply Bivalence* — It is initially plausible to think that — since realists hold that there are theory-independent facts with respect to which correspondence truth is defined — realism must imply that every factual statement, or at least every adequately formulated factual statement, must be determinately either true or false. In fact, however, it follows from the homeostatic property-cluster account of natural kind definitions that a consistently developed scientific realism *predicts* indeterminacy in extension for those natural kind or property terms which refer to complex homeostatic phenomena. Such indeterminacy is a necessary consequence of "cutting the world at its (largely theory-independent) joints". Thus consistently developed scientific realism *predicts* that there will be some failures of bivalence for statements which refer to complex homeostatic phenomena (contrast, e.g., Putnam 1983a on "metaphysical realism" and vagueness).

Of course when there is a failure of bivalence resulting from the predication of a homeostatic property-cluster term the realist denies neither that there are facts of the matter regarding the homeostatic cluster property and the object of which it is predicated, nor that these facts are describable. What is important is that an appropriate description of the relevant facts regarding

indeterminate or "borderline" cases of homeostatic cluster kinds (properties, etc.) consists not in the introduction of artificial precision in the definitions of such kinds but rather in a detailed description of the ways in which the indeterminate cases are like and unlike typical members of the kind (see Boyd 1982 on borderline cases of knowledge, which is itself a homeostatic cluster phenomenon).

Interestingly, the homeostatic cluster conception of definitions permits a more perspicuous formulation of the central explanatory thesis of scientific realism upon which our current investigation is predicated. I have argued elsewhere (Boyd 1979, 1982, 1983) for an understanding of knowledge and of reference according to which (although I did not use this terminology) the relations 'x knows that y' and 'x refers to y' possess homeostatic property-cluster definition. Arguments similar to those presented in the case of species definitions above suggest that this is also true of the relation of copersonality for temporal stages of persons. I strongly suspect that this will prove true for a great many phenomena of philosophical importance. In particular, I suggest that scientific rationality (and, indeed, rationality in general) has a homeostatic property-cluster definition and that the realist's explanation for the reliability of scientific methods is best understood as the crucial component in an explanation of the homeostatic unity of scientific rationality. [This thesis is developed further in Boyd 1989].

3.2 *Realism Does Not Imply that there is One True Theory, One Preferred Vocabulary, or One Distinctly Privileged Science* — Consider the following construction. Let **L** be the class of all possible interpreted languages. For each L in **L** form the sublanguage L′ consisting of those sentences in L which have no free variables and no indexical expressions. Using whatever set-theoretic devices you like, alter the vocabularies of the sub-languages L′ so that no term occurs ambiguously either within the vocabulary of one of the sub-languages or within the union of the vocabularies of any pair of them. For each of the sub-languages L′ call the resulting reformed language L′*, and let T*(L) be the truth-set for L′*. Now let **T** be the union of the truth-sets T*(L). If the construction proposed is mathematically acceptable (if, e.g., **T** is not too "large" to have a cardinality, or if it can be constructed in some suitable framework for defining proper classes) and if for each language L truth is well-defined then, in a sense of "theory" which only a positivist could love, **T** is the one true theory. Realists do not differ systematically from non-realists in their scruples regarding set theoretic constructions, nor are they any less able than their opponents to indicate the many ways in which **T** isn 't a theory in any ordinary sense of the term (it addresses no natural class of questions, it employs no systematically unified body of concepts, it couldn't even remotely be the object of anyone's belief or contemplation, etc.). Realists it would seem are no more committed to the success of the construction in question, and thus no more committed to the existence of "one true theory", than any of their philosophical opponents who accept some notion of truth.

Putnam (1983b) thinks otherwise. For Putnam, and for many others, contemporary realism is connected intimately with contemporary materialism and I do not deny the connection: it is hard (for me at least) to see how a realist could plausibly deny that the currently available evidence strongly supports materialism. *If* we understand materialism as entailing the reducibility of the vocabulary and theories of the special sciences to the vocabulary and theories of physics then it would follow that, in the current dialectical setting, realism entails that there is good evidence that *via* translation reduction all theories in the special sciences are special cases of one or more of the various theories of physics (and thus that there is only one true theory, or at any rate that there are only a few). Recall however that the reductionist analysis of materialism according to which it entails the definability of all of the terms of the special sciences in the vocabulary of physics (and the reduction of the suitably translated laws of such sciences to the laws of physics) is a standard piece of logical empiricist Humean "rational reconstruction". If what I have said earlier about the Humean conception of causation is correct, realism implies that the reductionist analysis of materialism is fundamentally mistaken. There is no reason for the materialist realist to assign some special semantic role to the vocabulary of physics, nor for her to assign a corresponding reductive role to the laws of physics. For her there can be (in any ordinary sense) a great many different true theories.

Other considerations central to the realist project indicate that the realist need not — indeed should not — assign some privileged role to the findings or to the peculiar explanatory devices of physics itself. The claim that all natural phenomena are composed of the sorts of things which students of "fundamental physics" study provides no reason to believe that we can best find out about larger scale natural phenomena by studying the physics of their micro-constituents. Thus the materialist realist need not have any reservations about accepting the commonplace observation that reductive research strategies are only sometimes fruitful and are often misleading in the study of complex macroscopic phenomena.

Moreover, even when the apparent findings of some "special science" contradict the apparent findings of physicists, the materialist realist need not in every case hold that epistemic priority belongs to the physicists. Any judgement regarding priority would have to reflect an estimate of the current reliability

of the methodology within the relevant disciplines with respect to the particular issue in question rather than an estimate of the "metaphysical" priority (whatever that might be) of the objects they study. Arguably realists and others currently have good reason to hold the research methodology of fundamental physics in very high esteem but even in the present setting it is easy to imagine scenarios in which apparently well confirmed theories in, say, solar astronomy might be overthrown because of conflicts with evidence regarding the history of terrestrial weather obtained by such mundane types as geologists and paleontologists.

3.3 *Realism Does Not Imply that There are No Unrecognized Arbitrary or Conventional Aspects to Scientific Theories* — Central to much of traditional logical positivism and to neo-Kantian constructivism is the conception that many features of sound scientific description of the world rest on some sort of linguistic or social conventions and it has been a characteristic theme in realist philosophy of science to deny about certain pieces of scientific theorizing that they are largely reflections of such conventions. It would be a mistake however to think of the realist as denying that there are important conventions in the use of scientific language. Of course it is unproblematical that there are the obvious conventions: the choice of units for length, mass, etc. More importantly it is uncontroversial that, given a particular theoretical or practical project and the associated need for a vocabulary which reflects the relevant sorts of causal structures, there will be more than one way of "carving" up nature which satisfies that constraint and in consequence that there will be equally appropriate but conceptually different theories whose vocabularies reflect those alternative "carvings". One element of conventionality or arbitrariness in the linguistic scheme of a scientific discipline will be a reflection of the way of carving which was, as a matter of historical fact, actually established.

Lots of logically clever philosophers' examples illustrate this point but it can perhaps be seen more clearly from an ordinary example from biological taxonomy. Traditional evolutionary systematics (Mayr 1970) attempts to erect taxa above the species level which reflect causal factors important in determining the direction of macro-evolution. Supposing the conception of macro-evolution on which such taxa are based to be sound (for a discussion of this issue and of cladist challenges to evolutionary systematics see Guyot 1987) the taxa thus erected are natural kinds in the realist's sense. But, of course, there will still be features of those taxa which do not, on any plausible realist account, reflect causal structures relevant to the direction of macro-evolution. Surely, for example, the exact number of taxonomic levels in the currently employed Linnean hierarchy is not dictated by facts about such structures. In some significant respects the exact number of levels is arbitrary or conventional: an artifact of the actual history of biological classificatory practice and nothing more.

Of course the realist need not deny this conventionality. What we need to see is what the realist must say about the possibility of unobviously conventional aspects of scientific language: aspects which might well be taken to reflect the structure of the world rather than merely quite particular facts about the history of a particular piece of scientific language. Need she deny that such unobvious conventionality obtains in order to avoid adopting a tacitly empiricist or constructivist position?

The answer is "no". The realist differs from the constructivist in that (like the traditional empiricist in this instance) she denies, while the constructivist affirms, that the adoption of theories, paradigms, conceptual frameworks, perspectives, etc. 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. The realist does not deny (indeed she must affirm) that the adoption of theories, conceptual frameworks, languages, etc. is itself a causal phenomenon and thus contributes causally to the establishment of, for example, those causal factors which are explanatory in the history of science and of ideas. What she denies is that there is some further sort of contribution (logical, conceptual, socially constructive, or the like) which the adoption of theories, etc. makes to the establishment of causal powers and relations.

What then must the realist say about the possibility of alternative conceptual schemes between which the choice is arbitrary or conventional? All she need do is to hold that the establishment of one rather than another of the conventions in question does not non-causally contribute to the causal powers of, or causal relations between, the relevant objects of scientific study. In the easy case of the number of levels of the Linnean hierarchy, for example, in constructing her explanations of the reliability of biological research methods she must refrain from adopting a conception of approximation according to which taxonomic schemes which employ slightly different numbers of levels differ in the extent or dimensions of their approximation to the truth or according to which such schemes reflect different sorts of causal structures.

For harder cases realist's approach is the same. She need not deny that there are conventional or arbitrary aspects of scientific language which are not easy to recognize and which may appear to reflect real causal structures. Let us say that the choice between one or another of two theories or descriptive schemes (henceforth: conceptions) is arbitrary just in case such a choice would

reflect facts about linguistic conventions or about the particular history of language use within the scientific community rather than a difference in how well the conceptions reflect casual structures which are themselves independent of that choice. The realist need not deny that there may be features of received conceptions which are currently held to be reflections of real causal structure but about which it is true that there are alternative conceptions apparently reflecting different causal structures such that the choice between the received conceptions and those alternatives is in fact arbitrary. What is distinctive to the realist's treatment of arbitrary choices between conceptions is that what the constructivist sometimes affirms the realist must always deny: that the apparent differences in causal structure reflected in conceptions between which the choice is arbitrary should be thought of a corresponding to a real difference in (partly socially constructed) causal structures.

Where there are features of the received conceptions which appear to be reflections of real causal structures but are in fact artifacts of an arbitrary choice between conceptions, those scientists who accept the conceptions which embody them will warrantedly take those features to be methodologically significant — to be relevant to judgements about, e.g., projectability or explanatory power. Similarly philosophers of a realist bent, commenting on the methods of those scientists and sharing their conceptions, may reasonably appeal to the (approximate) truth of those conceptions, and in particular to the features in question, in explaining the reliability of the relevant scientific practice. What the realist must hold regarding such cases, is that the scientists and philosophers in question would be (non-culpably) mistaken: the methodological recommendations which are peculiar to the received conception (in contrast to those alternatives with respect to which choice is arbitrary) will be reliable, if at all, only accidentally and the realist's explanation of the reliability of scientific practices will thus be defective to the extent that it cites as causally relevant those dimensions of approximation to the truth which are similarly peculiar to the received conception.

The possibility we are abstractly considering here may be actual with respect to the case of biological taxonomy. Cladists challenge evolutionary systematics and their challenge is best regarded (narrowly positivist statements by cladists notwithstanding) as resting on a critique of the evolutionary systematists' theory of the mechanisms of macroevolution (Guyot, op. cit.). If cladists are right in their criticisms then the requirement that causal factors in evolution be reflected in classification imposes only a very weak constraint (that taxa be monophyletic) on biological taxonomy and thus most of the features of received taxonomies are unexpectedly arbitrary. On the realist view, any theory-determined methodological or inferential principles peculiar to received taxonomies (as opposed to those with respect to which the choice is arbitrary) will be only accidentally reliable and any philosophical explanations for reliability or justifiability of biological methods which appeal to what is peculiar to those taxonomies will, in that respect, be defective. There may be surprising truths by convention in science but what is arbitrary or conventional, even when true, does not reflect a distinctive feature of causal structure. It is properly available neither for scientific nor for philosophical explanation. [For the historian of recent empiricism there is the interesting question of whether the later views of Carnap, say his position in "Empiricism, Semantics and Ontology" (Carnap 1950), should be counted as sophisticated logical empiricism or as early constructivism. I suggest that the question is whether or not Carnap is there, like the realist, committed to denying that linguistic conventions contribute non-causally to causal powers and relations. I suspect that the answer is "yes" and that late Carnap should be thought of as an empiricist still.]

3.4 *Realism Does Not Imply that Scientists Routinely do Good Metaphysics* — Logical positivists employed the term "metaphysics" for the sort of inquiry about "unobservables" which verificationism led them to reject. Most of what has traditionally fallen under that term was 'metaphysics' in the positivists' sense, but so was inquiry about, e.g., the atomic structure of matter. If scientific realism is right then it follows that scientists routinely do successful 'metaphysics'. With respect to metaphysics (as philosophers and others ordinarily use the term) the situation is more complex.

If scientific realism is true for any of the standard reasons then scientists have discovered the real essences of chemical kinds (Kripke 1971, 1972) and have thus done some real metaphysics. Moreover, the fact that scientific knowledge of unobservables is possible makes it a serious question whether or not scientific findings have (or will have) resolved some traditional metaphysical questions. Certainly the recent near consensus in favor of a materialist conception of mind reflects a realist understanding of the possibility of experimental metaphysics. Nevertheless it does not follow from scientific realism that scientists routinely tend to get the right answers to the distinctly metaphysical questions which are the special concern of philosophers even when their methods lead them to adopt theories which reflect answers to such question.

In particular, when a realist proposes to explain the reliability of scientific methods at a time as a consequence of the approximate truth of background theories she need not hold that the metaphysical conceptions they embody are a good approximation by philosophical standards. Two examples will illustrate the point.

Consider the way in which the reliability of the methods by which Darwin's account in the *Origin* was assessed is to be explained by reference to the approximate truth of much of the prevailing background biological theory. A great deal was known, for example, about species — not just facts about particular species but about anatomical, behavioral, genetic and biogeographical generalizations which can only be formulated in terms of the notion of a species. The realist will hold that the approximations to the truth embodied in this lore of species is part of what explains the reliability of the research methods in biology employed by Darwin and his contemporaries.

Prior to Darwin's work the prevailing conception made species membership, in the first instance, a property of individuals; after Darwin we have correctly seen a species as, in the first instance, a family of populations. The background biological theories of Darwin's era got it profoundly wrong about the metaphysics of species. Nevertheless, the classificatory practices of pre-Darwinian biologists were reliable enough to serve to establish the rich and significantly accurate lore about species upon which reliability of methodology in early evolutionary theory crucially depended — or, at any rate, so the realist may reasonably maintain.

Similarly, the realist will want to explain the reliability of the methods by which physicists assessed early developments in quantum theory by appealing to respects in which the pre-quantum theory of, say, atoms and sub-atomic particles was approximately true. She will appeal to the correct identification of various sub-atomic particles and of (many of) the fundamental physical magnitudes, to the availability of reliable procedures for the detection of those particles and for the measurement of various of their physical properties, and to the classical insights reflected both in the formulation of the equation for the time-evolution of quantum-mechanical systems and in the techniques employed in practice in picking the appropriate Hamiltonian for quantum mechanical systems.

Indeed much of the early development of quantum theory took place as the gradual extension of the range of phenomena for which an adequate quantum-mechanical treatment had been provided. At any given stage in the early development of quantum theory the proposed models for physical systems were always a mixture of distinctly quantum-mechanical components together with essentially classical (or relativistic) components awaiting later quantum-mechanical reformulation. The realist will want to explain the reliability and justifiability of this sort of development by appealing to the respects of approximation to the truth of classical mechanics itself and of the successive stages in the development of the quantum theory.

Consider now the classical conception of atomic phenomena understood as a contribution to philosophical metaphysics. Arguably the metaphysical component of that conception is some sort of mechanistic atomism: a picture of discrete and unproblematically individuated particles and their associated fields interacting in a deterministic fashion without action at a distance. Our current quantum mechanical conception of matter rejects each component of this picture: for the atomist's discrete particles we substitute entities with wave-like features for which particle-like individuation is sometimes impossible; we reject determinism; and we acknowledge that there are non-local effects which would surely be precluded by the classical philosophical rejection of action at a distance. Classical conceptions of the atomic world were, let us agree, poor approximations to the truth in metaphysics. Does this preclude their having been good enough approximations in other respects to sustain the realist's account of the development of quantum theory?

Plainly not. Whatever other objections there may be to the realist's account, it is not a cogent objection that the classical conception which her account treats as relevantly approximately true is not good metaphysics. All she need do is to explain how the metaphysical errors in the classical conception failed to vitiate the methodological contribution of its genuine insights. To this end she might, e.g., appeal to the respects in which subatomic particles are (classical) particle-like, to the determinism of the time-evolution of quantum-mechanical systems prior to measurement, and to the wide variety of phenomena which do not significantly exhibit the effects of non-local "action at a distance". Perhaps in the case of the development of evolutionary theory and certainly in the case of quantum mechanics the realist's account will have scientists doing 'metaphysics' with some significant success; in neither case must she portray them as doing good metaphysics.

The cases just discussed illustrate an additional point. In each case, if the metaphysical criticism of the earlier theoretical tradition is sound, then that tradition embodied, in addition to metaphysical errors, errors about the logical form of certain key propositions. Conspecificity is a relation between populations, not between individuals, so that pre-Darwinian biology embodied a mistake about the logical type of propositions regarding species membership. Similarly, quantum mechanics requires that we think of the classically acknowledged physical magnitudes as corresponding to Hermitian operators rather than to vector or scalar valued functions; in consequence classical mechanics is mistaken about the logical form of, e.g., attributions of position or momentum to particles. Neither error undermines the contribution which the approximate truth of the earlier theory is said to have made to the methodology by which the latter theory was developed and confirmed. The realist need attribute to successful background theories neither metaphysical success nor logical exactitude. Approximation need not be philosophically clean. [Note that the distinctly realist naturalistic semantic conceptions are operative in this discussion. What evolutionary theory and quantum mechanics have taught us is that, as we might say, "there are no classical species" and "there are no classical particles". Only naturalistic alternatives to the empiricist conceptions of definitions and reference permit the realist to say — as the account just given requires — that nevertheless Darwinian species and the particles-like phenomena acknowledged by quantum mechanics were the subjects of the relevant classical investigations.]

4. *Final Remarks: Realism, Pluralism and Unity of Knowledge* — One feature of the conception of scientific realism which I have been defending here is that it is highly non-reductionistic. Considerable conceptual and methodological autonomy for the various special branches of human inquiry is a consequence of non-reductionist realism rather than a challenge to it. If I am right then many philosophers who have been tempted to anti-realism by a fear of the "imperialism of physics" are mistaken about who their natural allies are. Sometimes however they are not. Let me explain.

Positivists often accepted the conception of "unity of science" to which they always gave a reductionist gloss, but the idea they hoped to capture was of a non-reductionist, methodological principle of unity of knowledge: the legitimate findings of any of the special branches of human inguiry are potentially relevant to the assessment or to the application of the findings of any of the others. In the present dialectical context realism implies this methodological principle; in particular it implies that the findings of any particular branch of inquiry are *prima facie* accountable to the methodological requirement that they be inductively plausible given the best established findings of the rest of our disciplines. Disciplinary autonomy is not absolute. In particular, since realism in the present context implies that there is good evidence for materialism, the realist's understanding of unity of knowledge entails that on the best available evidence one should hold that atheism is true and traditional religious theories are false and the same is true for, e.g., quasi-mystical theories of nationality and the like. Philosophers who look to anti-realist pluralism to avoid these conclusions do correctly diagnose their dialectical situation. Those who look to anti-realism merely to defend the integrity of the special sciences and the humanities probably do not.

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