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Homeostasis, Species and, Higher Taxa¹

Richard Boyd

0. Introduction.

0.0. Overview. In this paper I identify a class of natural kinds, properties and relations whose definitions are provided, not by any set of necessary and sufficient conditions, but instead by a "homeostatically" sustained clustering of properties or relations. It is a feature of such homeostatic property cluster kinds (properties, relations, etc.; henceforth I'll use "kinds" as the generic term wherever that will not cause confusion) that there is always some indeterminacy or "vagueness" in their extensions.

I introduce the notion of accommodation between conceptual and classificatory practices and causal structures and explain why the achievement of such accommodation is necessary for successful induction and explanation. I defend the view that the naturalness (and the "reality") of natural kinds consists solely in the contribution which reference to them makes to such accommodation. In the light of this accommodation thesis I explain why reference to "vague" homeostatic property cluster kinds is often essential to successful inductive and explanatory practice in the sciences.

I deploy these notions to address some aspects of the "species problem" in the philosophy of biology. I conclude that biological species are paradigmatic natural kinds, their historicity and lack of sharp boundaries notwithstanding.

Regarding the alternative conception that species are individuals, I examine the individuation of individuals in the light of considerations of accommodation and conclude that accommodation constraints operate on their individuation exactly as they do in the definition of natural kinds and categories. I conclude, in consequence, that the debate over whether species are kinds or individuals is less momentous metaphysically and methodologically than one might at first suspect, and that even those who are convinced that species are individuals must conclude that they are natural kinds as well.

I draw a distinction between two equally legitimate notions of definition in science: programmatic definitions and explanatory definitions. I deploy the idea that species are homeostatic property cluster kinds, together with this distinction, to clarify other issues about the metaphysics of species. In the first place, I conclude that individual species have (homeostatic property cluster) essences, so that a form of "essentialism" is true for species, albeit a form of essentialism quite different from that anticipated by Mayr and others who have discussed essentialism in biology. Furthermore, I indicate how recognizing species as homeostatic property cluster phenomena and drawing the distinction just mentioned allows us to make better sense of issues regarding "realism" and "pluralism" about species level taxa.

I extend the application of the accommodation thesis to consideration of the question of the reality of higher taxa. I argue that, in the sense of the term required by the accommodation thesis, some higher taxa are probably real natural kinds--indeed, probably homeostatic property cluster natural kinds. I deploy that thesis to identify a crucial relation between judgements of arbitrariness or conventionality of representational schemes, and show how that reference to that relation can help to clarify and to evaluate claims about the conventionality of higher taxa.

0.1. Homeostatic Property Cluster Kinds. In the empiricist tradition since Locke, the standard conception of scientific (and everyday) kinds has been that they are defined by "nominal essences," or by other purely conventional specifications of membership conditions. Part of that conception has been a conception of linguistic precision according to which a properly defined kind will be defined by necessary and sufficient membership conditions. Since the boundaries of kinds are, on the nominalist conception characteristic of empiricism, purely matters of convention, any failure of scientific concepts to correspond to this standard of precision could, in principle, be remedied by the adoption of more precise nominal definitions.

¹. In formulating my approach to natural kinds, I have benefited greatly from conversations with Eric Hiddleston, Barbara Koslowski, Ruth Millikan, Satya Mohanty, Sydeny Shoemaker, Susanna Siegel, Jason Stanley, Zoltan Szabo and Jessica Wilson. My thinking about biological taxonomy benefited greatly from conversations with Christopher Boyd, Kristin Guyot and Quentin Wheeler.

The realist critique of Lockean nominalism which arose with naturalistic conceptions of natural kinds and of the semantics of natural kind terms (Kripke 1971, 1972; Putnam 1972, 1975a, 1975b) was articulated around examples of a posteriori definitions of natural kinds which likewise specified necessary and sufficient membership conditions, like natural definitions of chemical kinds by molecular formulas (e.g., "water=H₂O"). These critiques thus gave support to what many authors call the "traditional" essentialist conception of natural kinds according to which, among other things, such kinds possess real (as opposed to nominal) essences which define them in terms of necessary and sufficient membership conditions². It is likewise implausible that traditional essentialist views always incorporated such a conception of kind definitions. Those biologists who held that human races, as they are ordinarily recognized, have different essences should not be understood to have held the absurd position that such races always have such sharp boundaries. Incidentally, if the analysis of kind essences offered here is correct then such races do have essences, albeit essences reference to which is important in sociology, history and related disciplines but not in biology..

At the time I began thinking about these issues, philosophical conceptions of kinds and categories which did not treat definition by necessary and sufficient conditions as the relevant standard of precision were pretty much limited to Wittgensteinian and other "ordinary language" conceptions whose extrapolation to scientific cases did not seem to me very plausible.

I had the intuition that nevertheless the prevailing conception of linguistic precision was a holdover from logical positivism. My first foray into defending that view (Boyd 1979) focused mainly on the question of whether or not the linguistic precision appropriate in science was compatible with the use of "vague" metaphors in scientific theorizing, with the associated risk of what Field 1973 calls "partial denotation." I concluded that partial denotation and subsequent "denotational refinement" (Field 1973) are constituents of the very phenomenon of precise reference. In the course of defending this view I found myself advancing a conception of reference according to which certain relations between a term in use and, say, a natural kind are constitutive of the reference relation without any one of them being necessary for it to obtain. Thus I became committed to the view that the relation of reference was not definable in terms of necessary and sufficient conditions.

I became convinced that this was true of a great many scientifically and philosophically important natural kinds, categories and relations, and in a series of papers (Boyd 1988, 1989, 1991, 1993, forthcoming b) I advanced a conception of homeostatic property cluster kinds designed to explain why there were such natural kinds.

Here's what I proposed happens in such cases (I formulate the account for monadic property terms; 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 an a posteriori theoretical rather than an a priori conceptual issue.

2. Wilson (1996) goes so far as to make such a conception of natural kinds part of what he calls "traditional scientific realism". It seems to me that the tradition of scientific realism, such as it is, was centered on the issue of refuting empiricist verificationist arguments against knowledge of "unobservables" rather than on the issue of whether or not scientific kinds are individuated by essences which specify necessary and sufficient membership conditions. Early on, the traditional realist turn in the philosophy of science gave rise to a critique of behaviorism and to realism about mental states and properties. It is implausible to hold that scientific realists who participated in this critique believed, or were committed to believing, that the natural kinds of psychology always have sharp boundaries determined by necessary and sufficient membership conditions.

(viii) Moreover, there will be many cases of extensional indeterminacy 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 \underline{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 \underline{t} is a natural kind.

(x) No refinement of usage which replaces \underline{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 \underline{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 \underline{t} may vary over time (or space), while \underline{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 \underline{t} refers.

0.2. Examples. In almost any philosophical discussion about the nature of natural kinds the author will illustrate her claims with especially persuasive illustrative examples. It will, no doubt, seem odd to readers who are biologists or philosophers of biology that in the papers just cited I deployed biological species as such examples of HPC natural kinds. It is a peculiarity of the literature that in mainstream analytic philosophy biological species are--along with chemical elements and compounds--the paradigmatic natural kinds, whereas among philosophically-inclined biologists and philosophers of biology there is almost a consensus that they are not kinds at all (see, e.g., Ghiselin 1974, Hull 1978, Ereshefsky 1991).

My aim in the papers just cited was mainly metaphilosophical: I hoped to persuade mainstream readers that many philosophical categories and relations (reference, knowledge, rationality, moral goodness,...) might be HPC kinds. In that context biological species served as useful illustrative examples. In the present essay, however, my aim is to establish the credibility, within the philosophy of biology, of the view that species are HPC natural kinds and to explore the implications of this conception for our understanding of the species problem in biology and of related problems about essentialism and about the reality of higher taxa.

0.3. Strategy. I propose to address four considerations which might be thought to support the view that species are individuals and not natural kinds:

1. They are not defined by necessary and sufficient conditions, specified in terms of intrinsic properties of their members, as respectable kinds should be.
2. They are necessarily restricted to particular historical periods and circumstances, whereas natural kinds are universal in the sense of not being so restricted.
3. They do not fall under universal exceptionless laws as genuine natural kinds do.
4. They differ from natural kinds in that what unites their members is their historical relationships to one another rather than their shared properties.

I maintain that the first three of these considerations draw their current plausibility from a profoundly outdated positivist conception of kinds, and that the fourth participates in both that error and in a misestimate of the explanatory role of species concepts in biology. I'll offer an alternative to the positivistically motivated conception of natural kinds and their essences, and explain why, in the light of that account, biological species properly count as natural kinds, defined by real essences, even if, in some sense, they are also like paradigm cases of individuals.

I'll then indicate how the insights of the alterative account can be extended to provide resources for the treatment of other aspects of the species problem, and even to certain issues about higher taxa.

0.4. The Essence of Essentialism: Towards a New Understanding. One implication of the HPC conception of (some) natural kinds is that the positivist conception of natural kinds reflected in considerations 1-4, above, and suggested by examples like "water=H₂O", mislead us about what is essential to the essentialist critique of Lockean nominalism about kinds. What is essential is that the kinds of successful scientific (and everyday) practice cannot be defined by purely conventional a priori "nominal essences." Instead they must be understood as defined by a posteriori real essences which reflect the necessity of our deferring, in our classificatory practices, to facts about causal structures in the world. What is definitely not essential to an essentialist conception of scientific (and everyday) natural kinds is that it conform to the positivist picture suggested by 1-4. So, in here defending the HPC conception, and its application to the species problem, I hope to contribute to a new understanding of issues of essentialism in biology and elsewhere.

A point of clarification is in order here about the relation between my defense of a new understanding of essentialism and prominent critiques of "essentialism" in biology. Several authors (e.g., Mayr 1988, Hull 1965) point to an essentialist tradition within biology prior to the consolidation of the Darwinian revolution. According to the essentialism they have in mind, biological species, like other natural kinds, must possess definitional essences which define them in terms of necessary and sufficient, intrinsic, unchanging, ahistorical properties of the sort

anticipated in 1-4. They attribute the influence of this traditional conception of species, and of kinds in science generally, to the influence of a number of philosophers, including Plato and Aristotle, and in rejecting such conceptions they take themselves to be rejecting essentialism.

I'm offering an alternative approach to the problem of essentialism. I'll argue that species (and, probably some higher taxa) do have defining real essences, but that those essences are quite different from those anticipated in the tradition which Mayr, Hull and others criticize.

In attributing the current plausibility of the conception of natural kinds (and thus of real essences) which I criticize to the influence of recent positivism, I do not mean to dispute the claim that earlier philosophers, including ancient ones, contributed to establishing the plausibility of the sort of essentialism which was influential in pre-Darwinian biology. What I claim here is that what plausibility the conception of natural kinds and real essences I criticize currently enjoys among philosophers of science and philosophically sophisticated biologists derives from the legacy of recent positivist philosophy of science rather than, for example, from any lingering Platonistic or Aristotelian tendencies³.

1. Natural Kinds and Accommodation.

1.0. Accommodation and Reliable Induction. It is a truism that the philosophical theory of natural kinds is about how classificatory schemes come to contribute to the epistemic reliability of inductive and explanatory practices. Quine was right in "Natural Kinds" (1970) that the theory of natural kinds is about how schemes of classification contribute to the formulation and identification of projectible hypotheses (in the sense of Goodman 1973). The naturalness of natural kinds consists in their aptness for induction and explanation; that's why (on one scientifically central notion of definition) definitions of natural kinds are reflections of the properties of their members which contribute to that aptness.

The thesis I'll defend here (the accommodation thesis) makes the further claim that what is at issue in establishing the reliability of inductive and explanatory practices, and what representation of phenomena in terms of natural kinds makes possible, is the accommodation of inferential practices to relevant causal structures.

Here is the basic idea: Consider a simplified case in which reliable inductive practices depend on our having a suitable vocabulary of natural kind terms. Suppose that you have been conducting experiments in which you exposed various salts of sodium to flames. In each of many cases the flame turned yellow. You conclude that always (or almost always) if a salt of sodium is heated in a flame, then a yellow flame results. You are right and your inference is scientifically respectable.

Your inductive success in this matter is a reflection of the fact that the categories salt of sodium, flame, and yellow are natural categories in chemistry, and of the fact that the hypothesis you formulated with the aid of reference to these categories is a projectible one.

Now anyone who has read Goodman (1970) can come up with indefinitely many unprojectible generalizations about such matters which equally well fit all past data but which are profoundly false. You were able to discern the true one because your inductive practices allowed you to identify a generalization which was appropriately related to the causal structures of the phenomena in question. In this particular case, what distinguished the generalization you accepted from the unprojectible generalizations which also fit the extant data was that, for any instantiation of it which makes the antecedent true, the state of affairs described by the antecedent will (in the relevant environment) cause the effect described by the consequent. Your deployment of projectible categories and generalizations allowed you to identify a causally sustained generalization.

What is true in this simplified example is true in general of our ability in scientific (and everyday) practice to identify true (or approximately true) generalizations: we can identify such generalizations just to the extent that we can identify generalizations which are (and will be) sustained by relevant causal structures. Things may be more hairy than they are in our example; perhaps the truth makers for the antecedents of true instantiations are symptomatic effects of causes of the states of affairs described by the consequents. Perhaps the generalizations speak of causal powers and propensities rather than of determinate effects, so that it is the causal sustenance of propensities rather than the causation of effects which is relevant. Perhaps the generalizations have a more complex logical form, etc.

Still, we are able to identify true generalizations in science, and in everyday life, because we are able to accommodate our inductive practices to the causal factors which sustain them. In order to do this--in order to frame such projectible generalizations at all--we require a vocabulary, with terms like "sodium salt" and "flame" which is itself accommodated to relevant causal structures. This is the essence of the accommodation thesis regarding theoretical natural kinds.

1.1. Accommodation Demands and Two Notions of Definition.

1.1.0. Terminology. Some terminology will prove useful. It is widely recognized that the naturalness of a natural kind--it's suitability for explanation and induction--is something like discipline relative. The states of human organisms which are natural kinds for psychology (that is: kinds reference to which facilitates accommodation of the inferential practices of psychology to relevant causal structures) may not turn out to also be natural kinds in the same sense for physiology. In discussing this sort of relativity of accommodation I prefer to speak of disciplinary matrices as the situations of inferential practice with respect to which accommodation is accomplished. It is characteristic of natural kind terms that, although the kinds they refer to are suited to induction and explanation in some contexts and not others, their utility for explanation and induction is rarely, if ever, circumscribed by disciplinary boundaries as these are ordinarily understood. Psychological states are natural kinds for psychology, but also probably for sociology, anthropology, intellectual history, and other disciplines. Acids form a natural kind for chemistry, but also for geology, mineralogy, metallurgy, etc. By a disciplinary matrix I'll understand a family of inductive and inferential practices united by common conceptual resources, whether or not these correspond to academic or practical disciplines otherwise understood.

By the accommodation demands of a disciplinary matrix, M, let us understand the requirement of "fit" or accommodation between M's conceptual and classificatory resources and relevant causal structures which would be required in order for the characteristic inductive,

3. I thank Professor Hull for suggesting this clarification.

explanatory (or practical) aims of M to be achieved. Of course, there may be basically successful disciplinary matrices not all of whose accommodation demands can be satisfied: for some of the explanatory or inductive aims of such a disciplinary matrix there might not be the sorts of causal structures which could sustain the sought after generalizations or regularities.

What the accommodation thesis entails is that the subject matter of the theory of natural kinds is how the use of use of natural kind terms and concepts (and, likewise, natural relation terms or natural magnitude terms, etc.) contributes to the satisfaction of the accommodation demands of disciplinary matrices.

1.1.1. Definitions. There are two quite different but perfectly good senses of the term "definition" in play when we discuss the definitions of scientific kinds and categories. In one sense of the term, a "definition" of a natural kind is provided by specifying a certain inductive or explanatory role which the use of a natural kind term referring to it plays in satisfying the accommodation demands of a disciplinary matrix. Call this sort of definition of a kind a programmatic definition. Defining an element by the inductive/explanatory role indicated by its location in the periodic table would be an example of offering a programmatic definition for it.

There is another perfectly legitimate sense of "definition" according to which a definition of a natural kind is provided by an account of the properties shared by its members in virtue of which reference to the kind plays the role required by its true programmatic definitions. Call this sort of definition of a kind an explanatory definition. Defining a chemical element in terms of its atomic number and the associated valence structures is an example of offering an explanatory definition.

To a good first approximation [I'm ignoring here the issues of partial denotation, non-referring expressions, subtle questions about the individuation of disciplinary matrices, translation of natural kind terms between different languages employed within the same disciplinary matrix, etc.] one can characterize true explanatory definitions in terms of the notion of the satisfaction of accommodation demands as follows:

Let M be a disciplinary matrix and let t_1, \dots, t_n be the natural kind terms deployed within the discourse central to the inductive/explanatory successes of M. Then the families F_1, \dots, F_n of properties provide explanatory definitions of the kinds referred to by t_1, \dots, t_n just in case:

1. (Epistemic access condition) There is a systematic, causally sustained, tendency--established by the causal relations between practices in M and causal structures in the world--for what is predicated of t_i within the practice of M to be approximately true of things which satisfy F_i , $i=1, \dots, n$.

2. (Accommodation condition) This fact, together with the causal powers of things satisfying F_1, \dots, F_n , causally explains how the use of t_1, \dots, t_n in M contributes to accommodation of the inferential practices of M to relevant causal structures: that is to the tendency for participants in M to identify causally sustained generalizations and to obtain correct explanations.

To put the matter slightly differently, one can say that the explanatory definition of a natural kind is provided by an account of the family of properties shared by its members which underwrite the inductive/explanatory roles indicated by its true programmatic definitions.

1.1.2. A (Sort of) Continuum of Definitions. The best known treatments of programmatic and explanatory definitions in the philosophical literature probably lie in functionalist discussions of the definition of psychological states. The very general and abstract definitions of such states proposed by so called "analytic" functionalists are efforts at programmatic definitions: they defined psychological states in terms of very broadly characterized explanatory roles. By contrast, so called "psychofunctionalist" accounts represent efforts at explanatory definitions of the same states. [Excellent discussions of these conceptions are to be found in Block 1980.]

There are, however, many ways in which the literature on functionalism raises issues--about the analytic-synthetic distinction and about the properties of mental states in physically impossible organisms, for example--which are irrelevant for our present purposes (for a discussion of some of them see Boyd forthcoming a). For that reason, it is probably better to take, as paradigm cases of programmatic definitions, the definitions of chemical elements in terms of the inductive/explanatory roles indicated by their positions in the periodic table and to take, as paradigm cases of explanatory definitions their, definitions in terms of atomic number.

What these examples illustrate--and what is true in general--is that both programmatic and explanatory definitions of a natural kind embody claims about the causal powers of its members. In fact, although there is an important difference between the aims of the two sorts of definitions, there is something like a continuum between the most abstractly formulated programmatic definitions of a natural kind and its explanatory definitions. Thus, for example, a chemical element might be programmatically defined in terms of the causal/explanatory role corresponding to a particular place in the periodic table, but the causal/explanatory role it occupies might equally well be spelled out in term of valence, or in terms of the structure of orbitals, or, ..., with ever increasing specification of the details of its causal/explanatory role in chemistry until the characterizations in terms of causal/explanatory role converge to an account of an explanatory definition of the element in question.

Thus, the relationship between proposals for programmatic definitions on the one hand, and explanatory definitions on the other, is quite complex. As the literature on analytic functionalism and psychofunctionalism suggests, even when proposed programmatic and explanatory definitions for a natural kind are quite different there need be no incompatibility between them. Once the "continuum" just discussed is recognized, we can see that the same can be true of two quite different proposed programmatic definitions of the same kind, provided that they are cast at different levels of abstraction. At the same time, since programmatic definitions are a posteriori claims about the relation between the causal potentials of things and the accommodation demands of disciplinary matrices, unobvious conflicts between programmatic and explanatory definitions of the same kind, or between programmatic definitions of a kind involving different levels of abstraction, are possible.

What will prove important for our purposes in considering definitions of individual species is the simple point that programmatic formulations of species definitions in terms of general explanatory roles are not, in general, rivals to explanatory definitions in terms of common factors, relations of descent, gene exchange, etc.

1.2. Accommodation in Inexact, Messy and Parochial Sciences.

1.2.0. Kinds, Laws, and all that: The Standard Empiricist View. There is a venerable (or, at least, serious and admirable--a lot depends on how inclined you are to veneration) empiricist tradition of identifying natural kinds as those kinds which (a) are defined by eternal, unchanging, ahistorical, and intrinsic, necessary, and sufficient conditions and (b) play a role in stating laws, where laws are understood as exceptionless, eternal, and ahistorical generalizations. It is this tradition which underwrites many of the arguments to the effect that species are not natural kinds. Thus, we need to see to what extent its conclusions can be sustained in the light of the accommodation thesis.

One thing we can say with some certainty is how the empiricist account originates: from three (or more) parts Hume and one part physics envy. Physics envy first. The logical empiricists' conception of precision, both of laws and of kind definitions, owed much to an idealized conception of the achievements of fundamental physics, whose laws and kinds seem to have the properties in question.

Hume is more important here. The logical empiricist project crucially involved rationally reconstructing the notion of causation in terms of the subsumption of event sequences under laws of nature. Such a reconstruction required that the notion of a law itself have a non-metaphysical (and, in particular, non-causal) interpretation. If by a law one understands just a true (or, worse yet, approximately true) generalization then the 20th century version of the Humean analysis of causation fails, since there are (many) too many laws, many of them mere accidental generalizations. What empiricists needed was a syntactic (or, at any rate, a non-metaphysical) distinction between law-like and non-law-like generalizations, and it was pretty clearly recognized that this distinction would have to do epistemic as well as (anti)metaphysical work--that it would have to mark out the distinction which we would now describe as the distinction between projectible and non-projectible generalizations.

The proposal that laws be exceptionless, that they be universally applicable (in the sense that their universal quantifiers not be restricted to any particular spatio-temporal domain), and that they be ahistorical (in the sense that they make no reference to any particular place, time or thing) was part of the effort to provide such a non-metaphysical account of law-likeness, and the characterization of natural kinds in terms of their role in such laws was a consequence of the intimate connection between law-likeness and projectibility.

We'll later address the question of whether a contemporary Humean should adopt the same conception of natural kinds, and with it the implication that species cannot be kinds. [The answer will be "no."] For the present, what is important is that we recognize that the empiricist characterization of natural kinds we are considering arose, not from an investigation of actual linguistic, conceptual, and inferential practices in science, but solely from an attempt to reconstruct such practices to fit an independently framed empiricist philosophical project.

1.2.1. Lawlessness. According to the empiricist conception we are considering, natural kinds must figure in laws which must themselves be true, tenseless, universal generalizations which hold everywhere in space-time and which involve no references to spatio-temporal regions or to any particulars. It follows from this conception that there are no laws--and thus no natural kinds--in history and the social sciences, in most of biology, in most of the geological sciences, in meteorology, etc.

It should be obvious that no such conclusion about natural kinds is compatible with the account in terms of accommodation offered here. The phenomenon which the theory of natural kinds explains--successful inductive and explanatory inferences and the accommodation of conceptual resources to causal structures which underwrite them--occurs no less in inductive/explanatory enterprises which seek (and achieve) more local and approximate knowledge than in fundamental physics--or whatever discipline it is whose laws are supposed to fit the empiricist conception.

The problem of projectibility and the associated accommodation demands are no less real in geology, biology, and the social sciences than in (philosophers' idealization of) basic physics. What requires explanation, and what the theory of natural kinds helps to explain, is how we are able to identify causally sustained regularities which go beyond actually available data, and how we are able to offer accurate causal explanations of particular phenomena and of such causally sustained regularities. These regularities need not be eternal, exceptionless, or spatiotemporally universal in order for our epistemic success with them to require the sort of explanation which the theory of natural kinds provides. Whatever philosophical importance (if any) there may be to the distinction between causally sustained regularities and statements which describe them, on the one hand, and LAWS (Ta! Ta!), on the other, it is not reflected in the proper theory of natural kinds.

1.2.2. Inexactitude. In the disciplines just mentioned we are largely unable to formulate exact laws. It is important to see that this fact makes the demand for accommodation of conceptual and inferential structures to relevant causal structures if anything more pressing (or, at any rate, more demanding) than it is in the case of disciplines where exact laws are available (assuming that there are any such disciplines). Here's why: The unavailability of exact laws in, e.g., meteorology, arises from the fact that the number of causally relevant variables which have some effect on the phenomena studied is much too large to be canvassed in generalizations of the sort which practitioners (even aided by high speed computers) can formulate. The conceptual machinery of a discipline with this feature must be adequate to the task of identifying important natural factors or parameters which correspond to causally sustained, but not exceptionless, tendencies in the phenomena being studied. That's what projectibility judgments in such disciplines are about.

What this means in practice is that practitioners are faced with data which exhibit lots of discernable patterns some, but not most, of which are in fact sustained by the sought after natural factors or parameters. Since none of these patterns comes even close to being exceptionless, researchers cannot rely on approximate exceptionlessness as a clue to projectibility as they might well in disciplines which are capable of discerning exact (or nearly exact) patterns. If anything, then, the task of identifying causally sustained generalizations (and explanations licensed by them) will be more difficult and complex than in the cases of more nearly exact disciplines. Thus the importance of achieving accommodation between conceptual machinery and important causal structures in inexact disciplines--the task of identifying natural kinds, categories and magnitudes--cannot possibly be less significant than it is in the exact disciplines. Whatever the philosophically important differences between exact and inexact disciplines might be, they are not a matter of the unimportance of natural kinds in the latter.

1.2.3. Natural Vagueness and Non-intrinsic Defining Properties. Exactly similar considerations about the task of identifying natural categories in the inexact disciplines, where taking account of all causally relevant factors is impossible, make it clear why the natural kinds in such disciplines need not (indeed cannot) be defined by necessary and sufficient membership conditions. Because, e.g., a natural kind in meteorology must be

defined by only a proper subset of the causally relevant factors, and must participate only approximately in (only approximately) stable weather patterns, there is no prospect whatsoever that there will be absolutely determinate necessary and sufficient conditions which provide the its explanatory definition. [This is not, I should add, analytic; it's just true.] Instead, the explanatory definitions of such kinds will reflect the imperfect clustering of relevant properties which underwrites the contribution reference to them makes to accommodation--just as the accommodation thesis requires.

It is likewise non-analytic but true that in the inexact sciences of complex phenomena the explanatory definitions of natural kinds often involve some relational (as opposed to intrinsic) properties. Social roles, whether in human societies or in the societies of non-human social animals, are clear cut examples. It is no objection to the naturalness of such kinds to say, as an ardent reductionist might, that, whenever the occupier of a particular social role (alpha male, let us suppose) exhibits, on a particular occasion, the causal powers and dispositions characteristic of that role, there will always be intrinsic properties of other relevant organisms and of relevant features of the environment which are causally sufficient, together with intrinsic properties of that organism, to establish the causal powers and dispositions in question.

Relationally defined categories, like social roles, are natural kinds just in case deployment of references to them contributes to the satisfaction of the accommodation demands of the disciplinary matrices in question. Their explanatory definitions include relational properties just in case the shared causal powers and dispositions among their members upon which that contribution to accommodation depends are causally sustained by (among other things) shared relational properties. That an imaginary and unpracticable disciplinary matrix might embody the project of, e.g., predicting and explaining the behaviors of social animals by deriving them from independently formulated intrinsic physical characterizations of the animals and of their environments is irrelevant to the question of whether (partly) extrinsically defined social kinds are natural kinds in the disciplinary matrices we actually work in.

1.2.4. Historicity. It may be somewhat harder to see why the definitions of natural kinds need not be ahistorical and unchanging. Consider first the question of whether the explanatory definition of a natural kind can be such that members of the kind are necessarily restricted to some spatial or temporal region, or such that it involves reference to a particular space-time region or individual.

The obvious cases of natural kinds with just these properties are the historical periods recognized by an explanatorily relevant periodization of the history of some phenomena or other. Suppose for the sake of argument that it is revealing of important causal factors in European history to distinguish, for any given political and economic region, between a feudal period on the one hand, and the period of transition to recognizably modern organization of trade, production and governance. If this is so, then the distinction in question will correspond, for each region, to two different natural categories of historical events and processes such that the consequences of an historical event will tend to be significantly determined by its situation with respect to this periodization. Of course, the natural historical periods in question would have "vague" boundaries--they would possess homeostatic property cluster explanatory definitions--but as we have seen this would not undermine their status as natural kinds in the sense appropriate to the accommodation thesis.

If an example in which the members of the kinds are historical events seems too atypical to be fully convincing, consider the (homeostatic property cluster) distinction between feudal and capitalist economic systems. It is almost certainly true that recognizing this distinction contributes fundamentally to accommodation in the disciplinary matrix which includes economic and social history.

Now, according to some economic theories (Marxist ones, for example) this distinction corresponds to quite general (inexact) "laws" of economic development such that in any suitably situated human society there would be a tendency for the means and organization of production to go through a feudal stage followed by a capitalist one. An alternative view is that the explanatory utility of the distinction rests instead on a very large number of factors peculiar to European economic history, so that, while it is explanatorily important to study the transition from feudalism to capitalism in various different European countries or regions, this is so because of factors peculiar to Europe.

What's at stake in the difference between these two conceptions is methodologically important. It is commonplace to describe China's economic organization as having been feudal until the present century. If the first conception is correct, this claim, if true, should be expected to indicate explanatorily important similarities between, say, early 19th century China and 14th century England. If, on the other hand, the second conception is correct, the economy of China was 'feudal' only in an extended metaphorical sense of the term, and expecting to find explanatorily important similarities of the sort indicated would be a mistake.

Suppose now, for the sake of argument, that the second conception of the distinction is correct. Then deployment of the categories "feudal economy," "capitalist economy," and of the categories employed to characterize the transition between feudal and capitalist economies, will contribute to the satisfaction of the accommodation demands of economic and political history only to the extent that it is recognized that the phenomena they describe are peculiar to a particular temporal segment of European history. If this is so, then the deployment of the categories in question contributes significantly to the accommodation of the explanatory practice of economic and political historians, albeit only when they are examining economic and social developments in Europe between, e.g., the 10th and 21st centuries.

On the assumption we are entertaining, the category feudal economy, and the other categories in question, are thus natural kinds in the sense established by the accommodation thesis. They are less widely applicable than one might have hoped, but this merely illustrates the claims that both programmatic and essential definitions of natural categories are a posteriori and revisable. It does not undermine the claim that these categories are natural: they do represent real achievements in the accommodation of explanatory practices in European history to relevant causal factors, and that itself is no mean feat.

My own guess is that the first of the two conceptions of the notion of a feudal economy is more likely, and that the notion may well be fruitfully applicable outside the European context. Another reader might hold that the distinctions we have been discussing fail to contribute to accommodation even within the European context. What would be extraordinary, however, would be for there to be no natural kinds which exhibit historicity of the sort we are discussing.

I conclude, therefore, that there is no reason to deny that there can be genuine natural kinds which are historically delimited in the way we have been considering. Of course, if biological species are natural kinds, then almost certainly they are such kinds, but that is a question to

which we will come later.

1.2.5. Non-Eternal Definitions. Consider now the question of whether or not the explanatory essence of a natural kind must always involve the same properties--must be in that sense eternal or unchanging. The obvious examples of a natural kinds with non-eternal definitions, if they are admitted as cogent, are those biological species whose integrity depends on gene exchange between constituent populations and reproductive isolation from closely related conspecific populations. At any given time in the history of such a species, whatever properties operate to ensure such isolation will be constituents of its explanatory definition. With the extinction of some relevant conspecific populations and the emergence of others, the properties which are thus parts of the species' explanatory definition can change over time.

Of course, all the elaborate machinery of the present part of this paper is directed towards persuading the skeptical reader that biological species are natural kinds. For the reader who has not already anticipated--and been convinced by--the arguments to come, there are other examples which illustrate, albeit not so uncontroversially, the same point. Consider, for example, philosophical or scientific or religious conceptions, like christianity, Islam, empiricism, rationalism, behaviorism or vitalism, considered as natural kinds in intellectual history. Such doctrines typically are motivated, molded and sustained by a number of different factors, "internal" to the relevant discipline or practices as well as "external." The reader is invited to consider for herself the view (which I now advocate) that the effect of this diversity of factors is such that, at any given time, such a doctrine will be characterized by a homeostatic cluster of particular doctrines, methods, explanatory and argumentative strategies., etc.

It seems evident that the intellectual historian will treat these homeostatically defined conceptions as persisting social phenomena whose historical development forms a central part of the subject matter of her discipline. Accommodation to the complex causal factors which underwrite and change the homeostatic unity of the conceptions she studies will require that she individuate such conceptions in such a way that the doctrines, methods, etc. which constitute their definitions change over time. This is, I suggest, exactly what historians in fact do, and what they should do. So, conceptions of this sort are natural homeostatic property cluster phenomena with (in the relevant sense) non-eternal definitions.

Similar considerations suggest that other categories defined in terms of causally important but evolving historical phenomena will have non-eternal homeostatic property cluster definitions, at least with respect to those disciplinary matrices concerned with historical developments as well as static situations. Social structures like feudalism or capitalism, or like monarchy and parliamentary democracy are probable examples. I conclude that the best available conception of natural kinds implies that non-eternal definitions are a perfectly ordinary phenomenon in disciplinary matrices concerned with the history of complex phenomena.

1.3. Homeostasis, Compositional Semantics and Disciplinary Matrices. There is one more consequence of the accommodation thesis which it will be useful to have examined before we turn to issues about biological species. Disciplinary matrices are themselves HPC phenomena. What establishes the coherence of an intellectual discipline is a certain commonality of methods, explanatory strategies, relevant findings and the like. We may see how this sort of commonality results in disciplinary coherence by recognizing that, within any disciplinary matrix, there are very, very many accommodation demands arising from the enormous range of quite particular phenomena for which explanations and/or predictions are sought. What we recognize as an intellectual discipline is the phenomenon manifested when a cohesive set of laws, generalizations, conceptual resources, technical and inductive methods and explanatory strategies contributes to the satisfaction of a very wide spectrum of accommodation demands.

The conditions of satisfaction of these accommodation demands are thus themselves homeostatically related: the satisfaction of various of those demands tends systematically to contribute to the satisfaction of many of the others. In typical disciplines this homeostasis is in large measure a matter of widely applicable causal knowledge: the commonalities among, or systematicity in, the significant causal interactions between the factors which produce the phenomena under study are such that the knowledge of such factors necessary to solve one disciplinary problem will conduce to the solution of a great many others.

This homeostatic tendency is reflected in the very phenomenon of natural kinds. What we recognize as a natural kind is a multipurpose category, reference to which facilitates the satisfaction of a great many accommodation demands within a disciplinary matrix. Here then is a particular aspect of the homeostasis just mentioned: typically, the kind distinctions central to meeting one of the accommodation demands of a disciplinary matrix will facilitate the satisfaction of many others of its accommodation demands.

What will be important for our purpose is the way in which this particular aspect of disciplinary homeostasis is related to the compositional semantics of natural kind terms. We are used to the idea that natural kinds are the kinds which are the subjects of natural laws--not perhaps eternal, ahistorical, exceptionless laws, but at least explanatorily significant causal generalizations of some sort. It is important to note that even this concession to the positivist tradition overstates the connection between natural kinds and laws. There are lots of natural kinds whose naturalness is indicated, not their being the subjects of natural laws, but by the fact that reference to them is crucial for the formulation of laws with more specific subject matters. Goodman's (1973) contrast between green and grue illustrates this point. There are no interesting laws about green things generally, but references to colors like green are important in formulating explanatorily important psychological generalizations.

More scientifically important examples of the same phenomenon are provided by, e.g., the categories acid, element, ion, and compound in chemistry. There are few explanatorily important generalizations which apply to all of the members of any of these categories, but reference to them is central to the formulation of important laws. The contribution which recognition of these categories makes to the satisfaction of accommodation demands in chemistry depends on the compositional roles of the terms "acid," "element," "ion," and "compound" in specifying the subject matters of important generalizations.

Even when a natural kind exhibits its naturalness by being the subject matter of explanatorily important causal generalizations, the homeostatic contribution which its recognition makes to the satisfaction of accommodation demands in the relevant disciplinary matrix will typically depend to a much greater extent on the compositional role of natural kind terms referring to it. The paradigmatic natural kinds (species excepted)--chemical elements--provide a spectacular illustration of this point. There are, to be sure, laws regarding each of the elements.

Nevertheless, the overwhelming majority of chemical natural kinds are compounds rather than elements, so the overwhelming majority of chemical laws do not have elements as their subject matter. Thus the main contribution which the use of terms referring to elements makes to the satisfaction of accommodation demands in chemistry arises from the use of such terms in formulas for chemical compounds.

1.4. The "Reality" of Natural Kinds. Two related points follow which will be important when we turn to discussions of the metaphysics and epistemology of the species category. In the first place, the naturalness of a natural kind is not a matter of its being somehow fundamental, with less fundamental kinds being somehow less natural than more fundamental ones. Thus, for example, with the discovery of the phenomenon of chemical isotopes there was no methodologically or philosophically significant problem about the true or real elemental level in chemistry with conflicting positions regarding the question of whether the true or more fundamental elemental level consisted of categories defined just by atomic number or, instead, by categories defined by atomic number and atomic weight. The decision to adopt the practice of using the term "element" for categories of first sort was a matter of convenience, not a matter of fundamental metaphysics or fundamental chemistry. What was important--and not just a matter of convenience or convention--was that either choice would result in the establishment of a vocabulary for chemistry in which the same class of causally and explanatorily relevant distinctions could be drawn. The naturalness of a natural kind is a matter of the contribution reference to it makes to the satisfaction of the accommodation demands of a disciplinary matrix, in the context of a system of a compositional linguistic resources for the representation of phenomena.

This fact, in turn, constrains how we should interpret questions of "realism about" particular (allegedly) natural kinds, or questions about which kinds exist or are "real." What the accommodation thesis indicates is that the metaphysical achievement which the deployment of kind terms and concepts may or may not represent is the accommodation of inferential practices to relevant causal structures, so the "reality" of a kind consists in the contribution which reference to it makes to such accommodation. What we have just seen is that--strictly speaking--questions of "realism" or "reality" are, in the first instance, questions about a family of classificatory practices incorporated into the inferential practices of a disciplinary matrix, rather than questions about particular kinds, or even about families of kinds, abstracted from the context of disciplinary practices.

When we ask about the "reality" of a kind or of the members of a family of kinds--or when we address the question of "realism about" them--what we are addressing is the question of what contribution, if any, reference to the kind or kinds in question makes to the ways in which the classificatory and inferential practices in which they are implicated contribute to the satisfaction of the accommodation demands of the relevant disciplinary matrix. Claims to the effect that some kind or kinds are not "real," or (equivalently) "anti-realist" claims about them, are best understood as claims to the effect that reference to the kind or kinds in question fails to play an appropriate role in such accommodation, where the role in question is often tacitly indicated by the context in which such "anti-realist" claims are made.

It is thus always preferable for such claims to be spelled out explicitly in terms of the relevant sort of contribution to accommodation, rather than by misleading reference to issues regarding the "reality of" or of "realism about" the kind(s) in question. It's important to note in this regard that what is misleading about these less precise formulations is not that they misleadingly suggest that what is at issue are metaphysical questions about the kinds in question: questions about the accommodation of representational and inferential practices to real causal structures in the world are at issue, and these questions are paradigmatically metaphysical. Instead, what is misleading about talk of the "reality" or "unreality" of kinds, or about "realism" or "anti-realism" about them, is that they suggest, wrongly, that the issue is one regarding the metaphysical status of the families consisting of the members of the kinds in question--considered by themselves--rather than about the contributions which reference to them may make to accommodation. Issues about "reality" or about "realism about" are always issues about accommodation (see Boyd 1990).

1.5. Disciplinary Relativism and Promiscuous Realism. It follows from the account just developed that the naturalness of a natural kind will ordinarily be a matter of the role which reference to it plays in some particular family of inductive or explanatory practices. A kind may be natural "from the point of view of" some discipline or disciplinary matrix but not "from the point of view" of another. Perhaps jade is a natural kind in gemology or the history of art, but not in geology (since some jade is jadeite and some is nephrite and these two minerals are chemically quite different). What we have just seen is that this relativity to a discipline or disciplinary matrix does not compromise the naturalness or the reality of a natural kind. Natural kinds just are kinds defined by the ways of satisfying the accommodations demands of particular disciplinary matrices.

Duprè (1993) makes a similar point about the relativity of the naturalness of kinds to particular projects. He argues for a "promiscuous realism" about natural kinds according to which, among other things:

There is no God-given, unique way to classify the innumerable and diverse products of the evolutionary process. There are many plausible and defensible ways of doing so, and the best way of doing so will depend on both the purposes of the classification and the peculiarities of the organisms in question, whether those purposes belong to what is traditionally considered part of science or part of ordinary life (p. 57).

The accommodation thesis, according to which the naturalness (and the "reality") of a natural kind consists in the contribution which reference to it makes to the satisfaction of the accommodation demands of a particular disciplinary matrix, supports, and provides a metaphysical rationale for, this aspect of Duprè's conception (but probably not to his other critiques of unificatonomist conceptions of science--critiques I confess to not fully understanding). Different disciplinary matrices, and different accommodation demands within a disciplinary matrix, will--given the complexity of the biological world--require reference to different, and cross classifying, kinds in order to achieve accommodation, and this fact in no way demeans the naturalness or the "reality" of those kinds.

One of the criticisms of Duprè's conception offered by Wilson (1996) is that the classificatory categories of ordinary life and language are not natural kinds at all; he denies that common sense and common language are "...in the business of individuating natural kinds at all (p. 307)." According to Wilson, ordinary language lacks the systematic purpose of uncovering order in nature which governs scientific practice and language, and which makes it necessary for scientific terms (as opposed to ordinary language ones) to refer to natural kinds defined by real essences. Duprè himself indicates that the plurality of natural kind classifications in ordinary language is unsurprising because common sense aims to gather information about the world, rather than primarily to achieve a unified picture of it. Wilson agrees, but identifies the latter aim with the sciences and sees reference to natural kinds, defined by real essences, as appropriate only to the latter task.

The position advocated here allows one to "split the difference" between these two conceptions of everyday kinds. Although the choice of the term "disciplinary matrix" undoubtedly betrays my special concern with the issue of kinds in the theoretical sciences, everyday life provides disciplines, or at any rate regimes of inferential and practical activity, in which the accommodation of practices to casual structures is central. Consider the category lily, made famous (among a select few) by Duprè 1981. As it is employed in everyday life--in gardening, flower arranging, landscaping, decorating houses, etc.-- the category lily does not, according to Duprè, contain such members of the biological family Liliaceae as onions and garlic, and various tulips. Nor is there any biological taxon below Liliaceae whose members are just the lilies. So lily represents an ordinary life natural kind distinct from the kinds of scientific botany. Wilson agrees that onions and garlic are not lilies, but denies that the ordinary language category lily is a natural kind.

I suggest that the plants we ordinarily call lilies (excluding onions and garlic, etc.) do form a natural kind in the sense required by the accommodation thesis. Lilies share a family of causal properties and capacities (as it happens, a homeostatic cluster of such properties), and that fact is what explains why reference to lilies helps to satisfy the accommodation demands of the disciplinary matrix which involves gardening, landscape design, decorating, and the like. Lilies share aesthetically relevant features of structure and coloration, and they fall into a manageably small set of categories which characterize their horticulture-wise relevant growing conditions and blooming periods. Deploying the category lily, as horticulturalists, gardeners and others do, contributes to their ability to achieve the botanical and aesthetic results they aim at precisely because categorization of flowering plants in terms of these shared properties achieves accommodation to relevant causal factors.

This illustrates an important fact: even the affairs of everyday life require accommodation between conceptual and classificatory resources and causal structures, so everyday kinds are usually natural kinds in the sense defined by the accommodation thesis. Gruified gardening would be as unsuccessful as gruified mineralogy.

On the other hand, the accommodation demands of everyday practical disciplines may well often be quite different from those of theoretical disciplinary matrices. In particular, they may often involve far less deep or fundamental (although not necessarily less subtle) inductive and explanatory achievements. It is this fact which underwrites Wilson's insight that the kinds of everyday life are much less deeply implicated in projects of theoretical unification than those of the sciences.

Millikan (forthcoming) draws a distinction between natural kinds generally and those natural kinds which play a role in systematic and integrated scientific theorizing. I prefer this way of putting the distinction to Wilson's. In the first place, Millikan's approach helps to preserve the insight that everyday kinds are vehicles for satisfying accommodation demands just as scientific natural kinds are. Secondly, I suspect that there is something like a continuum in degree of theoretical or integrative commitment between everyday accommodation-serving kinds and scientific natural kinds, and that this fact is reflected in our everyday linguistic practices.

I have in mind, of course, the cases in which reference to what are plainly scientific kinds (of diseases and medicines; of sort of semiconductors and other electronic parts; of reagents for photographic development, etc.) play a role in everyday practical or recreational endeavors. But, I have in mind as well a general feature of ordinary linguistic usage which seems to point towards a general recognition of the everyday relevance of theory-driven standards of classification.

Duprè 1981 launched the case for (what became) promiscuous realism by insisting that, in the ordinary everyday sense of the term "lily," onions (among other plants) aren't lilies. While it is true that we don't ordinarily count onions as lilies--since they aren't decorative--our judgments (even our ordinary ones) about whether onions are lilies are remarkably sensitive to the ways the question is put. Someone who says "Onions are lilies," may seem to have spoken falsely or misleadingly, but someone who says "Onions are a kind of lily," says something that many would intuitively accept. There are lots of similar cases ("Birds are a kind of dinosaur," "The glass snake is a kind of lizard," "Tomatoes are a kind of fruit," "Mushrooms are not really a kind of plant") where the expression "kind of" signals reference to (or, if you prefer, deference to) scientific and theoretical standards. The fact that ordinary language has such a semantic device for marking out, and thus making available, reference to scientific standards provides, I believe, further reason for recognizing that ordinary kinds and scientific natural kinds lie along a continuum. They do so precisely because they are all kinds of natural kinds: that is, resources for achieving accommodation.

1.6. Natural Individuals. We shall presently turn our attention to the famous (or infamous) question of whether biological species are kinds or individuals. It will be important, therefore, to recognize that it is a consequence of the accommodation thesis that the question may not have as deep a metaphysical import as the literature would suggest. Once we begin to think of natural kinds as features of human inferential architectures--as artifacts rather than as Platonistic entities--as the accommodation thesis requires, the distinction between natural kinds and natural individuals becomes less important.

A number of philosophers have suggested something like this conclusion in discussing the species-as-individuals issue. Duprè (1993) concludes that the real question about whether species are individuals or kinds "...is whether the same set of individuals can provide both the extension of a kind and the constituent parts of a larger individual. And the answer to this is clearly yes..." (58).

Ereshefsky (1991) understands the "traditional" notion of a natural kind approximately along the lines indicated in section 0.3; he therefore concludes that species are not kinds but "historical entities." Still, he does maintain that some of them are individuals as well whereas others are not, so he does not take the category individual to be incompatible with the much more kind-like category historical entity, which includes the higher taxa.

Finally, Wilson (1996) seems to hold that Duprè's conception, if developed in Duprè's promiscuous or pluralist style, would commit one to "the absurdity of saying that one and the same thing is a natural kind and an individual" (310). But even he then goes on to say that the choice between the two conceptions of species is "merely pragmatic," suggesting, I believe, that neither has an advantage in regards to satisfying the accommodation demands of biology. What I propose is that by seeing the similarities between the inductive and explanatory roles played by reference to natural kinds, on the one hand, and by reference to individuals, on the other, we can see why the distinction between natural kinds and (natural) individuals is, in an important way, merely pragmatic.

After all, successful induction and explanation depend just as much on the accommodation of our individuating practices for individuals to relevant causal structures as on the accommodation of those practices for kinds. A failure to be able to recognize the various stages

in the maturation of an organism as stages of the same organism would undermine induction and explanation in biology just as much as a failure to deploy accommodated schemes of classification for the organisms themselves. The fact that it is, for certain familiar cases, easier to get this sort of thing right should not prevent our recognition that the classification of temporal stages as temporal stages of the same individual must meet just the same constraints of accommodation as the classification of individuals into natural kinds. Nor should it lead us to miss the point that sometimes accommodation of inferential practices for individuals is a real scientific achievement, as in the case of organisms whose larval and adult stages are so dissimilar as to appear contraspecific. If the truth be known, the spatial or temporal stages of a natural individual form something like a natural kind.

It may seem odd to think of the stages of some ordinary object--that rock over there, for example--as forming a natural kind; after all, particular rocks aren't typically explanatorily important enough to make the honorific title "natural kind" seem appropriate. This is less clearly so for some bigger rocks--that of Gibraltar, for example--or for other sorts of individuals--Oliver Cromwell, let's say. In these cases, and lots of others, the accommodation which underwrites cogent explanations (I assume that historical explanations count as causal and require accommodation) depend on our capacities to individuate explanatorily important individual entities. Of course, if biological species are individuals, then they are individuals with the explanatory importance characteristic of natural kinds.

Even with respect to the cases of inconsequential (but still natural) individuals our capacities to individuate are central to successful accommodation of inferential practices to causal structures. Thus, for example, experimental trials on ordinary and (individually explanatorily unimportant) mice, trees, mineral specimens, DNA samples, fossils, rivers,, etc. depend for their inductive cogency on experimenters' abilities to properly individuate these things. Experimental studies on "gruified" mineral samples would represent failures of accommodation in just the same way (and to just the same extent) that such studies of grue (but properly individuated) samples would.

Just think about a Quinean hydrologist studying river-kindred water stages. The distinction between natural kinds and natural individuals is almost just one of syntax. In particular, the metaphysics of accommodation is the same for natural kinds and for natural individuals.

1.7. A Humean Note. I have just argued against a conception of natural kinds according to which they must be defined by unchanging necessary and sufficient membership conditions and must figure in eternal, ahistorical, exceptionless laws. I suggested that the current plausibility of this conception arises, not from any important features of actual scientific practice, but from the demands of the logical empiricists' project of providing Humean rational reconstructions of causal notions (together with a bit of physics envy).

Now, I have argued elsewhere (Boyd 1985b) that such Humean reconstructions must always fail. [Here's the argument in brief: Scientific realism is true, so we have (unreconstructed) knowledge of such factors as the charge of electrons. But charge just is a causal power. So knowledge of unreconstructed causal powers is actual.] What is important for our purposes is that a rejection of the Humean project of rational reconstruction is not necessary in order to accept the conclusions of the preceding sections.

Perhaps there is some metaphysically innocent notion of "law" or "lawlikeness" in terms of which an anti-metaphysical reconstruction of causal notions can be provided. Whether this is so or not, scientific (and historical, and everyday) knowledge often depends on our being able to identify causally sustained generalizations which are neither eternal, nor ahistorical nor exceptionless, and our ability to do so depends on our coordination of language and classificatory categories with causal phenomena involving, and defined by, imperfect property homeostasis. Any adequate Humean rational reconstruction, whether of science or of other areas of empirical knowledge, will need to be compatible with the recognition of these facts, and will thus be compatible with a (suitably reconstructed version of) the homeostatic property cluster conception of natural kinds advanced here.

2. Species as Homeostatic Property Cluster Natural Kinds.

2.0. Species as Homeostatic Phenomena.

2.0.0. Species Level Homeostasis. It is, I take it, uncontroversial that biological species, whether or not they are natural kinds, are phenomena which exhibit something like the sort of property homeostasis which defines homeostatic property cluster natural kinds. A variety of homeostatic mechanisms--gene exchange between certain populations and reproductive isolation from others, effects of common selective factors, co-adapted gene complexes and other limitations on heritable variation, developmental constraints, the effects of the organism-caused features of evolutionary niches, etc.--act to establish the patterns of evolutionary stasis which we recognize as manifestations of biological species. Indeed, the dispute between defenders of Mayr's biological species concept and those who hold that the species category properly includes asexually reproducing organisms is just a dispute over the relative power of these sorts of homeostatic mechanisms in sustaining the sort of homeostatic integrity characteristic of biological species.

2.0.1. Quibbles and Refinements. The account of HPC natural kinds which I offered in earlier papers and rehearsed in section 0.1. requires some fine tuning in order to capture species level homeostasis, whether or not biological species are natural kinds. I'll briefly indicate here what is required. In the first place, the earlier account emphasizes the homeostatic unity of properties shared (imperfectly, of course) by all (or almost all) of the members of the relevant kind. The fact that there is substantial sexual dimorphism in many species, and the fact that there are often (for example, in insect species) profound differences between the phenotypic properties of members of the same species at different stages of their life histories, together require that we characterize the homeostatic property cluster associated with a biological species as containing lots of conditionally specified dispositional properties for which canonical descriptions might be something like, "if male and in the first molt, P," or "if female and in the aquatic stage, Q."

Once this is recognized, and once the more general phenomenon of polytypic species is recognized, it becomes clear that an even more precise formulation of the homeostatic property cluster conception of species would, in the first instance, treat populations as their members and would describe species level homeostasis as connecting causal factors influencing the statistical distribution of phenotypes among their members. No doubt, additional refinements would be in order but, like those just mentioned, they would elaborate rather than undermine the conception of biological species as homeostatic property cluster phenomena.

2.1. Species and Accommodation. Species are homeostatic property cluster phenomena. Are they homeostatic property cluster natural kinds? The obvious next question to ask is whether or not reference to species is crucial to the satisfaction of the accommodation demands of the relevant disciplinary matrix, and how closely the contributions which reference to them makes to accommodation resembles the contributions achieved by reference to uncontroversial examples of natural kinds.

I take it that it is uncontroversial that our ability to identify biological species and their members with some high level of reliability is central to our ability to obtain correct explanations and predictions in the biological sciences. In that regard species are like natural kinds, and like the natural individuals discussed earlier, in that reference to them is central to the satisfaction of accommodation demands. Thus, the argument rehearsed earlier shows that biological species--whether kinds or individuals or whatever--are very much like natural kinds with respect to issue of the metaphysics of accommodation.

In fact the resemblance is much greater. One way in which the family of stages which constitute some natural individual might be thought to differ from a paradigm natural kind lies in the way the commonality in properties between the various stages of the individual contributes to accommodation. In the case of paradigm natural kinds, the fact that its instances (tend to) share lots of explanatorily relevant properties in common is central to the contribution which reference to the kind makes to accommodation.

In the case of some natural individuals this sort of commonality of properties is much less important to accommodation; instead it is the nature and dynamics of the continuity between their temporal stages which is overwhelmingly important. This is, perhaps, true, for example, of (individual) tropical storms and of individual forests, considered as objects of study in historical ecology. The explanatorily relevant respects of continuity between stages of such individuals enforce some similarities between nearby stages, but it is probably the continuity of historical development rather than these similarities which is explanatorily central.

Since biological species are historical entities, one might conjecture that the same sort of thing happens with them. They exhibit homeostatic unity of phenotypic properties over time, but the properties shared by individuals (better yet, populations, on the more sophisticated formulations just discussed) within a species might not be especially explanatorily significant. If this were so, then biological species would be like tropical storms rather than like paradigm natural kinds in that the historical continuities between their temporal realizations, rather than their shared properties would be centrally important in their contributions to accommodation. The plausibility of this conjecture might be enhanced if one followed Mayr (1961) in distinguishing "functional biology" from "evolutionary biology," and offered the conjecture being discussed as relevant to the evolutionary (and thus historical) notion of species (I do not mean to imply that Mayr would approve of this application of his distinction).

If this could be maintained then the objection discussed in section 0.3. that biological species differ from natural kinds in that what unites their members is their historical relationships to one another rather than their shared properties would be sustained for the case of species as objects of evolutionary theorizing.

Of course, this conception cannot be sustained. All of the standard sorts of evolutionary explanations, either for speciation, or for the phenotypic properties species exhibit, tacitly (if not explicitly) presuppose that members of each of the various species in question exhibit a very wide range of shared phenotypic characters of the sort sustained by mechanisms of property homeostasis, and they ordinarily presuppose the action of many of these homeostatic mechanisms. The reader is invited to examine for herself evolutionary explanations in terms of, e.g., individual selection, kin selection, genetic drift, founder effects, etc. to determine whether or not they fundamentally presuppose approximately static background property commonalities among the members of the relevant species even while explaining changes in other particular properties.

2.2. Species as Homeostatic Property Cluster Natural Kinds. Species are at least very much like natural kinds: they reflect solutions to the accommodation demands of biology. Moreover, the ways in which reference to them contributes to satisfying these demands makes them resemble paradigmatic natural kinds as opposed to the least kind-like natural individuals (which are themselves very much like natural kinds).

I propose that biological species simply are HPC natural kinds. What is interesting is that the best arguments in favor of the alternative view that they are individuals rather than kinds actually support the thesis I am proposing. When the residual positivist conception of kinds is stripped away, what the best arguments that species are individuals rather than kinds come down to, at least to a good first approximation, is that organisms which are in the same biological species must (a) be members of some initial population of that species or descendants of its members (so that a species cannot become temporarily extinct and then re-evolve) and (b) must, if contemporaneous, be members either of the same population or of populations which are relevantly reproductively integrated (so that the constituents of species have important internal relations with each other as constituents of paradigm individuals do).

The more cogent reasons for insisting that species must have the two characteristics just mentioned do not depend on outdated philosophy of science, but on biology. When a family of populations of organisms satisfies (a) and (b), above, the fact of their common descent and reproductive integration is a source of a tendency toward evolutionary unity. The biologically serious arguments for (a) and (b) rest on the scientific claim that, without the operation of the factors they require, a family of populations will not possess the evolutionary unity characteristic of species level taxa. [Considerations of this sort are explicit in, for example, Hull 1978 and in Ghiselin, 1974.]

Let's suppose, for the sake of argument, that the considerations in favor of (a) and (b) are correct. Then common descent and reproductive integration of the sort they require are essential to establish the homeostatic evolutionary unity of biological species: the unity anticipated by inferences and explanations in evolutionary biology, and thus required for accommodation. But, as we have seen, the unity anticipated by such inferences and explanations is that appropriate to HPC kinds. Both species-as-individuals theorists and their opponents are tacitly treating biological species as HPC natural kinds. That's what they are.

2.3. Programmatic Definitions of Individual Species. It is important to reply to one possible rebuttal to the homeostatic property conception of species just defended. Someone who was persuaded that species are natural kinds and that the homeostatically unified properties their members

(imperfectly) share are crucial to the satisfaction of accommodation demands in biology, might still hold that, strictly speaking, a biological species is not defined by the associated homeostatic property cluster. She might reason as follows:

My favorite candidate for a programmatic definition of the species level in taxonomy is P. For any given species, S, the proper definition of S is provided by the formula 'the P which is instantiated in T', rather than by the associated homeostatic property cluster.

[Where 'P' is some functional characterization of the species level in taxonomy, like Mayr's biological species concept, and 'T' denotes the type specimen(s) of S or some other suitable representative(s).]

Such a proposal might seem attractive. After all, all extant proposed programmatic definitions of the species level are not more than a couple of paragraphs long, whereas it may be in practice impossible to survey all the members of a species level homeostatic property cluster, so only if something like the proposal in question were right, would we ever be able to state the definition of any biological species.

What the proposal fails to take account of however, is the distinction between programmatic and explanatory definitions. If we have an adequate programmatic definition of the species level (Good luck!) then we can indeed offer programmatic definitions of individual species in the way indicated. But such programmatic definitions would not be competitors with the explanatory definitions provided by the relevant homeostatic property clusters (see section 1.1.2.). This is easy to see by reflecting on the fact that the programmatic definition "stuff which" (where specifies the role of gold in the periodic table of the elements) is not a competitor for the definition of gold as the element with atomic number 79.

2.4. Biological Species are Paradigmatic Natural Kinds (after all). A number of philosophers have argued that the taxonomic claims put forward by species-as-individuals theorists are better and more naturally put by claiming that biological species are historically delimited natural kinds (see, e.g., Kitcher 1984). I agree, of course, but the arguments presented here do more than indicate why this is a better or more natural way of making a formulating taxonomic claims.

In the first place, I have offered a general theory of the nature of natural kinds (the accommodation thesis) which affords a rebuttal to the more philosophical (and positivist) arguments against the thesis that species are natural kinds. It does more than that however. The category "natural kind" is itself a natural kind in metaphysics and epistemology, and the accommodation thesis is a thesis about its essential or explanatory definition. It follows from that definition that biological species are natural kinds, and not marginal examples either. Their homeostatic property cluster structure is perfectly ordinary for natural kinds, they are deeply important to the satisfaction of the accommodation demands of a very, very successful disciplinary matrix, and their departures from the positivists' conception of natural kinds are all essential to the accommodation which reference to them helps to achieve.

In fact, just as philosophers have usually thought, biological species are paradigmatic natural kinds. The natural kinds which have unchanging definitions in terms of intrinsic necessary and sufficient conditions and are the subjects of eternal, ahistorical, exceptionless laws are an unrepresentative minority of natural kinds (perhaps a minority of zero). Every sort of practical or theoretical endeavor which engages with the world makes accommodation demands on the conceptual and classificatory resources it deploys. For very few (perhaps none) of these endeavors can those demands be met by the recognition of the sorts of kinds beloved by positivists. Instead, the sort of kinds (many of them homeostatic property cluster kinds) required for the inexact, messy and parochial sciences are the norm. Of these, biological species are entirely typical, indeed paradigmatic, examples.

3. Species among the Taxa.

3.0. Pluralistic Realism.

3.0.0. Realism. A number of authors (Duprè 1981; Mishler and Brandon, 1987; Mishler and Donoghue 1982; Kitcher 1984; Ereshefsky, 1992) advocate the "pluralist" view that there are different but equally legitimate strategies for sorting organisms into species. The pluralisms they advocate all seem to agree that, for different groups of organisms, different standards for defining conspecificity are appropriate to the explanatory demands of evolutionary biology so that, for example, interbreeding between populations might define conspecificity in the case of one species but not in the case of another.

For Duprè, Kitcher and Ereshefsky (but apparently not for Mishler and Brandon and Mishler and Donoghue) there is another dimension to the pluralism they advocate. Depending on what explanatory project is to be served, the groups of organisms assigned to the species level taxa may be different, so that, for example, a family of populations might constitute a species for the purposes of one explanatory project, but be classified into different species within the same genus for the purposes of another (Ereshefsky proposes eliminating the "superfluous" term "species" in favor of terminology, like "biospecies" and "ecospecies", which reflects the different types of lineages reference to which is appropriate to different explanatory projects).

[There are other important differences--Mishler and Brandon and Mishler and Donoghue require that species be monophyletic while the others do not; Kitcher differs from Ereshefsky in countenancing non-historical, non-evolutionary uses of the term "species"--but there will not be important here.]

Each of these two dimensions to species pluralism is plausible in light of the proposal defended here that species are HPC natural kinds. The first is dictated by the reasonable assumption (defended by all the authors cited) that the homeostatic mechanisms important to the integrity of a species vary from species to species. The second is plausible in the light of the project or discipline relativity of kind definitions indicated in section 1.5. What I want to indicate in the present essay is how the resources developed here can help to articulate and defend pluralistic realism. There are two obvious questions here: (1) if species taxa are properly defined by reference to different sorts of projects, in what sense are they real entities in nature?, (2) if the species category is heterogeneous in this way, what makes it the species category?

In the present section, I'll address the first of these questions. Kitcher's answer is that various approaches to the demarcation of

species taxa correspond to "something in the *objective structure* of nature," which exist independently of human thought, even though different objective interests corresponding to different research programs may require demarcation by reference to different ones of those objective structures. What is important here to the pluralist realism Kitcher defends is that it explains realism about species in terms of the correspondence between species level classificatory practices and objective structures, rather than in terms of some sort of unique metaphysical fundamentality of one or another of the ways of demarcating species. Different ways of demarcating species can correspond to different objective structures, and thus define species categories that are equally real.

I suggest that the accommodation thesis provides us with just the machinery required to make the relevant notion of realism precise. As I suggested in section 1.4., issues about the "reality" of kinds, or regarding "realism about" some kind or family of kinds, is best understood as an imprecise way of addressing the question of the nature of the contributions (if any) which reference to those kinds makes to the satisfaction of the accommodation demands of the relevant disciplinary matrix. The objective structures existing independently of human practice are causal structures, and the "reality" of a kind consists in the contribution which reference to it makes--within the context of disciplinary practices--to the accommodation of those practices to the relevant causal structures. The sort of realist pluralism about ways of demarcating species we are considering amounts to the insight that a plurality of species level classificatory schemes contribute significantly to achieving (different aspects of) the accommodation of inferential practices in biology to relevant causal structures.

3.0.1. The Species Level. Let us now turn to the question of why, if the species category is heterogeneous, it is appropriate to describe it as the species category? I have already remarked (in section 1.3.) that disciplinary matrices are themselves homeostatic phenomena: the satisfaction of some of the accommodation demands of a disciplinary matrix generally tends to contribute to the satisfaction of lots of others.

What makes it possible to speak of taxa at the species level, or of different ways of demarcating species, is, I believe, a particular way in which homeostasis among ways of satisfying accommodation demands happens to work in biology. Defenders of the claim that different explanatory projects require different species definitions argue that species level categories are deployed in biology in the service of significantly different sorts of explanatory projects and that there are different, equally legitimate, ways of demarcating species corresponding to various different ones of these explanatory projects. In the terminology introduced here, they argue that these different projects place somewhat different accommodation demands on the conceptual and classificatory resources deployed by biologists--including demands on species level classifications.

Now, there is in general a homeostatic relationship between the satisfaction of different accommodation demands within biology. What I propose is that the category of species level taxa is fairly well defined, despite pluralism, because of an especially close homeostatic relation between the classificatory practices which satisfy the accommodation demands associated with the identification of the (different) primary subject matters of functional and evolutionary biology. A basic scheme of classification of (populations of) organisms which satisfies the accommodation demands of one set of projects within, say, functional biology will come very close to satisfying the demands, not only of other functional biological projects, but of the different explanatory projects in evolutionary biology, and vice versa. This second-order (or is it third-order?) homeostatic clustering of accommodation demand satisfactions is, of course, no accident. It obtains just because the sorts of stable phenomena which are the subject matter of various species level biological explanations get their stability via a number of relatively closely (homeostatically) related evolutionary mechanisms (Wilson 1966, section 7, makes a very similar point).

Thus the existence of a (pluralistic) species level among taxa, if there is such a level, is an artifact of an especially robust instance of the sort of homeostasis which characterizes disciplinary matrices generally.

3.0.2. Why there is a "Species Problem". The "species problem" is the problem of defining the nature of species taxa. Pluralists of the sort we are considering propose that there is no such nature, that instead there are many different, (approximately) equally methodologically important ways of demarcating species, each corresponding to a different legitimate way of understanding species level taxa. If the solution is so easy, why does it represent a fairly recent proposal?

One reason, no doubt, has been the admirably motivated, but (in the light of the complexity of homeostatic mechanism) ultimately fruitless, effort to establish something like a universally applicable "operational definition" of conspecificity (or at least a unitary formula which determines the relevant definition for any group of organisms) and to thereby establish consistency and uniformity of classificatory and nomenclatural practice. Arguably, the articulation of the species-as-individuals conception contributed to the plausibility of this project. If species are thought of as unique among the taxa in being evolutionary individuals in nature rather than human constructs (as many believe), then perhaps it is more plausible that a single unitary conception of conspecificity--defined in terms of the relevant notion of individuality--will be forthcoming.

What I suspect, however, is that the main source of the species problem is practical. There are lots of disciplines which are like biology in that there are schemes of classification which--by themselves--are almost adequate for the satisfaction of a wide variety of different accommodation demands. This is true, for example, of the classification of the elements in chemistry, and the standard classification of (what are called) mineral species in geology. In each of these disciplines the compositional character of natural kind terms is exploited to "fine tune" these almost adequate categories to fit more particular accommodation demands. Thus, we speak, for example, of the isotopes of chemical elements, the different physical forms of elemental sulfur, and the different varieties of quartz in order to achieve more nearly complete accommodation. There is no persisting "elements problem" in chemistry, and there is no "species problem" in geology, precisely because by using suitable natural adjectival terms to modify other natural terms we can achieve accommodation, and it's merely a matter of convenience just how we do this. This is just the point made in section 1.3. that the compositional semantics of natural kind terms is important to the ways in which the accommodation demands of disciplinary matrices get satisfied.

Why can't we do this in biological taxonomy as well? The answer, I suggest, is that the compositional semantic structure of the standard Linnaean system of taxonomic nomenclature is inadequately flexible. Thus, for example, one might hope to take advantage of the tight homeostasis between the factors sustaining homeostasis within each particular species by settling (it might not matter exactly how) on some one reasonable way of defining the species level taxa and then satisfying the accommodation demands of explanatory programs not perfectly served by this classification by deploying additional natural adjectival terms to differentiate further between groups of organisms or populations.

[Wilson 1996 suggests that the HPC conception of natural kinds might be used to formulate a more unified conception of species. This might be one way of carrying out his project.]

The problem with such a proposal is not that it would be unworkable in the abstract--after all, that's how things are done in lots of disciplines. The problem is specific to the Linnaean hierarchy and the ways in which it constrains the compositional semantics of taxonomic names. Different fine-tuning would no doubt be required for different explanatory projects, but the Linnaean system of nomenclature does not have devices to, for example, distinguish between sub-species-from-the-point-of-view-of-ecology and sub-species-from-the-point-of-view-of-the-genetics-of-speciation.

This is a serious practical problem, given the overwhelming need for a uniform system of biological classification and the entrenchment of the Linnaean nomenclatural scheme. But, there is no reason to mistake it for a metaphysical problem about fundamental entities in nature--or even about the "reality" of species in the sense defined by the accommodation thesis. Instead, it is a metaphysical problem about the lack of fit between the Linnaean hierarchy's representational resources and the causal structures important in biology (for an important account of other such metaphysical problems with the Linnaean hierarchy see Ereshefsky 1994⁴).

3.1. Higher Taxa and Species.

3.1.0. A Dubious Contrast. One of the standard themes in the metaphysics of biology is that species, being individuals, are real entities existing independently of human practice, whereas higher taxa are merely human concepts, reflecting only facts about the history of life, and hence largely unreal, or arbitrary, or merely conventional or something of the sort. The considerations we have rehearsed so far suggest that there is something seriously wrong with this approach to the metaphysics of higher taxa. In the first place, species probably aren't individuals, but they seem quite real enough nonetheless. Secondly, the contrast between individuals on the one hand and conceptual entities like kinds on the other is compromised by the fact that natural individuals are very much like kinds anyway. In particular, the correct individuation conditions (or persistence conditions) for a natural individual are a matter of how reference to it contributes to the satisfaction demands of a disciplinary matrix--a conceptual phenomenon if there ever was one. Finally if, as pluralist realists maintain, there are different but equally legitimate ways of demarcating species, answering to different demands for the accommodation of conceptual resources arising from different explanatory projects, then species--whether they are individuals or natural kinds--are in some sense project-dependent and are thus, in yet an additional way, conceptual (or, at least, concept involving) entities, so they can't contrast with higher taxa on that score.

I suggested in section 1.4. that the question of the reality of a kind should be understood as a question about the contribution which reference to it makes to accommodation, rather than as a question about its metaphysical fundamentality, or anything of that sort. What I propose to do now is to explore the consequences of that approach for the issue of the metaphysics of higher taxa.

3.1.1. Locke. Kitcher says that the reality of species consists in a correspondence between species classifications and the *objective structure* of nature. I agree, and I proposed that the relevant objective structure is causal structure and that the relevant correspondence is a matter of the satisfaction of accommodation demands. It is tempting to articulate this claim further by saying that the realist about species believes that species are natural kinds which exist independently of scientific practice. Call this latter conception the "practice-independence of natural kinds" (henceforth: pink) conception of realism about kinds. [There's an initially unintended pun here. I take the version of realism developed in this paper to be a natural extension of dialectical materialism in the Red tradition. I here defend that tradition against a merely pink alternative.]

If one's conception of realism about kinds is pink, then it will be tempting to treat higher taxa as (much) less real than individual organisms or species. After all, it might be thought that it is hard to see how Mammalia could exist independently of classificatory practice. I propose to rebut the pink conception.

Locke maintained that while Nature makes things similar and different, kinds are "the workmanship of men." I believe that, gender bias aside, he was right to say this. Indeed, I think that the lesson we should draw from the accommodation thesis is that the theory of natural kinds just is (nothing but) the theory of how accommodation is (sometimes) achieved between our linguistic, classificatory and inferential practices and the causal structure of the world⁵. A natural kind just is the implementation, in language and in conceptual, experimental and inferential practice, of a (component of) a way of satisfying the accommodation demands of a disciplinary matrix. Natural kinds are features, not

4. One metaphysical commitment of Linnaeus himself which Ereshefsky criticized is that taxa at the levels of genus and species are defined by mind independent essences whereas taxa above these levels are subject to only pragmatic constraints. Ereshefsky denies the distinction on the grounds that there are no taxon-specific essences at any level. If the conception of essences defended here is correct, then species, and probably many taxa above the species level do have essences (albeit not of the sort Ereshefsky has in mind) but all biological taxa are, in a certain sense of the term, mind dependent, or at least practice dependent (see sections 3.1.1, 3.2.1).

5. Actually, I agree with the suggestion, implicit in Quine 1969, that the theory of natural kinds can be thought of as extending as well to the ways in which accommodation is achieved in non-human inductive and inferential systems.

of the world outside our practice, but of the ways in which that practice engages with the rest of the world. Taxonomists sometimes speak of the "erection" of higher taxa, treating such taxa as, in a sense, human constructions. They are right--and the same thing is true of natural kinds in general.

Locke said that "...each abstract idea, with a name to it, makes a distinct Species." His conception was that kinds are established by a sort of unicameral linguistic legislation: people get to establish kind definitions by whatever conventions (nominal essences) for the use of general terms they choose to adopt.

According to the accommodation thesis, we should, instead, see natural kinds as the product of bicameral legislation in which the (causal structure of the) world plays a heavy legislative role. A natural kind is nothing (much) over and above a natural kind term together with its use in the satisfaction of accommodation demands. ["What else?," you ask. Well, there's whatever is necessary to accommodate translations which preserve satisfaction of accommodation demands and to accommodate phenomena like reference failure and partial denotation.] Or, better yet, the establishment of a natural kind (remember that natural kinds are legislative achievements--that is, artifacts) consists solely in the deployment of a natural kind term (or of a family of such terms connected by practices of translation) in satisfying the accommodation demands of a disciplinary matrix. Given that the task of the philosophical theory of natural kinds is to explain how classificatory practices contribute to reliable inferences, that's all the establishment of a natural kind could consist in. Natural kinds are the workmanship of women and men.

The causal structures in the world to which accommodation is required are, of course, independent of our practices (except when it's our practices which are (part of) the subject matter; see Boyd 1989, 1990, 1991, 1992 for better formulations). Still, natural kinds are social artifacts. That's why asking whether a kind exist independently of our practice is the wrong way to inquire about its reality. No natural kinds exist independently of practice. The kind natural kind is itself a natural kind in the theory of our inferential practice. That's why the reality of kinds needs to be understood in terms of the satisfaction of the accommodation demands of the relevant disciplinary matrix.

3.1.2. Natural Individuals, Again. The very same points can be made about natural individuals, like organisms. The relations of causal continuity, similarity, or whatever, which unite the temporal stages of an organism exist independently of our practices, and they have the causal effects which makes reference to that organism important to the satisfaction of accommodation demands independently of our practice. But the grouping of those temporal stages under a common linguistic or conceptual heading--treating them as constituting an organism--is just as much a matter of social practice in service of accommodation as the establishment of a natural kind.

It's tempting to reply that this can't be right because, even if we become extinct, dogs might continue to exist, so they must be organisms which exist independently of us. Of course dogs might continue to exist: the persistence conditions (properly) associated with the notion of an individual dog might continue to be satisfied. But, the fact that these persistence conditions are natural ones--the fact that persisting dogs are individuals "in nature," as one might say--is a fact, not about nature alone, but about how biological practices are accommodated to nature. After all, some organisms would be in Mammalia even if we became extinct, and they would continue to occupy places in the relevant continuing historical lineages: in that sense Mammilla too exists independently of us.

Nature makes temporal stages similar and different, continuous and discontinuous, but things are the workmanship of women and men.

3.2. Realism about Higher Taxa

3.2.0. Higher Taxa and Accommodation. Neither for kinds nor even for individuals is the question of their reality best understood as a question about independence from our practices. That's why questions of "reality" or "realism" about them are best understood as questions about the accommodation of disciplinary matrices to causal structures. Thus no simple contrast between species and higher taxa with respect to their independence of practice can establish the unreality (or diminished reality) of the latter. Higher taxa may yet be unreal (or less real), but this cannot be because they are matters of human conception and practice. If they are unreal then this will be a matter of their failure to contribute effectively to accommodation.

3.2.1. Assessing Accommodation: Methodological Spectra and the Equifertility Principle. I want to make a proposal about how issues about the contribution of reference to higher taxa (or to any other kinds) to accommodation might be fruitfully approached. Let's say that the choice between two alternative classificatory schemes within the context of a disciplinary matrix is arbitrary just in case neither reflects accommodation-relevant causal structures better than the other. When such a choice is arbitrary, the disciplinary matrix would (from the point of view of accommodation) be equally well served by either scheme.

Now, one measure of the extent to which a classificatory scheme contributes to accommodation--one measure of its "reality"--is given by the range of alternative schemes with respect to which a choice would be arbitrary. Philosophers or biologists who differ about the reality of higher taxa will differ about which choices between higher taxonomic schemes are arbitrary ones. How are we to assess competing claims about such respects of arbitrariness?

It will help to answer this question if we consider the methodological import of such claims. By the substantive conception reflected in a disciplinary matrix at a time let us understand the theories, doctrines, putative insights, etc. regarding the relevant subject matters which are accepted at that time. Of course, in any actual case, there will be issues and controversies of varying degrees of importance within a disciplinary matrix, so referring to the theories, etc. which are accepted at a time involves some degree of idealization, but nothing in what I will argue here depends on any subtleties about how the idealization is understood. I do intend that substantive conceptions be thought of as conceptual entities: as representations of phenomena deploying the conceptual resources of the matrix, rather than, e.g., as sets of propositions understood as non-conceptual entities. The substantive conception, C_M , of a disciplinary matrix M is thus the representation within M of the causal knowledge putatively achieved in M .

The inferential practices within a disciplinary matrix, M , will be (except in cases where practitioners reason badly) justified by the substantive conception, C_M . That's how the accommodation of inferential practices to causal structures is implemented (Boyd 1982, 1985a, 1990,

1991). Now in every case--real or imaginary--there will be some arbitrary or conventional elements to the representational resources deployed within M . By a conventionality estimate E_M , for a disciplinary matrix M , let us understand an account of what the arbitrary or conventional elements are in M 's representational resources. Because (as we shall see) the methodological import of C_M depends on the nature and extent of the conventionality of M 's representational resources, we may think of practice within a matrix at a time as being determined, in part, by practitioners' tacit estimates of conventionality.

Here again some harmless idealization is involved in speaking about the tacit estimate of conventionality prevailing within a matrix at a time. What would not be harmless, however, would be to identify the tacit estimate with the explicit estimates of conventionality articulated by practitioners within M . Those will often be more a reflection of peculiarities of their philosophical education than of the accommodational achievements of their practices. Instead, we should think of tacit estimates of conventionality as being reflected in inferential practice. Thus, for example, the recognition that units of distance measurement are arbitrary or conventional is reflected in the fact that reference to distances in scientific laws is always in terms of distance ratios (either explicitly or via proportionality constants) whereas the non-conventionality of cardinality for sets of humans is reflected in the fact that population statistics often appear in non-ratio forms in the findings of the social sciences and history.

It will prove important for our purposes to note a particular way in which tacit judgments of conventionality are reflected in methodological practice. The primary way in which accommodation of explanatory and inferential practices to relevant causal structures is achieved in mature sciences is via the ways in which the substantive conception within a disciplinary matrix (formulated, of course, with the aid of reference to natural kinds, etc.) informs methodological judgements and practices: in determining projectibility judgments, for example, or in determining the appropriate categories for statistical calculations. Tacit judgments of conventionality are characteristically reflected in the ways in which prevailing substantive conceptions are deployed in making such judgments.

Thus, for example, the tacit (and also explicit, but that's not the point here) recognition that the assignment of negative and positive signs to the charges of electrons and protons, respectively, is conventional--rather than, e.g., a reflection of deficiencies or excesses--is reflected in the fact that the fact, about certain particles, that they have negative charge, whereas others have positive charge, is not taken to render projectible hypotheses to the effect that negatively charged particles suffer from some sort of deficiency in a sense in which positively charged particles do not.

Similarly, the recognition of the conventionality of national units of currency is reflected in the fact that no one makes use of differences in, or ratios between, national debts without prior conversion to some common currency or other economic measure.

These points are obvious, but important. They allow us to identify ways of specifying and assessing conventionality estimates regarding disciplinary matrices. One way of specifying an estimate of conventionality, E_M for a matrix M with substantive content C_M is to specify a range of alternatives to C_M such that the choice between C_M and any of them is to be understood as arbitrary or conventional in the sense that disciplinary matrices just like M except that they deployed any one of these other representations would equally well reflect facts about the relevant subject matter(s).

The examples we have just considered illustrate a quite general and fundamental methodological principle concerning conventionality and its relation to methodology--one which indicates another (related) way in which conventionality estimates can be specified (and, sometimes, assessed). According to the equifertility principle, when the choice between two substantive contents is arbitrary or conventional, the two substantive contents are methodologically equifertile in the sense that no methodological principle or practice is justified by one unless it is also justified by the other. The equifertility principle is about as obvious a methodological principle as there can be. It follows via a pretty straightforward application of the accommodation thesis, provided that one rejects the neo-Kantian view, apparently advocated by Kuhn (1970), that the adoption of a paradigm or conceptual framework can non-causally determine the causal structures of the relevant phenomena (see Boyd 1990, 1992).

What is especially important for the present discussion are the implications of the metaphysical innocence thesis in cases in which it is proposed that the prevailing conventionality estimate, E_M for a matrix M is too modest and that there are alternatives to C_M with respect to which the choice of C_M is unexpectedly conventional. Such a proposal entails that any inference or inferential practice which would be justified (by the standards previously prevailing in the matrix) given C_M , but not given any one of the alternative representations, is thereby shown to be itself unjustified. No inferences which depend on conventional or arbitrary choices of representational schemes are justified.

By the methodological spectrum of a disciplinary matrix, M , at a given time let us understand the inferential strategies and methodological practices which are justified by C_M . What we have just seen is that any proposal of unexpected conventionality within a disciplinary matrix entails that its methodological spectrum is, in a systematically specifiable way, narrower than practice within the matrix assumes. Thus we have two ways of specifying the import of a claim of unexpected conventionality. One characterizes the conventionality in terms of the representations with respect to which the choice of prevailing substantive content is said to be arbitrary or conventional; the other indicates the dimensions of the narrowing of the methodological spectrum of the disciplinary matrix thereby required in the light of the equifertility thesis.

The latter characterization may be important, I suggest, in assessing the merits of proposals to revise prevailing tacit conventionality estimates. It has proven notoriously difficult for philosophers and others to achieve consensus on issues about conventionality. Sometimes, it seems to me, consensus on methodological issues is easier to achieve. When that is so, I suggest, specifying the import for methodological spectra of proposals about conventionality may prove helpful.

3.2.2 Extreme Cladism: A Worked Example. I propose to illustrate the way in which the equifertility principle and considerations about methodological spectra can be deployed in assessing arbitrariness claims by deploying it to criticize an extreme form of cladism about higher taxa. I do not mean to suggest that serious cladists need to hold any position close to the version I discuss, or to suggest a general criticism of cladistic approaches to higher taxa. Indeed I am sympathetic to some versions of cladism. I choose the extreme version discussed here simply to make the application of the equifertility principle simpler.

Imagine that you meet a cladist who maintains that the only scientifically legitimate constraint on the erection of taxa above the species level is that they should be strictly monophyletic. She allows that reasons of convenience might dictate the choice of one taxonomic scheme which honors strict monophyly over another, but neither, she claims, will more accurately reflect evolutionarily relevant features of nature.

Here's how you might reply. Consider efforts to identify and study mass extinctions. Evolutionary biologists interested in such phenomena often wish to estimate how the rate of species extinction has varied over geological time. Since the fossil record does not allow reliable distinctions to be drawn at the species level, they often compare rates of disappearance of genera or families from the fossil record by way of estimating the rate of extinction of species.

You might ask your extreme cladist colleague whether or not she finds such studies cogent. If the answer is "yes," you could point out that, by choosing alternative classificatory schemes such that the choice between them and standard taxonomic scheme is arbitrary by her extreme cladist standards, evidence for mass extinctions could be made to disappear (just make the genus level taxa in the new scheme correspond to, say, class level taxa in the standard scheme). An application of the equiprobability principle entails that the genus extinction data calculated with respect to this scheme are no more or less indicative of evolutionary facts than those based on more standard classificatory practices. Thus, her acceptance of the methodology of the studies in question is incompatible with her version of cladism.

A natural reply would be that, given the alternative scheme in question, the relevant statistical calculations could be done with respect to appropriately chosen sub-generic categories. If your extreme cladist offered this reply, she would be acknowledging a tacit commitment to the idea that there is something natural (that is: non-arbitrary, non-conventional) about the similarity relations between species corresponding to various genus level taxa in current classificatory practice, even if the assignment of those sort of similarity relations to the genus level is arbitrary. She would thus be acknowledging an additional non-conventional constraint on the erection of higher taxa: they must, somehow or other honor reflect the naturalness of those taxa assigned to the genus level in current classificatory practices (and similarly for family level taxa, if she accepts the methodological relevance of family level statistics, etc.).

In real life cases, things would be harder, of course, but the point is this: different estimates of the degrees of arbitrariness or "reality" of classificatory schemes have quite different implications regarding the reliability of inferential methods. Often we are in a position to evaluate these implications and thus make some headway in evaluating claims about arbitrariness.

3.3.3. Homeostasis and the Reality of Higher Taxa. If some form of pluralist realism is right about taxa at the species level, then it will not do to think of nature as picking out the unique real sort of biological taxa, with the rest being arbitrary or conventional. It does not follow, of course, that any of the levels of the Linnaean hierarchy above the species level are--given current taxonomic practice--real, in the sense provided by the accommodation thesis. Still, controversies about the species level seem to revolve around whether certain groups of similar populations should be grouped into the same subspecies, species or genus. If pluralist realism is right, different choices from among these alternatives may, for a given family of populations, each correspond to the establishment of a real taxon. This suggests, although it does not entail, that at least some subspecies and some genera (as these are ordinarily erected) are themselves real rather than arbitrary. [Ereshefsky 1991 makes the similar point that the cohesion thought by some to be distinctive of species level taxa can be sustained by mechanisms which operate at higher taxonomic levels.]

Similarly, statistical calculations like the ones mentioned in the previous section are methodologically important, and this too suggests that genera are real. [Here again, there is no strict entailment. It could be, for example, that genera are real enough for such calculations to be indicative of extinction rates, but sufficiently arbitrary otherwise that the slogan that they are "unreal" is basically right.] What I propose to do in this section is to explore the metaphysics of the proposal that some higher taxa are real.

Of course, the reality of a higher taxon would consist in the contribution which reference to it made to accommodation. What sorts of contributions might one expect? One clue is provided by the view, characteristic of mainstream evolutionary systematics before the triumph of cladism, that higher taxa are to be thought of as defined by adaptive evolutionary innovations which constrain future courses of evolutionary development. On this conception, species within a higher taxon--like populations within a species--share common evolutionary tendencies. In the case of higher taxa these are derived from the constraints on evolutionary development produced by the shared evolutionary innovations or novelties. Higher taxa are defined, in other words, by novel adaptations understood as sources of evolutionary tendencies towards stasis. Reference to them contributes to accommodation in evolutionary theory because the stasis-inducing factors in terms of which they are defined are important in the explanation of macroevolutionary patterns.

An important criticism of this conception of higher taxa has been that it rests on an overestimate of the extent of the role of natural selection in macroevolution. Many of the patterns discernable in the fossil record, and reflected in the evolutionary systematists' erection of higher taxa are, according to this criticism, not products of systematic evolutionary tendencies at all, but merely the effects of historical phenomena which are, from the point of view of evolutionary theory, random.

It seems reasonable to extend the evolutionary systematists' conception of higher taxa as (representations of) loci of evolutionary stasis so as to claim that the reality of such a taxon consists in a distinctive configuration of stasis-enhancing factors which defines it, whether these factors are matters of adaptive evolutionary innovation, developmental constraints, co-evolved gene complexes, niche-organism interactions, or other sources of "phyletic inertia." On this extended conception, too, reference to real higher taxa would contribute to accommodation because their defining properties would be crucially involved in explaining macroevolutionary patterns.

If this conception were right about some higher taxa, these taxa would, like species, be homeostatic property cluster kinds (perhaps with exceptional cases in which a single evolutionary novelty, situated, of course, within the context of other homeostatically related properties, established the relevant tendency towards stasis). The conception that some higher taxa are real in just this way would not be so deeply committed to an "adaptationist" strategy of evolutionary explanation as would more traditional evolutionary systematics, but it would be vulnerable--both in theory and in applications--to the concern that many patterns in the history of life may lack altogether the sorts of

explanations it anticipates.

[My understanding of traditional evolutionary systematics may have been too strongly influenced by critics of "adaptationism." Perhaps what I here present as an extension of the evolutionary systematists' conception may instead represent what they believed all along, free from anti-adaptationist caricature. If so, so much the better for the points I am making here.]

This is not the only way in which some higher taxa might turn out to be real in the sense required by the accommodation thesis, but it is a very important one. The reason is that there are very good reasons to believe that at least some genera are real in this way. I have already indicated why pluralist realism about species suggests that some genus level categories are real. If, as lots of authors have suggested, there are cases in which homeostasis at approximately the species level obtains in families of populations between which gene exchange is minimal or (in the case of asexually reproducing reptilian or amphibian "species" for example) nonexistent, we have reasons to believe that the same sort of homeostasis might obtain at least in some recognized genera, perhaps in most.

Moreover, if some higher taxa are real kinds which are important in evolutionary theorizing, it is hard (although, no doubt, not impossible) to see what their importance could be except as (representations of) stasis producing factors. If that's what real higher taxa are, then it's equally hard, given the complexity of evolutionarily relevant causal factors, to see how the contribution to stasis in any particular case could fail to involve homeostasis of several different factors. I propose, therefore, that in so far as some higher taxa are real and important categories in evolutionary theory (above and beyond their important role in representing patterns of ancestry and descent) they are probably, like species, homeostatic property cluster kinds.

If there are higher taxa which are real in this way, it is important to note that there is no particular reason to believe that their homeostatic property cluster definitions will honor strict monophyly, which is not to deny that the homeostasis linking the members of such a taxon might always crucially involve facts about the effects of their common ancestry. Thus, even if a requirement of strict monophyly is appropriate for some other higher taxa, it need not be so for them.

3.3.4. Modest Cladism. Suppose, for the sake of argument, that some higher taxa--some genera for example--are real homeostatic property cluster kinds in the way indicated. What are we to make of the concern that efforts to discern evolutionary patterns in the fossil record in terms of whose causes higher taxa are to be defined will identify patterns for which no explanation in terms of evolutionary tendencies exists?

The obvious answer is that this problem may arise for some higher taxa and not others. Perhaps taxa at the genus level--as taxa at that level are generally erected--are usually real in the special sense discussed here, but that, say, order-level taxa are usually not. Perhaps some such pattern obtains, but it is different--given extant practices--across phyla. Perhaps taxa of shorter historical duration are more likely to reflect genuine stasis-sustaining properties. Perhaps, taxa erected to account for the earlier stages in the history of life are more or less likely to be real that those erected to account for later stages. Perhaps in this regard things are really a mess of which there is no simple characterization.

In any event, barring the extremely unlikely possibility that the standard criticisms of evolutionary systematics are somehow without force in the light of the slight modification to this position we are considering, there will be some domain of higher taxa about which the cladistically inclined systematist can reasonably maintain that the only important facts about the evolution of life which we can reflect in erecting such taxa are historical facts about relations of ancestry and descent. About erection of taxa of this sort, the only non-conventional or non-pragmatic constraint would then be one of monophyly. This is the modest version of cladism in which I am inclined to believe.

3.5. Concluding Realist Postscript: Descent and Ancestry are Real. I can't resist pointing out that the relation between a species and its daughter species is of causal significance in evolution. The erection of higher taxa which are (at least approximately) monophyletic does, as cladists insist, make a significant contribution to the accommodation of inferential practices in evolutionary biology to relevant causal structures. Such taxa are, in the only available senses of the terms, real natural kinds. So are species. It is a tribute (if that's the right word) to the enduring influence of empiricist conceptions of language, classification and (anti)metaphysics that scientifically and philosophically fundamental points about the limitations of platonist conceptions of taxonomy and (overly) adaptationist conceptions of macroevolution have been formulated in philosophical terms which render obscure some of their main insights.

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