

Realism, Anti-Foundationalism and the Enthusiasm for Natural Kinds

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REALISM, ANTI-FOUNDATIONALISM AND THE
ENTHUSIASM FOR NATURAL KINDS

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0. THE TRADITION OF NATURAL KINDS

0.1 *Hacking's Challenge*

In his interesting and challenging paper “A Tradition of Natural Kinds” Professor Hacking identifies a long-standing empiricist tradition of theorizing about natural kinds, he identifies its roots as nominalist and its main concern as the characterization of the sorts of kinds and kind definitions appropriate for the formulation of reliable inductive generalizations. Hacking distinguishes a number of sub-traditions within the tradition of natural kinds but what is central to all of them is the conception that a natural kind is one such that it is (i) (at least on a certain idealization) defined by a set of necessary and sufficient properties (relations, etc.) such that, (ii) the possession of these properties is, as a matter of fact rather than of logic, indicative of a very large number of other methodologically interesting properties and such that, (iii) these defining properties are natural rather than social properties.

When the properties defining a kind satisfy condition (ii) then it is a *fact about nature* that the kind in question exists and a *fact about nature* that reference to the kind in question is appropriate for the formulation of causal generalizations related to the property correlations indicated in (ii).

Hacking points out that this conception of natural kinds was modest in scope: although the sub-traditions differed on just which kinds are the natural kinds all agreed that they constituted only a small fraction of all the kinds there were — a small fraction even of those non-arbitrarily defined kinds which are *natural categories* in some important sense of “natural”.

By contrast, recent work on natural kinds — especially work under-

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taken by realists in the philosophy of science — has reflected a much grander conception. Almost all sorts of kinds and kind terms except the most clearly arbitrary have been treated as natural kinds and kind terms at least in the sense that the naturalistic conceptions of definitions and of reference developed for the case of more traditional natural kinds and natural kind terms are understood to apply to them as well.

Hacking challenges the new enthusiasm for the theory of natural kinds. He suggests, in particular, that kinds which fail to satisfy the first of the traditional conditions on natural kinds (as property cluster kinds do) or which fail to satisfy the third condition (as social kinds do) should not be assimilated to a conception of natural kinds however “natural” the categories they correspond to may otherwise be. In neither case apparently is the existence of the kinds in question a fact *about nature* in the relevant respects.

Against the assimilation of the first sort of kinds Hacking argues that kinds of that sort “. . . are constructed along lines of family resemblance and what puts things into a family is not nature but people in concert.” He also argues that the relevant family resemblances will characteristically include “plainly social” as well as “natural” properties, causing most property cluster kinds to violate as well the stricture represented by the third condition.

In defense of that condition — which is to say against treating socially defined kinds as natural kinds — Hacking offers considerations which go to the heart of our conception of a natural kind as a fact of nature. He suggests that the complex feedback loops which connect our classification of, and theorizing about, people with changes in their self-concept and behavior and back again make it inappropriate to think of socially defined kinds as kinds defined *by nature* rather than by us. Realists often talk about employing natural kind terms to “carve *nature* at *its* joints.” In the case of socially defined categories, the causal powers of the objects classified — and thus the location of the “joints” — depends on our own acts of classification and theorizing rather than *on nature*.

What I propose to do in the present paper is to defend the enthusiasm for natural kinds against Hacking’s criticisms. I agree that there are important philosophical and methodological differences between kinds defined by property clusters and kinds defined by sets of

properties, and between natural properties and social properties. I believe however that there are extremely good reasons for treating many property-cluster kinds and social kinds, and the terms which refer to them, on the model of natural kinds and natural kind terms. In particular I hold, about any property cluster kind or social kind reference to which is important in explanation or induction, that three claims will be true of it which represent the central core of the current conception of natural kinds. First, the definition of the kind in question is determined *a posteriori* rather than by social convention. Second, this in turn is true because the use we make of reference to the kind in induction and explanation requires that it be defined by a set or cluster of properties whose membership is determined by the causal structure of the world and is thus, in a relevant sense (which I propose to specify), independent of our conventions or our theorizing. Finally, the establishment of a relation of reference between a term and the kind in question depends on the existence of an epistemically relevant pattern of causal relations between instantiations of the kind and the use of the term in question *and not on the acceptance by any or all of the members of the relevant linguistic community of the correct definition of the kind.*

In order to defend the assimilation of property cluster and social kinds to the model of natural kinds I'll need to offer a brief and (only slightly) anachronistic account of the history of the tradition of theorizing about natural kinds.

0.1. *Locke on Projectability, or the Tension Between Nominalism and Rational Induction*

Hacking maintains, correctly, that the tradition of research into natural kinds is largely nominalistic. What he does not say but what is uncontroversial is that the tradition is nominalistic because it is basically verificationist: the nominalism in question is an outgrowth of the traditional empiricist skepticism about the possibility of knowledge *real essences, hidden mechanisms, causal powers* or other “unobservables”. Hacking also rightly emphasizes the fact that the tradition is concerned with identifying those kinds appropriate for induction (and, I would add, explanation).

There is something else which is also uncontroversial but which Hacking does not emphasize: that the tradition of natural kinds reflects (at least in its empiricist stages) a deep tension between the nominalistic project of eliminating “metaphysics” and the project (equally central to empiricism) of characterizing rational principles for induction. That there is some such tension can be seen by reflecting on the fact that the central component of the tradition of natural kinds is *not compatible with traditional nominalism*.

I take traditional nominalism to be reflected in the Lockean position that while nature makes things similar and different the classification of them into kinds is solely “the workmanship of the understanding.” According to this central empiricist conception of nominal definitions such definitions are almost entirely arbitrary. For Locke, for example, the only permissible constraints on the idea of a mixed mode is logical consistency of the ideas united to form it; for an idea of substance there is the additional constraint that the qualities and powers corresponding its constituent ideas must be conjointly realizable in nature. Subject, at most, to these constraints “. . . each abstract idea, with a name to it, makes a distinct Species.”

According to the tradition of natural kinds, in both its empiricist and its realist versions, much deeper non-conventional constraints are appropriate: it is a fact *about nature*, rather than about the understanding, that natural kinds exist and have the definitions they do. Likewise it is a *natural* fact about a particular natural kind that reference to it is appropriate for certain sorts of inductive generalization and this natural fact is explained by property correlations which obtain independently of the understanding. If some kinds are features *of nature itself*, and if the principles of classification which define them fit the demands set by our projects *and by nature itself* better than others, then those kinds are at least as much the workmanship of nature as of the understanding. The traditional nominalist doctrine that kinds are merely the workmanship of the understanding is deeply compromised.

I see the tension between empiricist nominalism and the task of accounting for induction first arising in Locke’s *Essay*. As Locke acknowledges at several points (see, e.g., IV, iii, 13, 14, 25, 29; IV, xii, 9, 10) his nominalistic conception of kind definitions dictates a skeptical conclusion regarding the possibility of general knowledge of

substances. If the observable properties of substances are in fact determined by the primary qualities of their insensible parts, as Locke supposes, then inductive generalizations will be unjustified unless we are justified in believing that the categories in which they are framed correspond to uniformities in corpuscular structure: If all observed samples of kind K have exhibited causal power P *in virtue of their underlying microstructure*, then we will thereby obtain reason to believe that K's generally have P only if we have reason to believe that other samples of K will have relevantly similar microstructure. If, as Locke insists, we must classify substances according to arbitrary nominal essences instead of according to microstructural real essences, we never have any such reason and general knowledge of substances is impossible.

It is difficult to be certain just how deeply Locke takes the skeptical difficulties concerning general knowledge of substances which his theory of ideas of substance dictates; in some places he seems to be genuinely pessimistic about chemical knowledge; in others he seems oddly optimistic about the fruits of guessing and reasoning by analogy. What is important is that the skeptical force of his argument remains even when we're only guessing: if we have no reason to guess that K's all have relevantly similar microstructure (as we have not if the nominal essence defining the kind K is arbitrary) then we have no reason to guess that properties observed K's have displayed in the past they will continue to display upon further examination. Even for the purposes of guessing we need categories of substance whose boundaries are not (or not just) "the workmanship of the understanding."

Locke recognizes this and several related embarrassing consequences of his nominalism and at several places he suggests as a remedy that substantial kinds should ideally be defined so that their boundaries are set, not solely by the workmanship of Man, but in accordance with observed correlations of properties — ". . . That Men, versed in physical Enquiries, and acquainted with the several sorts of natural Bodies, would set down those simple *Ideas*, wherein they observe the Individuals of each sort constantly to agree" (III, xi, 25; see also IV, xii, 14; II, xxix, 7). Officially in these passages Locke is addressing the problem of the "inadequacy" of kinds of substances rather than the problem of inductive categories, but (for reasons which lie beyond the

scope of this essay) I am inclined to think that he means the solution to apply to the latter problem as well. I thus believe that these and similar passages in Locke represent the earliest stages of the empiricist tradition of philosophizing about natural kinds to which Hacking refers. At least they represent the earliest appearance within empiricism of the *anti-nominalist* conception that in defining kinds we should defer to naturally occurring patterns of property correlations.

The tension in Locke between his anti-metaphysical nominalism and the need to account for ordinary inductive methods itself has a structure which is repeated again and again in the empiricist tradition. Locke's nominalism arises from his broader proto-verificationist skeptical critique of "real essences": they are not possible objects of knowledge and are thus unavailable to us as the basis for classification. This skepticism was meant to be *selective*: it was not meant to spill over into a denial of the possibility of everyday knowledge of substances or even of something like general knowledge of their properties. What Locke recognized is that if skepticism about real essences is understood to entail the doctrine that substantial kinds are arbitrary — are *just* the workmanship of the understanding — then the resulting skepticism fails to be selective. Locke's appeal to what we would now recognize as a conception of natural definitions may have been — and later empiricist theories of natural kinds definitely were — attempts to restore the selectivity to the empiricist skeptical critique of real essences: if the verificationist critique of real essences does not demand that substantial kinds have entirely arbitrary nominal definitions — but only that their definitions not be framed in terms of insensible real essences — then the underlying skeptical conception can perhaps be suitably selective after all.

We may characterize Locke's philosophical situation in anachronistic but nonetheless useful terms. He was concerned which the characterization of *projectable* properties of substances, in the sense of Goodman 1973, and was faced with an argument which suggested that judgments of projectability in chemistry would have to depend to the sort of *theoretical knowledge* which his *verificationism* precluded. The alternative to embracing extreme skepticism about induction or accepting a realist conception that theoretical knowledge is possible which he

perhaps embraced (and which latter participants in the tradition of natural kinds certainly did embrace) was to propose that the specification of projectable properties proceed by *second-order induction about induction* in the sense of Quine 1969. It is thus proposed to preserve the verificationist critique of metaphysics at the expense of abandoning a strictly nominalist conception of kinds.

0.2. *20th Century Developments, or, The Theory-Dependence of Almost Everything*

Except for suggesting a somewhat earlier date for its beginning I agree with Hacking's characterization of the development of the tradition up to the point at which the controversial enthusiasm for natural kinds emerged. That enthusiasm has arisen largely among philosophers committed to scientific realist rather than empiricist conceptions of science. To see why, we need to recognize that philosophical examination of the methods of actual science has led, in the last few decades, to the confirmation of what might have been Locke's worst nightmare. In the first place, of course, general and systematic knowledge of substances is certainly possible. Now the bad part: *all* of the fundamental methods by which that knowledge is obtained are profoundly theory-dependent: principles of classification, methods for assessing projectability and for assessing the quality and the evidential import of observations, standards for assessing explanatory power, etc.

Scientific realism has gained considerable credibility as a result of the recognition of these facts about scientific practice. It seems possible to argue that inductive inferences in science *about observables* are reliable only because they are guided by methodological principles which reflect previously acquired (approximate) knowledge of unobservable real essences. The articulation of a naturalistic conception of the definition of natural kinds and a related naturalistic conception of reference has proven essential to making this abductive case for realism (Putnam 1975a, b; Boyd 1979, 1982) so it is hardly surprising that scientific realists have enthusiastically participated in articulating the tradition of natural kinds.

It remains to see whether the extension of that tradition in which

realists have played a large role is justified. I hope to show that, whether one accepts a realist or an empiricist diagnosis of the theory-dependence of method, there are very good reasons for extending the conception of natural kind to include all property-cluster or social kinds reference to which plays any significant role in induction or social explanation.

0.3. *Life After Theory-dependence: Global Nominalism or Epistemic Naturalism*

Locke's theory of nominal essences emerged as 20th century operationalism and failed for just the reasons Locke might have feared: scientific methods of classification are profoundly theory-dependent and kind definitions are thus revisable in the light of new knowledge. There is an ineliminable *a posteriori* element in the definition of any scientifically significant kind or category. Empiricist responses to this and other consequences of the theory-dependence of methods fall broadly into two categories.

Many empiricists moved from the *local nominalism* represented by the operationalist conception that *individual scientific term definitions* are purely conventional to varieties of *global nominalism* according to which various higher level propositions are true by convention and in turn govern theory-dependent practices like the revision of kind definitions. When methodological practices depend irretrievably on theoretical principles apparently reflecting *a posteriori* knowledge of unobservables, the global nominalist "rationally reconstructs" certain of the relevant principles as analytic statements, L-truths for the relevant scientific language L.

This strategy for rational reconstruction is reflected, for example, in the view that, while particular kind definitions are not *a priori*, certain "bridge laws" or "correspondence principles" linking theoretical and observational terms are matters of linguistic convention. It is likewise reflected in the still more global conception of Carnap 1950 according to which it is the most fundamental laws within a scientific discipline which are to be thought of as L-truths, and in reconstructions according to which various features or categories of scientific terms or sentences are treated as being defined *a priori*: lawlikeness, for example, or the

notion of being a term or a law “of physics” as that notion is employed in traditional reductionist reconstructions of materialism.

Global nominalism, if it works, eliminates from our conception of scientific practice an apparent reference to “metaphysical” knowledge of unobservables and it does so in an essentially Lockean way: where rational scientific practice would seem to require knowledge of unobservables the relevant judgments are recast as having *a priori* conventional justifications.

The other empiricist strategy for accommodating the deep theory-dependence of scientific method is reflected in the tradition of natural kinds. Where methodological practices (definitions of scientific terms for example) appear to depend upon *a posteriori* knowledge of unobservables, they are reconstructed as depending on *a posteriori* claims not about unobservables but about property correlations or other methodologically relevant but non-metaphysical matters. Theory-dependent methods are thus diagnosed as a matter of second-order induction about induction (see Quine 1969).

Note that the first of these strategies cannot be applied to all cases of theory-dependence of methodological practices. This can be seen by considering the deepest strategic problem facing global nominalism. The global nominalist responds to the theory-dependence of methods by treating certain methodologically important laws or generalizations which are apparently *a posteriori* claims about unobservables as conventional and hence *a priori* instead. Since *any* law or generalization whatsoever may play a role in determining *some* methodological practices or other, she must assure herself, and her critics, that she will not be driven to treat as *a priori* all of the apparently *a posteriori* laws and generalizations of science. The methodological insights of the laws or generalizations which are not reconstructed as *a priori* must then be portrayed as reflections of inductions about inductive methods. Each particular version of global nominalism will embody a different solution to this problem but any version of global nominalism will necessarily be part of a mixed strategy for the rational reconstruction of theory-dependent methods. What is central to the new enthusiasm for natural kinds is a certain estimate of the kind of mix which is dictated by facts about the actual methods of science. It is to the justification of that estimate that we now turn our attention.

1. NATURAL KINDS AND THE
NATURALISTIC EPISTEMOLOGY OF SCIENCE

1.0. *The Failure of Foundationalism*

The recent enthusiasm for natural kinds arose from the realist insistence that a realist interpretation is required for all of the theory-dependent methods of science including the methods of kind definition. Without accepting this claim — in particular without precluding the possibility of a sophisticated empiricist treatment of those methods — we can draw some important epistemological conclusions from an examination of the one of the most central of the abductive arguments for realism. It goes like this:

1. In actual scientific practice a theory, T, is substantially supported by a body D of data if and only if, (a) T itself is *projectable*, (b) the data in D confirm the predictions of T or are explicable on the basis of T, (c) for each significantly *projectable* theory T' which is an alternative to T there are in D data which either refute predictions of T' or cast significant doubt on its explanatory resources, and (d) the experiments or observational situations in which the data in D were obtained embody adequate controls for those possible experimental or observational artifacts which are themselves suggested by *projectable* theories about the relevant circumstances.
2. Projectability judgments are almost always judgments of *theoretical plausibility*. A theory is projectable just to the extent that what it says about both unobservable and observable phenomena coheres appropriately with the best confirmed background theories: it is projectable just in case it is supported by plausible inductive inferences from the “observational” and “theoretical” claims embodied in previously well established theories.
3. Projectability judgments depend on the *theoretical content* as well as the observational content of background theories: In general a theory diagnosed as projectable with respect to one of two empirically equivalent sets of background theories will not be empirically equivalent to any of the theories diagnosed as projectable by the other.

4. Projectability judgments are always *empirically revisable* in the light of revisions to background theories necessitated by new data or new theoretical insights.
5. Therefore projectability judgments are *a posteriori* judgments whose contribution to the reliability of scientific methods depends on knowledge reflected in the background theories. [From premises 1, 2, 4]
6. That knowledge goes beyond the empirical content of the relevant background theories and is “extracted” by reasoning which has just the form of inductive inference from partly “theoretical” premises. [From 2, 3, 5]
7. Therefore the best account of projectability judgments is the realist one according to which those judgments reflect inductive inferences from previously acquired approximate knowledge of unobservable as well as observable phenomena.

What is important for our purposes is that, through step six, this argument is acceptable to most sophisticated empiricists as well as to realists. The crucial idea in step 4 that important methodological practices are determined by empirically revisable features of accepted theories has been accepted by empiricist philosophers of science for a long time: the same point about measurement procedures did, after all, lead to their abandonment of operationalism. Moreover the empiricist analysis of theory-dependent methods as reflections of second-order inductions about induction is ideally suited to an empiricist reconstruction of the role of background theories in determining projectability judgments: the knowledge reflected in background theories which goes beyond their empirical adequacy is merely a matter of those background theories also being repositories of empirical knowledge about the reliability of inductive methods.

The premises of the argument we are considering are extremely well established. It is thus reasonable to suppose that the most plausible versions of empiricist philosophy of science will accept steps 1–6 while adopting something like the reconstruction just mentioned as an alternative to step seven. It is on this assumption that I will defend the claim that the recent extensions by realists of the traditional empiricist

conceptions of natural kinds are warranted by empiricist as well as by realist standards.

Modern epistemology has been dominated by foundationalist conceptions of knowledge. We may make explicit a fundamental but often tacit assumption of foundationalist epistemology if we think of foundationalism as consisting of two parts. *Premise foundationalism*, which is typically made explicit, holds that all knowledge is justifiable from a core of epistemically privileged foundational beliefs. *Inference foundationalism*, which is less often made explicit, holds that justifiable principles of inductive inference are ultimately reducible to inferential principles which are justifiable *a priori*.

Recent work in naturalistic epistemology (e.g., Goldman 1967, 1976; Quine 1969) strongly suggests that the foundationalism is fundamentally mistaken. In typical cases of perceptual knowledge, there seem to be neither premises nor inferences. Even where premises and inferences are important, it seems to be their contribution to the reliable production of belief that constitutes their contribution to knowledge. A variety of considerations suggests that there are no beliefs which are epistemically privileged in the way traditional foundationalism seems to require.

The response to the theory-dependence of projectability judgments which is common to scientific realism and to sophisticated empiricism suggests that foundationalism is even more profoundly mistaken. If, as that response suggests, all inductive reasoning rest upon *a posteriori* projectability judgments, then *inference foundationalism* is profoundly false. For the case of general knowledge of natural phenomena we must always rely on inferential principles which lack *a priori* justification. This *radical contingency* in epistemology is, I shall suggest, the crucial fact — agreed upon both by realists and sophisticated empiricists — which determines the proper scope of the theory of natural kinds.

1.1. *Radical Contingency and Natural Kinds*

Early theorists of natural kinds recognized that there were certain important circumstances in which it was necessary, in order to have the resources for formulating successful inductive generalizations, to deploy

categories defined *a posteriori* in such a way as to reflect the actual causal structure of the world. As we have seen, in those cases in which reference to them *is* necessary, natural kinds reflect a strategy of deferring to nature in the making of projectability judgments: we define such kinds *a posteriori* in ways which reflect actual causal structure precisely because we are unable to identify or specify projectable generalizations without doing so. Our question in the present essay is how widespread are the cases which thus require deference to nature.

On certain traditional and plausible conceptions of induction the answer might be, “Only rarely”, or “Only in immature sciences”. It might, for example, seem that a kind of rough and ready deference to nature with respect to projectability judgments would be necessary only in the earliest stages of scientific investigation before *a priori* justifiable statistical techniques become available to ground such judgments. What we have seen however is that the emerging consensus in the theory of induction indicates that the answer is “Always.” Projectability judgments are always *a posteriori*, so we should *always* require the sort of semantic machinery indicated by the theory of natural kinds when our aim is induction or explanation.

This is, if I am right, the basic consideration favoring the recent extrapolations of the traditional theory of natural kinds. Note that the extrapolation is justified by both sophisticated empiricist and sophisticated realist conceptions of induction. Realists and empiricists can even agree on a realist-sounding formulation of their common insight about kinds and induction. Kinds useful for induction or explanation must always “cut the world at its joints” in this sense: successful induction and explanation always require that we accommodate our categories to the causal structure of the world. Of course the empiricist has a Humean conception of the reference to causal structure whereas the realist has a metaphysical one. Nevertheless it is a non-trivial fact that the same formula can be used to describe each conception. The differences in interpretation of the formula in question correspond precisely to the difference between the distinctly realist treatment of the epistemology of the theory-dependence of method and the sophisticated empiricists’ induction-about-induction account of the same phenomenon. What the two conceptions have in common the formula captures precisely: that in induction and explanation we must refer to

kinds whose definitions are specified *a posteriori* in deference to nature rather than nominally.

1.2. *The Scope of the Expanded Theory of Kinds*

What should the lesson be for the question of the scope of the theory of natural kinds? At least *prima facie* the realist and the sophisticated empiricist should be led to conclude that that scope should be quite broad: that in defining a kind we should be required to defer to the world just in case *and to the extent that* reference to the kind in question is to be part of an inductive or explanatory project. In cases in which our concerns are largely with the establishment of workable conventions for non-inductive practice, deference to the world should be largely unnecessary.

Thus, for example, the definition of the kind “gene” should possess few conventional features whereas the definition of the kind “fish fork” should be largely arbitrary. It likewise follows that there should be kinds and categories whose definitions combine naturalistic and conventional features in quite complex ways. Consider the notion of having been legally married in New York State. Complex psychological notions like consent, deeply linked to induction and explanation, are involved in the legal definition of marriage, but so are notions with a high degree of arbitrariness — like being a properly ordained member of the clergy. In consequence the proposal we are considering dictates that we should expect that the definition of the legal notion in question should have, as it does, a mix of conventional elements and of elements in which a deference to the world is reflected. It follows that extensions of the traditional account of natural kinds should be appropriate just to the extent that the kinds in question are employed for induction and explanation — just as the expansionist realism to which Hacking objects maintains.

2. THE THEORY OF NATURAL KINDS EXTENDED

2.0. *Property-Clusters and Natural Kinds*

Hacking maintains, following the empiricist tradition, that natural kinds

should be defined by a set of necessary and sufficient conditions. He holds that in the case of kinds for which this condition fails — property cluster kinds — what unites the properties in the cluster is human social decision rather than nature, so that the extension of the theory of natural kinds to property cluster kinds is inappropriate. It is an interesting question whether or not there are property-cluster kinds such that the unity of the relevant properties is exclusively or mainly conventional. What is important for our purposes is that there are a great many property-cluster kinds for which the opposite is the case: kinds such that the unity of the property-cluster which defines them is *causal rather than conceptual*.

What I have in mind is something which should be congenial to the general project of the traditional theory of natural kinds. On that conception a natural kind is associated *causally* with a large family of methodologically important properties. Even if the kind is thought of as being defined by a set of necessary and sufficient conditions its naturalness is a reflection of a wider sort of property correlation. It is natural to inquire whether in defining some kinds we might defer more fully to nature and take the kind in question to be defined by the larger family of correlated properties rather than by any special sub-set singled out as providing the necessary and sufficient conditions. If this possibility is acknowledged then it is reasonable to inquire whether there may be kinds so defined except that the relevant property correlations are not perfect, so that the set of correlated properties functions as a property-cluster.

I have argued elsewhere (Boyd 1988, 1989, 1990) that this possible situation is actual for an important family of natural kinds. I 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 except that the unity of the properties in the defining cluster is mainly causal rather than conceptual. The natural definition of one of these *homeostatic property cluster kinds* is determined by the members of a cluster of often co-occurring properties and by the (“homeostatic”) mechanisms that bring about their co-occurrence. It is an *a posteriori* theoretical question which of these properties and which of the homeostatic mechanisms count, and to what extent they count, in

determining membership in the kind. In cases of imperfect homeostasis in which some of the properties in the cluster are absent or some of the mechanisms inoperative it will sometimes happen that neither theoretical nor methodological considerations assign the object being classified determinately to the kind or to its complement, with the result that the homeostatic property-cluster definition fails to specify necessary and sufficient conditions for kind membership. Both the property-cluster form of such definitions and the associated indeterminacy are dictated by the fundamental epistemic task of employing categories which correspond to inductively and explanatorily relevant causal structures. In particular, the indeterminacy in extension of these natural definitions could not be remedied without rendering the definitions unnatural in the sense of being scientifically misleading.

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 to be essential to the homeostatic unity of the other properties characteristic of the species and it is thus reflected in species definitions. 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 and would be scientifically inappropriate and misleading.

Thus some paradigmatic cases of natural kinds, playing the paradigmatic role of specifying categories apt for induction and explanation in science, are counterexamples to the claim that — at least on an ideal rational reconstruction — natural kinds must be defined by necessary and sufficient conditions. I conclude that the requirement that natural kinds have such definitions is to be diagnosed as a holdover from traditional empiricist conceptions of linguistic precision which must be

abandoned once it is agreed that kind definitions must conform to the (sometimes messy and complex) causal structure of the world.

2.1. *Social Construction and the Metaphysics of Social Kinds: The Metaphysical Innocence of Social Practice*

Should the theory of natural kinds also, *contra* Hacking, be extended to cover social kinds? We have seen that there are considerations acceptable both to realists and to sophisticated empiricists which suggest that such an extension is necessary for social kinds involved in induction or explanation.

Against these considerations there are two plausible counterarguments, each suggested by Hacking's insistence that the adoption of definitions for social kinds has a real but inscrutable influence on the properties of the social objects under study. According to the first, the contemplated extension of the theory of natural kinds gets the metaphysics wrong: social kind definitions must indeed be accommodated to actual causal structure but the dependence of the causal structure of social phenomena on our social theories and (especially) on our definitions of social kinds makes the causal structure which those definitions must match theory-dependent or mind-dependent a way which neither the realist nor the empiricist accounts of natural kinds can handle.

The second counterargument is epistemological rather than metaphysical: to be sure social kinds appropriate for induction and explanation would require naturalistic definitions, but the dependence of social structures on our conceptions of them is so profound and so far beyond our comprehension that in some appropriately deep sense induction and explanation about social matters are impossible. I shall be concerned with the first of these counterarguments in the present section.

Scientific realism is often contrasted with various social constructivist conceptions by saying that realists affirm, while constructivists deny, that the reality scientists study is "mind independent" in the sense that it is independent of their theories and conceptions. Certain phenomena scientists study, especially but not exclusively social phenomena, have some properties which depend causally on aspects of social practice which include the construction of theories about those very phenomena. In this sense, they are not "theory-independent".

Proposed definitions of scientific realism are often designed to accommodate this fact, which realists do not deny. Often realism is said to imply that subject matter of science is “*largely* theory-independent”. This terminology suggests that if the causal theory-dependence of the objects of investigation in some science is much greater or deeper than expected, then realism regarding that science is compromised. On such an interpretation of realism, the influence of our classificatory practices on the properties of the social phenomena we classify might refute realism regarding social inquiry and thus undermine the extension of the realist conception of natural kinds to the case of social kinds. Since empiricists also treat reality as theory-independent, the extension of an empiricist version of the theory of natural kinds would also be excluded.

I have discussed this interpretation of realism at some length elsewhere (Boyd 1990). What I conclude there, and urge here, is that it is a mistake to see realists (and empiricists) as differing with constructivists about the *extent* of the theory (or mind) dependence of reality. Instead, the disagreement is over the *nature* of that dependence. Realists and empiricists need not deny that the adoption of theories (schemes of classifications, etc.) may have a profound *causal* effect on the subject matter of those conceptions. What they deny is that the adoption of such conceptions makes any *non-causal*, logical or conceptual contribution to the causal powers of the objects of study. For both the Carnap of “Empiricism, Semantics and Ontology” (1950) and the Kuhn of *The Structure of Scientific Revolutions* (1970) the adoption of certain theoretical conceptions (L-truths, paradigms) can result in the truth of certain high level theoretical claims “about atoms.” For Kuhn and other social constructivists what is involved is the actual social construction of the causal properties of atoms. The world chemists live in (or at least the world they study) embodies certain *quite general* sorts of causal phenomena because chemists have adopted the paradigm they have.

For Carnap, by contrast, as for other empiricists and for realists, human social practices, like the adoption of theories and classificatory schemes, are *metaphysically innocent*: they affect the causal structure of the world only *via* the operation of intermediary causal mechanisms which supervene on the causal structures studied by the various special

sciences and not also in some additional way studied only by philosophers practicing conceptual analysis. The worlds which would be studied by chemists who accepted different paradigms would not differ in *general* causal structure and would differ *specifically* only in ways which supervene causally on the differences in the resulting social practices. Note that it is no part of a realist or an empiricist analysis to deny (or affirm) the inscrutability of the details of that causal supervenience.

It is beyond the scope of this paper to defend the metaphysical innocence of social practices. Suffice it to say here that (1) it is an assumption central to both the realist and the empiricist traditions within which the extension of the theory of natural kinds is contemplated, (2) it appears to be underwritten by fundamental conceptions of causal relations common to all of the established sciences, and (3) it appears itself to be required to underwrite some pretty obviously correct methodological principles regarding the role of conventions in science (Boyd 1990). Tentatively accepting metaphysical innocence principle, I conclude that the influence of classificatory practices on the actual properties of the objects of study in the social sciences does not compromise the realist (or empiricist) conception of properties upon which the proposed extension of the theory of natural kinds depends.

2.2. *Classificatory Causation and the Possibility of Social Knowledge*

It remains to ask whether the causal influence of classificatory practices makes genuine induction and explanation impossible in social inquiry. Might the influence of our classificatory practices be so great and so inscrutable that our very attempts to classify social phenomena will render them unstudyable? If the metaphysical innocence of social practices is assumed then for an important class of social explanations and generalizations the answer must clearly be “no.”

The metaphysical innocence principle entails that the influence of classificatory practices on causal structure always supervenes on ordinary causal mechanisms. If this is assumed then we can be certain that our current classificatory principles in history *do not* influence the properties which the objects of our historical studies exhibited when they were extant. To the social constructivist philosopher it might seem

that the properties exhibited by past events and persons are constituted by our current theorizing, but this possibility is precluded by metaphysical principles common to the two philosophical traditions within the extension of the theory of natural kinds is contemplated. I conclude that, barring a successful defense of social constructivism, the project of extending the theory of natural kinds to historical kinds is secure.

It might still be argued that the effects of classificatory practices are so great and so inscrutable as to preclude non-historical social knowledge, or perhaps that they are so inscrutable that even historical knowledge is precluded, not by the inscrutability of the effects of our current classificatory practices, but by the inscrutability of the social effects of the classificatory practices current during the historical periods we study. It is beyond the scope of this essay to discuss this proposal at length but I suggest that the burden of proof would lie strongly on someone who held, for example, that the unknown social effects of classificatory practice render problematical all generalizations about the effects of unemployment on wage levels in market economies.

I do not mean to deny that the effects of classificatory practices are socially or politically or methodologically important. I am sure the opposite is the case. What I doubt is that those effects are so pervasive and inscrutable as to make social knowledge impossible. If social knowledge is possible then quite general arguments, acceptable to the realist and the sophisticated empiricist alike, dictate that we extend the traditional conception of natural kinds to social kinds as well.

2.3. *Social Construction, One Last Time*

One response to all of the forgoing argumentation might be to suggest that the theory of natural kinds — as developed in recent realism or sophisticated empiricism — deserves extension to all those areas of knowledge to which empiricist or realist, as opposed to constructivist, interpretations are appropriate, but to hold that it is characteristic of social inquiry that it, unlike inquiry in the physical or (some of) the biological sciences, requires a constructivist interpretation. A *selective constructivism* reflecting such a view seems to be an emerging theme in the thinking of many who reflect on the ideological determination, as well as the ideological impact, of social inquiry (and some biological

inquiry as well). I think that there are deep reasons why this approach is unpromising.

I have argued elsewhere (Boyd 1990) that by “realism about” a subject area we should mean the doctrine that the characteristic intellectual achievement in that area involves the acceptance of statements which reflect, when understood literally, approximate knowledge of a reality which is logically (conceptually, etc.) independent of the theories, conceptual schemes, research interests, etc. which are adopted. In other words, we should understand realism about an area of inquiry as the doctrine that it produces knowledge to which the principle of the metaphysical innocence of social practice applies.

The proposal to depart from realism (or empiricism) in favor of constructivism in the special case of social inquiry would then involve denying the metaphysical innocence doctrine for the special case of social theorizing while retaining it with respect to the effects of other sorts of social activity. This is a deeply unpromising prospect. The justification for the metaphysical innocence doctrine lies in very general and very deep features of our scientific conception of causation. To accept the doctrine for almost all cases of social practice but to hold, for instance, that historical theorizing can create causal relations between past historical events, would be to adopt a position of very doubtful coherence.

One last point should be made regarding constructivism about social inquiry. Sometimes when philosophers or others hold that social inquiry is a matter of “the social construction of reality” they are best understood as holding not that social inquiry typically produces genuine knowledge of real but socially constructed causal relations, but instead as holding that the most typical product of social inquiry is not knowledge at all but ideology ratifying existing social patterns. It goes with this conception of some area of social inquiry that the definitions of certain of its terms must be purely nominal (“Jewish physics”, “the British national character”, “I. Q.”). If the distinction between realism and empiricism on the one hand and constructivism on the other lies in the acceptance or rejection of the metaphysical innocence doctrine, then this critique of social inquiry as ideological, far from being constructivist in the philosophical sense, must rest on an empiricist or realist foundation; it is furthermore compatible with the claim that the theory

of natural kinds should be extended to whatever social kinds are implicated in genuine social knowledge, however rare it may be.

It is likewise true that the contemplated extension of the theory of natural kinds is compatible with the recognition that, in cases where social knowledge is possible and where, in consequence, kinds must have *a posteriori* natural definitions, ideological and other social factors will often be a source of error in our efforts to formulate such definitions. I conclude that nothing in the methodologically and politically very important social embeddedness of our classificatory practices precludes the extension of the theory of natural kinds to social kinds — an extension in whose favor there are, in any event, deep and independent epistemological arguments.

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