8 Naturalizing Kinds

Muhammad Ali Khalidi

Naturalism about natural kinds is the view that they are none other than the kinds discoverable by science. This thesis is in tension with what is perhaps the dominant contemporary view of natural kinds: essentialism. According to essentialism, natural kinds constitute a small subset of our scientific categories, namely those definable in terms of intrinsic, microphysical properties, which are possessed necessarily rather than contingently by their bearers. Though essentialism may appear compatible with naturalism, and is indeed sometimes qualified with the epithet "scientific," it has become increasingly clear in recent years that only a minority of categories posited by science satisfy those conditions. If one does not limit oneself to basic physics and chemistry, the categories found in the biological and other special sciences often violate one or more of the conditions that essentialists impose upon natural kinds. Indeed, I would argue that even when one does limit oneself to the basic sciences, the strictures of essentialism do not apply to all their categories. However, I will not try to argue against essentialism directly in this chapter. Instead, I will attempt to articulate an alternative, naturalist conception of natural kinds, according to which the mark of natural kinds is their discoverability by science, not just basic science but the special sciences and even the social sciences. I will locate the origins of this naturalist conception in the work of John Stuart Mill, then I will trace it through the works of W.V. Quine, John Dupré, and Richard Boyd. In each case, I will defend some aspects of the views of these philosophers while taking issue with other aspects. What will emerge is a preliminary defense of a naturalist account of natural kinds, which should provide a contrast with the prevailing essentialist conception.

This chapter is based on sections of my forthcoming book, *Natural Categories and Human Kinds* (Cambridge University Press). I am grateful to the publisher for permission to use material from the book. I owe a deep debt of gratitude to the editors of this volume for inviting me to participate in the conference from which this volume originated. I am also very grateful for the feedback I received at that conference, which was instrumental in helping me to develop my views on this topic.

1. DISCOVERABILITY BY SCIENCE

One commonly mentioned condition on natural kinds, by essentialists and nonessentialists alike, is that they should be discoverable by science, and an important feature of scientific inquiry when it comes to natural kinds is that the categories identified by science are corrigible. If scientific categories are found not to correspond to real divisions in nature, they are continually revised and refined until they do correspond. What is characteristic of this method of revision and refinement is the need to make one's categories delimit a class of things that not only share properties but also share important properties, where these are ones that are projectible, enter into new generalizations, are explanatorily fertile, and generate novel predictions, among other features.

Consider a category such as *vertebrate,* as applied to biological organisms. This category was initially introduced to apply to all and only creatures with a spinal column, since this feature was thought to be a significant one that distinguished them in an important way from other organisms. With time, as biologists discovered more about such creatures and as the theory of evolution came to be widely established, they came to believe that what distinguished such organisms was primarily a history of descent from a common ancestor. Eventually, this biological taxon, now considered a subphylum, was thought to contain some organisms that *do not* have a spinal column or vertebrae. Nevertheless, scientists continue to regard the subphylum vertebrate as a significant group and the organisms that belong to it as having important properties in common. Even though the initial classification was based on a feature that is now thought not to be possessed by all members of the taxon (a feature encoded in the very term used to pick out the category), the properties associated with the kind were modified in such a way as to accommodate the prevailing scientific theories and established discoveries about the world. Hence, a key facet of scientific categories is that they are revisable in light of evidence in such a way as to be associated with important or nonsuperficial properties from the point of view of the science in question. The willingness to revise our categories in this way is a key indicator that they are being altered to conform to nature, in other words, that they are discovered rather than merely invented. We can decide to use some of our words whichever way we wish, but we cannot do the same for our scientific concepts. We may link some concepts indefeasibly to certain definitions (e.g., "vertebrate" to the property of possessing a spinal column), but if we take the empirical evidence seriously, we will find that we need to introduce different concepts to correspond to real kinds and do the work of science.[1]

Perhaps we cannot afford to be so confident that scientific categories always track the divisions in nature and pick out the kinds that are genuinely to be found in the world. When scientists settle on superficial distinctions, these distinctions may be weeded out in the course of further investigation, but what if scientists introduce categories that are stipulative or arbitrary? Worse yet, what if their categories are tainted by certain biases or presuppositions that have little to do with the evidence and more to do with extraneous motivations? It is true that the revisability of scientific categories does not, in and of itself, establish that these categories are discovered rather than invented, since categories may be revised to conform with certain prejudices and preconceptions rather than with nature itself. However, in the absence of specific claims to the contrary regarding actual scientific categories, we can take science to aim (among other things) at discovering the kinds of things that actually exist and can conclude that, unless there is a specific reason to regard a scientific category as arbitrary or biased, scientific categories will correspond to natural kinds.

To say that scientific categories correspond to natural kinds is not to say that they do so at the current stage of scientific investigation but that they will do so when science has settled on its categories once and for all. Even so, despite whatever scientific revolutions may yet lie in store for us as we discover more about the natural world, it is unlikely that *all* of our current scientific categories will be displaced in favor of new ones. Past scientific theory changes have tended to leave the bulk of scientific categories intact, while ushering in new ones here and there. There may well be upheavals that lead us to discard a significant proportion of our current scientific categories, but it is improbable that they will all be abandoned in the course of inquiry.[2]

2. REAL KINDS: MILL

The idea that science aims to discover natural kinds, or that scientific categories generally correspond to natural kinds, is in accord with at least some traditional conceptions of natural kinds. It is a long-standing philosophical claim that can be traced back at least to Mill's *A System of Logic* ([1843] 1973). Indeed, two of the main claims emphasized by Mill are ones mentioned in the previous section: (a) that natural kinds are discoverable by science and (b) that our catalogue of kinds and the properties associated with them are generally revised in the course of scientific investigation. Mill ([1843] 1973, IV vii §2) distinguishes "natural" from "artificial" classifications on the grounds that natural classifications are ones that divide objects into groups whose members have a large number of properties in common. By contrast, artificial classifications do not group objects into categories all of whose members share many properties. As an example of an artificial classification, Mill mentions Linnaeus's classification of plants, which was based on the numbers of stamens and pistils in their flowers. The problem with such a taxonomic system, according to him, is that plants with a given number of stamens and pistils do not generally have enough other properties in common to make the classification useful. As Mill puts it, "To think of [plants] in that manner [i.e., as having a certain number of stamens and pistils] is of little use, since we seldom have anything to affirm in common of the plants which have a given number of stamens and pistils" ([1843] 1973, IV vii §2). Hence, a necessary condition on a natural classification system is that members of their classes must share a large number of properties. Though Linnaeus's system was proposed as a scientific hypothesis, it was rejected in light of further investigation, and the category that it was based upon was revised in such a way as to conform more closely to natural divisions. This constitutes another instance in which scientific categories are revised and refined to pick out natural kinds.

Natural or scientific systems of classification, for Mill, are the ones that identify the "real kinds" in nature. But even though all real kinds or natural groups belong to a natural system of classification, not all groups in a natural system of classification are real kinds. To be real kinds, such groups must also satisfy further conditions, besides possessing a large number of properties in common. Chief among these is that the properties associated with a kind be inexhaustible.[3] As Mill puts it, "The common properties of a true Kind, and consequently the general assertions which can be made respecting it, or which are certain to be made hereafter as our knowledge extends, are indefinite and inexhaustible" ([1843] 1973, IV vii §4). But since it is difficult to say what would constitute an "inexhaustible" set of properties, I would argue that it would be preferable to insist on two further conditions that Mill places on natural kinds. The first is the requirement that the discovered properties not follow as a matter of logic from those properties that we already associate with the kind. As Mill puts it in the previously quoted passage, the properties in question must "not [be] deducible from the former [property] by an ascertainable law" ([1843] 1973, I vii §4).[4] The second requirement is that these properties be *important* in some sense to be further specified. Regarding a scientific classification system, Mill states that "the test of its scientific character is the number and importance of the properties which can be asserted in common of all objects included in a group" ([1843] 1973, IV vii §2). Thus, I would argue that if the properties associated with the kind are indeed scientifically important and logically independent of one another, then even if they are not inexhaustible, that should not disqualify it from being a natural kind.

Mill devotes some attention to the issue of "importance," and though he does not delineate the idea clearly, his discussion will serve as a good starting point. He makes two points about importance in this context. The first is that importance is relative to the ends that one intends to accomplish with the classification in question and is not determined irrespective of a particular disciplinary framework or systematic investigation of some scientific domain. To illustrate, Mill writes, "A geologist divides fossils, not like a zoologist, into families corresponding to those of living species, but into fossils of the palaeozoic, Mesozoic, and tertiary periods, above the coal and below the coal, etc." ([1843] 1973, IV vii §2). The acknowledgment that "importance" may be relativized to particular sciences and that what may be important from the point of view of one investigation may not be so from another is a salutary proposal. However, Mill does not adequately spell out what would make something important from the point of view of one inquiry or another. To make matters worse, he proceeds to say that there may be such a thing as classification without reference to particular practical ends or purposes. Thus, his second point is that importance can also be judged outside of any particular context:

> When we are studying objects not for any special practical end, but for the sake of extending our knowledge of the whole of their properties and relations, we must consider as the most important attributes, those which contribute most, either by themselves or by their effects, to render the things like one another, and unlike other things; which give to the class composed of them the most marked individuality; which fill, as it were, the largest space in their existence, and would most impress the attention of a spectator who knew all their properties but was not specially interested in any. Classes formed on this principle may be called, in a more emphatic manner than any others, natural groups. ([1843] 1973, IV vii 2)

Giving a class its "most marked individuality" is a vague notion, as is the idea that important properties are ones that would most "impress the attention" of a disinterested spectator. It would seem that Mill has taken a wrong turn here and that he would have been better off grounding the notion of "importance" in the purposes to which science would put the kind in question. We know which categories science considers important and there is widespread consensus on the features that such categories ought to have. They ought to be projectible, enter into empirical generalizations, summarize a wealth of data, feature in explanations, and give rise to valid predictions.

By taking Mill's conception as our starting point, we have made some headway on the distinctive features of natural kinds. Each natural kind is associated with a number of properties, which are discoverable by science. Not only does science aim at discovering the kinds that exist in nature, but it also revises its categories in such a way as to eliminate categories that do not correspond to genuine kinds of phenomena. Natural kinds ought not to be associated merely with single properties—otherwise, there would be no need to posit a kind over and above the property with which it is associated. Nor, therefore, should they be linked to a set of properties all but one of which are deducible from one property. Hence, the properties associated with natural kinds should not follow from a single property in a trivial manner or as a matter of logic, though they may well be linked with one another according to natural law. Moreover, these properties should be important ones, where importance is understood in terms of the features that we usually look for in our scientific categories: projectibility, explanatory efficacy, predictive value, and so on. With Mill, we should also allow that the ways in which these criteria of scientific importance manifest themselves may vary from discipline to discipline within the sciences, depending on the interests and aims of particular subfields or areas of inquiry.

3. THE END OF INQUIRY: QUINE

The requirement that natural kinds correspond to the categories posited by science is not insular or scientistic but merely identifies natural kinds with the categories that are posited as a result of a systematic inquiry, as opposed to categories that we might be inclined to conceive as a result of a casual or passing acquaintance with some aspect of reality. Quine's attitude to natural kinds may seem somewhat at variance with the approach I have adopted so far. Far from being the categories that science aims to uncover at the end of inquiry, natural kinds are the rough-and-ready categories that we begin with prior to, or at an early stage of, scientific inquiry. For him, natural kinds are grounded in folk classifications; they are sets of things that are all *similar* in some respect. He holds that the notions of *kind* and *similarity* are both somewhat obscure and do not admit of precise definition, though they are interdefined in various ways. Things that we find similar are placed in sets that we consider to be natural kinds.[5] These similarities are initially grounded in our "innate similarity standards," the quality spacing that is common to members of the human species. But as we discover more about the world, many of these similarities are found to be spurious or not far-reaching enough, so we replace them with similarities that are more in line with the true nature of the universe. "Color is king in our innate quality space," Quine writes, "but undistinguished in cosmic circles. Cosmically, colors would not qualify as kinds" (1969, 127). The sciences replace natural kinds based on color similarities with kinds based on other similarities, and these similarity relations are defined differently in different branches of science. As each science matures, Quine thinks that it will define a precise similarity relation that is applicable primarily to its particular subject matter. Chemistry will define similarity of sample objects by matching their constituent molecules (1969, 135). Meanwhile, biology will define similarity of organisms or species in terms of proximity and frequency of common ancestors, or better yet, in terms of common genes (1969, 137). Thus, it seems as if Quine effectively thinks that the notion of similarity will be reduced in each of these cases to some complex relation based on identity (e.g., we might define organism *a* as being more genetically similar to organism *b* than to organism *c* if and only if *a* and *b* have more identical alleles than *a* and *c*).[6] This would make similarity drop out as a generic concept in science, being replaced by specific notions of similarity defined in terms of identity of molecules, genes, or similar constituent entities.

Whether or not Quine's hunch about similarity is vindicated, there does not seem to be anything to prevent the categories that are devised at the end of such a process from being considered natural kinds. Rather than say that the members that belong to such categories are similar, Quine thinks that we should be able to say that they have a certain proportion of identical constituents. But if the categories that are so identified are projectible, have explanatory value, and are otherwise important from the point of view of the relevant science, then they would seem to conform to the notion of natural kind that was discussed in the previous section. This is so particularly if we eschew talk of brute similarity among members of natural kinds and speak instead of members having identical constituents or, more generally, of sharing properties. Hence, Quine's conjecture that we will in the fullness of time be able to dispense with natural kinds altogether is not a conclusion that is forced on us by his conjecture that similarity relations will eventually be made precise and relativized to each branch of science, at least not if we understand natural kinds along the lines that I have been proposing.

Quine thinks that natural kinds have their origin in commonsense categories recognized by natural language and will eventually perish with the emergence of an advanced scientific worldview. Though he allows that the notion of a natural kind and the closely related notion of similarity will have a place in the immature sciences, he thinks that these notions will be phased out as the sciences come to fruition (1969, 138). However, he also states that as long as natural kinds continue to play a role in the immature sciences, these kinds can coexist alongside commonsense kinds. He holds that an "innate similarity notion" can coexist with a "scientifically sophisticated one" and that scientific kinds "do not wholly supersede" the natural kinds that we begin with (1969, 129). Quine does not make clear whether he thinks that our commonsense kinds will eventually be displaced entirely by scientific categories or whether, given certain human concerns, some "intuitive" natural kinds will continue to have a place in our total theory of the world even at the end of inquiry. At times, he implies that the natural kinds embedded in our commonsense concerns will be abandoned altogether, as in the passage quoted about our evolution from "unreason into science." But at other times, he seems to recognize that humans will always have certain mundane concerns and reasons for classifying things that are at variance with scientific classifications (1969, 128).

I have argued that even if Quine is right to think that similarity will be reduced to the more precise notion of identity, that is no reason to abandon natural kinds altogether. Natural kinds can still be considered to correspond to scientific categories, which will now group together not similar individuals but individuals that satisfy certain precise identity relations or that share certain properties. But what are we to say of nonscientific categories, which at least at this stage of inquiry, continue to thrive alongside scientific ones? Can they be candidates for natural kinds? Since I have made discoverability by science the central plank of my account of kinds, it may appear that this could not be the case. But, as Quine and others acknowledge, many scientific categories start out as folk categories. Moreover, there are at least some folk categories whose purpose is primarily to mark distinctions that really exist in nature. This suggests that we might not be able to dismiss such folk categories altogether; this issue will be explored further in the following section.

4. FOLK CATEGORIES: DUPRÉ

What is the relation between folk categories and scientific categories? Elsewhere, I have proposed that folk categories can be expected to be superseded by scientific categories when the purposes for which they are introduced are roughly the same. When they are not, we should not expect them to be so superseded (Khalidi 1998a). If folk medicine aims primarily to ascertain the real causes of human diseases and the folk are focused on distinguishing kinds of diseases based on their causal properties, then we should expect that folk categories will either coincide with scientific categories or, when they do not, that they will be superseded by them. In some cases, a folk disease, which is thought to have certain causes, is replaced by one with quite different causes. The theory associated with the disease *consumption* was modified greatly when tuberculosis bacteria were discovered, leading us to rename the disease and revise many of our beliefs about its causes (e.g., the belief that it was caused by vampirism). Meanwhile, a kind concept such *hysteria,* which was thought to denote a disease primarily afflicting women and involving disturbances in the uterus, proved eventually not to pick out a natural kind of disease and was dropped as a scientific concept. But it may well be that some folk categories are efficacious at treating illnesses and help advance the aim of curing patients and making them feel better, though we have good grounds for thinking that they do not pick out real diseases. That would not be an altogether unprecedented situation, since placebo effects are quite common in medicine. This is approximately the situation with a disease concept such as the *common cold* and its associated etiology (it is more prevalent in cold weather, can be caught by going outdoors lightly dressed, etc.). The concept does not pick out a single type of disease (since what goes by this name can be caused by a wide variety of unrelated viruses[7]), but it may still persist to a limited extent because of the utility of taking precautions in cold weather, a time when people tend to stay indoors in close proximity and are more liable to transmit viruses. Though in such cases the folk categories remain in circulation, they should not be considered candidates for natural kinds. If our aim is merely to make patients' lives better, we might continue to employ these categories in a clinical setting and in communication with patients. However, for the purposes of research, we come to recognize that these categories do not conform to real kinds and are merely useful crutches that enable us to accomplish certain fairly narrow goals. Speaking generally, it is quite possible that we might introduce categories that enable us to serve certain desired aims but do not correspond to the kinds that exist in nature. In such cases, since there is no direct competition between folk categories and scientific categories, there is no reason to expect the folk categories to be ousted by scientific ones. But such folk categories should not be expected to correspond to natural kinds.

At this point, it may be useful to contrast my view with Dupré's on the relation between folk categories and scientific taxonomy. Unlike Quine, Dupré does not think that folk categories will generally be superseded by scientific ones, and he insists that "folk taxonomies are as legitimate and can be interpreted as realistically, as scientific taxonomies" (1999, 461). Indeed, at one point Dupré (1995, 24) suggested that the folk classification of whales as fish was not illegitimate, but was rather warranted in certain folk biological contexts. However, in more recent work, he has come to revise this judgment, admitting that, in this case at least, the scientific mode of classification has now prevailed over the folk classification scheme. So prevalent has this scientific worldview become that the folk themselves no longer regard whales as fish. Hence, Dupré concludes, "Regrettably, I have had to admit that whales are not fish, for the sufficient reason that almost everyone in our culture . . . agrees not to call them so" (1999, 474). But what is missing from this judgment is a consideration of the possible reasons for the purported fact that whales are no longer generally classified as fish. Dupré seems to take it as a brute fact that the folk have deferred to biological practice in this case; indeed, he insists that there "is no good reason" for excluding whales from the category of fish. He thus admits defeat on de facto rather than de jure grounds: as a matter of fact, the folk have deferred to scientists in this case, but they need not have done so, and if they had not, it would have been quite appropriate to judge that whales are fish in certain folk contexts.[8]

If my proposal is correct, we ought to look for the reasons behind deference and lack of deference in each particular instance. It would be rational for the folk to defer to scientific classification if their purposes coincide with the scientific community, but not if their purposes diverge. If it is indeed the case that the folk have almost universally come to exclude whales from the category *fish* (an assumption I will go on to question below), that is presumably because they share (at least) some of the aims or purposes of scientists in classifying organisms, and these aims or purposes are best served by scientific rather than folk classification. Since scientific classification in biology is often based on descent, it would seem as though the folk now also share this interest and have deferred to the scientists at least partly for that very reason. Moreover, in this as in many other biological cases, classification by descent also happens to track important phenotypic features of the organisms involved. Whales are not only not closely related by descent to most of the other organisms we used to label as "fish," but they also do not have gills, they give birth to live offspring, and they possess other mammalian properties that fish generally lack. The original classification was presumably based on gross phenotypic features and a broadly shared habitat. Once these properties turn out not to be "important" (in the sense introduced in section 2), we cease to classify on their basis and seek other properties instead.

So far, I have granted Dupré's claim that the folk have largely deferred to the scientists in this case. Whether or not that is so can only be ascertained by a detailed sociolinguistic inquiry that looks at the way in which the relevant terms are used among laypersons and within the scientific community. Though I have not undertaken such an inquiry, there is at least some evidence that this is not entirely the case from lexicography, which tends to summarize ordinary usage. Many standard dictionaries now include two or more entries for the term "fish," at least one of which refers not to a biological taxonomic category but to the property of being an "aquatic creature" (perhaps prefaced with a parenthetical "loosely" or "colloquially").[9] This provides some reason for thinking that "fish" is equivocal as used in contemporary English, and it is not difficult to see why that would be the case. As Mill noted, "Whales are or are not fish, according to the purpose for which we are considering them" ([1843] 1973, IV vii §2). Hence, it would seem as though there is room for two concepts of fish, according to one of which whales are fish and according to the other of which they are not, depending on the purposes for which we want to use these concepts. However, to this, I would add that not all purposes are created equal. Though the folk may have occasion to use the term "fish" in ways that do not conform to scientific classification, these uses do not appear to be projectible or genuinely explanatory. When the category *fish* includes aquatic animals such as crayfish, jellyfish, starfish, mollusks, and crustaceans, as well as whales and dolphins, it ceases to have value as a projectible category. According to the Fisheries Glossary issued by the Food and Agricultural Organization of the United Nations, "fish" used as a collective term includes mollusks, crustaceans, and any aquatic animal that is harvested.[10] But in this inclusive sense, there is nothing more to be discovered about fish. The property of being (capable of being) harvested is a property that was built into it to begin with. It may be objected that this picks out an "important" property (in the sense of section 2), for the simple reason that anything harvested is subject to laws of supply and demand. But that property is shared with a much broader class of things (commodities), not one pertaining, even loosely, to all and only fish (in the broad sense). Hence, the category *fish,* when interpreted thus is epistemically otiose.

It is instructive to contrast this inclusive use of the term "fish" with the scientific one. Even though the standard scientific use of the term is itself not free of complication, the category is clearly projectible and has explanatory efficacy. There are over thirty thousand species that scientists refer to as fish, though they do not belong to a single monophyletic taxon (a taxonomic category that includes all and only descendants of a common ancestor). From the point of view of cladistic taxonomy, which classifies *strictly* according to descent, there is no taxon that corresponds precisely to the category *fish*. Still, according to other biological taxonomists, there is enough in common among these species that warrants classifying them in a single category, though there is no property that they all share (which is not also shared by nonmembers). The vast majority of creatures classified as *fish* live in water, breathe with their gills, are cold-blooded (ectothermic), swim using fins, lay eggs (oviparous), and have scales. These generalizations are not exceptionless; for example, mudskippers live partly on land, lungfish breathe air through their lungs, bluefin tuna are endothermic, and sharks do not have scales. Still, they hold widely enough across the diversity of fish species and the exceptions share enough other properties (including phylogenetic descent) with species that do have these properties to warrant including them in the category *fish*. In terms of shared properties, the category *fish* is a cluster or polythetic kind rather than a monothetic kind definable in terms of necessary and sufficient conditions, but it is a natural kind nonetheless. Despite the fact that it is not a unitary taxon from the evolutionary point of view, the category *fish* has undisputed value as an epistemic kind. There are a number of branches of science, such as ichthyology and marine biology, that use this category to explain and predict natural phenomena.

My view of natural kinds is avowedly pluralist, but it is less pluralist than Dupré's view, which he calls "promiscuous realism." I concur with him in thinking that different classification schemes reflect different interests and that there is no "uniquely best system of classification for all purposes or, which comes to the same thing, independent of any particular purpose" (1999, 473). However, unlike Dupré, I privilege epistemic purposes over other purposes and I therefore accord a special status to those classifications that are introduced primarily to serve those purposes. By contrast, Dupré argues that "scientific classifications . . . are driven by specific, if often purely epistemic, purposes, and there is nothing fundamentally distinguishing such purposes from the more mundane rationales underlying folk classifications" (1999, 462). But, I would maintain, what distinguishes epistemic purposes from other purposes is that our best epistemic practices aim to uncover the divisions that exist in nature. Since the attempt to ascertain these divisions is none other than the search for natural kinds, classificatory schemes that fulfill epistemic purposes ought to be privileged over others in determining which categories are natural kinds. A category that serves, say, a purely aesthetic purpose cannot be expected to coincide with a natural kind. Consider the category *aquarium fish,* which applies to all and only fish that humans tend to keep in aquaria, largely for their aesthetic qualities. The fact that lionfish are thought to be desirable by fish enthusiasts while codfish are not, and that the former can be correctly classified as an *aquarium fish* while the latter cannot, is a fact about human aesthetic preferences. It does not mark a division between two kinds of fish, nor was it intended to do so. There would seem to be no generalizations to be made about aquarium fish (beyond the fact that they are all and only fish that are kept in aquaria by humans), and there is therefore no epistemic value to the category. The same applies to the category *fish* when used in a loose rather than a precise scientific sense to pick out, roughly, all aquatic animals.

If natural kinds are classifications introduced for epistemic purposes, folk categories can be expected to correspond to natural kinds only when they serve an epistemic purpose. In these cases, they tend to be aligned with categories found in one or the other branches of the sciences or they become so aligned in the course of inquiry. When folk categories do not play an epistemic role, then we should not expect them to correspond to natural kinds, and we should not expect the folk to defer to the experts. Unlike Quine, I do not think that folk categories will always be rejected in favor of scientific categories (though when they are not, they will tend to persist for nonepistemic reasons), and unlike Dupré, I do not think that folk categories are *generally* as legitimate as scientific ones. In some cases, folk categories are revised or modified in such a way as to coincide with scientific categories *(consumption* and *tuberculosis),* in other cases folk categories drop out altogether *(hysteria),* and in yet other cases they remain in place to fulfill nonepistemic purposes (*common cold, fish* in the sense of aquatic animal) and scientific categories are introduced alongside them.[11] It is only in the first type of case that we can expect our folk categories to correspond to natural kinds, since they (come to) coincide with categories that play an epistemic role.

5. HOMEOSTATIC PROPERTY CLUSTERS: BOYD

I have stressed that natural kinds ought to be associated with a set of scientifically important properties, and I have allowed these properties to cluster loosely rather than be necessary and sufficient for kind membership in the case of the natural kind *fish* but have not been explicit about how those properties are linked. The fact that the properties involved are projectibly clustered indicates that they are inductively privileged, which in turn implies that there are causal links between them. So clusters of properties that happen to be coinstantiated are ruled out by this account. The causal links between properties associated with a kind have been emphasized by one of the most prominent contemporary accounts of natural kinds, namely the "homeostatic property cluster" (HPC) account of natural kinds advocated by Boyd (1989, 1991, 1999a, 1999b). On the face of it, the HPC account of natural kinds is inimical to the essentialist view that kinds are associated with a set of necessary and sufficient properties (though I will also mention some attempts to reconcile it with essentialism later in this section). It allows for the existence of cluster or polythetic kinds, contrary to the standard understanding of essentialism. Moreover, according to Boyd, it is not enough for there to be a (loose) cluster of properties associated with a natural kind; those properties must be so associated for a reason: they are kept in equilibrium by a causal mechanism. Boyd's HPC account of natural kinds states that every kind is associated with a set of properties not by happenstance but because there is some "underlying mechanism" that gives rise to all of them or because the presence of some of them favors the presence of others. Properties $P_1, \ldots, P_n$, associated with a kind K are "contingently clustered" in nature and this is not a cosmic coincidence but is rather the result of a process of "homeostasis" as a result of which these properties are kept in equilibrium. Boyd also recognizes that the properties associated with a kind need not be possessed by every member of that kind and he calls this "imperfect homeostasis." In such cases, only some of the homeostatic mechanisms might be present that hold such properties together. Moreover, the properties associated with a kind may vary over time since there is no single property (or subset of properties) that is necessary for membership in the kind (Boyd 1989, 16–17; cf. Boyd 1991, 143–44).

Biological species are widely thought to be a good fit for the HPC account of natural kinds. The HPC account clearly accommodates the fact that there is no set of genotypic or phenotypic properties that is both necessary and sufficient for belonging to a species, as most biologists and philosophers of biology now believe. The account also holds that the properties associated with each natural kind are held together as a result of a causal mechanism or set of mechanisms. In the case of biological species, the principal mechanism is *interbreeding*, according to Boyd, which ensures that properties possessed by members remain in circulation within the population. Others have added mechanisms of *genetic descent* and *environmental pressures* to the mix, on the grounds that there are multiple causes that hold a biological species in homeostasis in addition to interbreeding among members of that species (Wilson, Barker, and Brigandt 2009). Finally, the HPC account also makes room for the evolution of species, allowing that the properties associated with a kind can change, so long as there are mechanisms holding the kind in a state of equilibrium.

The HPC account of natural kinds would seem to posit something that any account of natural kinds should, namely the existence of a *causal mechanism* that holds together the properties associated with a kind. As Wilson, Barker, and Brigandt (2009, 199) put it, the mechanism ensures that these properties constitute a *cluster* rather than a mere *set*. Moreover, as they also explain, once the existence of the properties within the cluster is understood to spring from certain causal mechanisms, this assures us that the properties have not been associated with each other on artificial grounds, merely as a result of our predilections to lump certain properties together (Wilson, Barker, and Brigandt 2009, 198). The HPC account also has some additional benefits, which apply broadly to other cluster kinds, not just biological species. With respect to cluster kinds, the account provides a principled explanation for why some individuals should be considered members of the kind and others not. In the case of HPC kinds, the mechanism or mechanisms that hold those properties in place are crucial to the account and help determine whether an individual belongs to a kind. Individual organisms may lack some of the properties associated with a biological species, yet they may belong to the species nonetheless, since they are subject to the very same mechanisms that have led to the instantiation of those properties in other members of the species, including interbreeding, genetic descent, or environmental pressures. Still, the account itself cannot tell us which mechanisms a candidate individual must be subject to or how it should be causally affected by those mechanisms for it to be considered a member of the kind—nor should we be looking for such a panacea. That can be determined only by looking at the details of the case at hand by the scientific disciplines that study the case in question. Scientists (not philosophers) determine, based on their explanatory interests, which individuals belong to which kinds.

The HPC account of kinds has a number of strengths, not least because it lends greater credence to the viability of cluster kinds as natural kinds. But it also has some shortcomings. It is not that the HPC account of natural kinds never applies to natural kinds; it often does. However, I would argue that it would be a mistake to conclude that all natural kinds are HPC kinds. Typically, proponents of the view claim that the account pertains primarily to biology rather than to physics or chemistry, so it is not even meant as a complete account of natural kinds. But even in biology, the HPC account need not apply to a category for it to qualify as a natural kind. Although it is a useful framework for understanding why some kinds are natural kinds and is a convenient reminder of the causal dimension of natural kinds, it does not seem to fit many apparently natural kinds.

The application of the HPC account of natural kinds to biological species has been challenged by some philosophers of biology who find that it prioritizes similarity among members of a species as a criterion for species membership over descent from a common ancestor. Even though one of the homeostatic mechanisms cited by HPC theorists when it comes to species is genealogical descent, Ereshefsky (2010) thinks that the HPC account implies that what makes a species a kind is the similarity among its members, as opposed to descent or commonality of origin, whereas most biological systematists emphasize the latter.[12] When descent and similarity diverge, biological systematics chooses descent, whereas the HPC account opts for similarity, according to Ereshefsky.

The root of the problem is that HPC theory assumes that all scientific classification should capture similarity clusters. However, that is not the aim of biological taxonomy. Its aim is to capture history (Ereshefsky 2010, 676). Defenders of the HPC may respond by saying that this is true primarily of the cladistic approach to taxonomy and that other approaches to taxonomy also factor in other properties when classifying biological species. Cladists consider that speciation has occurred if and only if there has been branching in the phylogenetic tree (cladogenesis), whereas some other systematists assess speciation on the basis of gradual divergence of traits (anagenesis). On the latter view, speciation may occur without branching provided enough genetic mutations have occurred. In these cases, the cluster of genetic properties is considered more salient than the mechanism of evolutionary branching, according to some noncladistic systematists. Hence, it does not seem that the objection is fatal to the attempt to apply the HPC account to biological species, at least if one is not a strict cladist about taxonomy. Be that as it may, problems also afflict the HPC account when it comes to higher biological taxa, such as genera, families, classes, and so on. Here, the only serious candidate for a mechanism is genealogical descent (since interbreeding is out of the question and members of higher taxa are not generally subject to the same environmental pressures). But if that is the case, then it might seem as though there is no work left to do for the HPC. The kind is instead equated with a certain lineage in the phylogenetic tree, and any shared traits that exist among members, if indeed they do exist, are mere by-products of that common evolutionary history.

At this point, a natural modification of the HPC account might suggest itself, namely one in which a kind is identified with the mechanism that keeps it in homeostasis. As I mentioned previously, some proponents of the HPC account identify *genetic descent, interbreeding,* and *environmental pressures* as the causal mechanisms responsible for homeostasis in biological species. Boyd points out that, when it comes to biological species, "the homeostatic mechanisms important to the integrity of a species vary from species to species" (1999a, 170). Since the mechanism is supposed to be responsible for giving rise to the properties possessed by members, perhaps the causal mechanism corresponds to a "deeper" or "underlying" property that generates all the other properties. This may allow the HPC account to evade some of the criticisms of the account as it applies to biological species. Indeed, a curiosity of the HPC account of natural kinds is that some of its advocates consider it to be compatible with essentialism, indeed to be a form of essentialism, while others regard it as an alternative to essentialism. Griffiths writes, "The essence of a kind is its causal homeostatic mechanism—whatever it is that explains the projectability of that category" (1999, 212). He states that in equating essences with "causal homeostatic mechanisms," he is following Boyd (1991, 1999a). But Boyd himself does not think of these mechanisms in terms of the standard specification of essentialism.[13] Boyd states, "The natural kinds that have unchanging definitions in terms of intrinsic necessary and sufficient conditions . . . are an unrepresentative minority of natural kinds (perhaps even a minority of zero)" (1999a, 169).

If the HPC account is modified in such a way that the mechanism rather than the cluster of properties is taken to individuate the kind in question, we run into a different problem, namely that in the case of many natural kinds, there is no single mechanism that is causally sufficient for generating the properties associated with the kind (cf. Craver 2009). The HPC account considers the mechanism to be the cause and the cluster of properties to be the effect. But in many cases, the relationship between mechanisms and properties is not nearly so neat. There are biological kinds for which a homeostatic mechanism seems crucial and, as it were, holds the kind together. But there are other kinds for which there may be no single well-defined mechanism, or for which some of the properties associated with the kind cause others, or for which there is a self-sustaining process at work, as when properties present at one stage of development give rise to properties at another stage of development, which in turn give rise to the former properties in the next generation. This last type of relationship need not involve a metaphysically suspect type of "self-causation," just the familiar efficient causation operating across successive life cycles. Consider the process at work in maintaining the properties associated with the kind *larva*. The larva's adeptness at finding food is what (partly) causes the emergence of a mature imago, whose success at reproduction is what gives rise to the next generation's larvae, which in turn have traits designed for locating sources of food, and so on. Here, we do not seem to have a central causal mechanism that is responsible for a host of properties, but rather a set of self-sustaining causal property instances, which are scientifically important for inductive generalization and explanation.

It is not even clear that homeostasis is strictly necessary for the existence of a kind. Most species evolve, and the properties associated with them are not maintained in a strict state of equilibrium. As a result of mutation and natural selection, some of the properties possessed by members of a species are lost and others acquired, so there is a constant process whereby the properties associated with a species are altered (cf. Ereshefsky and Matthen 2005). Sometimes this leads to speciation and the emergence of a new kind altogether, but often the same species persists despite considerable divergence, and there is theoretically no upper limit on the extent to which members of a species might diverge from an ancestral form. The problem with a homeostatic account of species is that it seems to presuppose that there is some ideal or normal state that is being maintained by causal processes. But modern biology has disabused us of the notion of a "natural state model" of species, according to which "variability within nature is ... to be accounted for as a deviation from what is natural" (Sober 1980, 360). On that kind of outdated typological thinking, there is some natural type to which all specimens tend to converge and all specimens that do not conform to this type are deviations from the norm. This model has been rendered obsolete by one that regards variability among members of a species as being the norm itself rather than divergence from the norm. Positing a homeostatic mechanism in each species that tends to keep the properties in equilibrium is at odds with this way of thinking about species. Therefore, even when it comes to species, the paradigmatic biological kind, there are strong grounds for thinking that the HPC account is not a good fit.

Why not, then, give up on the idea that homeostatic mechanisms are centrally important to natural kinds? The account I am advocating does not necessarily require a homeostatic mechanism to be behind the properties associated with the natural kind, though it does retain the emphasis on causality. As Craver speculates,

> It is possible … to reject [the homeostatic mechanism] and to keep the rest as a *simple causal theory* of natural kinds. According to this view, natural kinds are the kinds appearing in generalizations that correctly describe the causal structure of the world regardless of whether a mechanism explains the clustering of properties definitive of the kind. (2009, 579; italics in the original)

The naturalist account of natural kinds already incorporates the causal component of the HPC, since inductive generalizations in science are ultimately underwritten by causal relations. Boyd himself makes this point well:

> Kinds useful for induction or explanation must always "cut the world at its joints" in this sense: successful induction and explanation always require that we accommodate our categories to the causal structure of the world. (1991, 139)

But the causal relations will be more variegated and diverse than the HPC account seems to permit. In some cases, the mechanism is separate from and is the common cause for the properties associated with the kind. In other cases, the mechanisms involved may be incorporated into the set of properties. In yet other cases, there may be nothing that deserves to be called a "mechanism" at all. In at least some of his formulations of the account, Boyd allows that when it comes to some natural kinds, the presence of some properties favors the presence of others, thus seeming to renege on the need for a homeostatic causal mechanism in all cases.[14] But if so, then the existence of a homeostatic mechanism is incidental and ought not to be the guiding principle of the account. In this vein, I am arguing that mechanisms need not be involved at all and that when they are, they need not be the cause of all the properties associated with the kind. Moreover, there does not seem to be a unitary account of the relationship between the properties associated with a kind that is applicable to all natural kinds.

The HPC account rightly draws our attention to the fact that there is a causal connection between some of the properties associated with a natural kind and others. If natural kinds are to play a role in inductive inference and serve the purposes of science, then they will be implicated in causal processes. Instead of a model whereby kind K is simply associated with some set of properties $P_1, \ldots, P_n$, we need to articulate an account according to which the projectibility of kind K is due to its figuring in certain causal relationships. However, that does not mean that there will always be some causal mechanism that holds the properties in the cluster together, or even that those properties are held together in a state of homeostasis.

6. CONCLUSION

I have argued, with Mill, that natural kinds are projectible categories in nonartificial scientific taxonomies. They can be used to infer numerous other properties, though not an inexhaustible number, as Mill seems to assume. Furthermore, Quine is right to say that the concept of similarity can be discarded in grouping individuals into scientific categories, since one can rely on identity of properties. But I argued against Quine in saying that these categories are the true natural kinds, as opposed to the folk categories that he thinks will be rejected in the fullness of time. Though folk taxonomies may sometimes identify natural kinds, as Dupré holds, these are then taken up by scientific inquiry. When folk taxonomies do not serve an epistemic purpose, they are not likely to be absorbed into science, nor should we consider their categories to be natural kinds. Finally, Boyd is right to emphasize the importance of causal relations to natural kinds, since causality is what holds together the properties associated with natural kinds. But I took issue with Boyd's idea that there is a single causal mechanism that maintains all these properties in a state of equilibrium, since the causal story is more complicated for many natural kinds.

The picture that emerges is a naturalist one, according to which natural kinds correspond to the categories posited by our best scientific theories. Some philosophers might react to this proposal by saying that it puts the epistemic cart before the metaphysical horse. But if we adopt a realist stance toward science, we thereby accept that the categories that science devises in order to understand nature provide the best insight into the kinds that really exist. The kinds that we arrive at as a result of the scientific enterprise are what enable us to discern the nature of reality. It is not that epistemology is driving metaphysics, but that the epistemic enterprise of science attempts to reflect the divisions in nature, and those divisions mark the boundaries between natural kinds. Furthermore, the projectibility of natural kinds, their role in inductive inference, and their explanatory and predictive value reflect the causal relationships in which they participate. But there does not seem to be a single causal template that fits all instances of natural kinds or relates all natural kinds to their associated properties.

NOTES

1. The phylum Chordata, which includes the vertebrates, has emerged as a more significant kind than the subphylum Vertebrata, which shows not only that science revises the categories that it introduces but also that it is always introducing new categories, either alongside or instead of existing categories.
2. I will not try to justify this claim here, but see Khalidi (1998b) for an argument against widespread incommensurability among successive conceptual schemes.

3. Another condition on real kinds, that there should be an "impassable barrier" between them, will not be discussed in this chapter.
4. According to Hacking (1991, 119), Peirce objects to this requirement on the grounds that part of the point of scientific inquiry is the derivation of certain properties from others as a matter of law. However, Mill can be charitably interpreted as saying that these other properties should not follow by logic alone or in a direct or trivial manner from other properties. Mill also says, "The properties, therefore, according to which objects are classified, should, if possible, be those which are causes of many other properties" ([1843] 1973, IV vii 2).
5. One notable respect in which Quine's account is at odds with most other philosophical accounts is that he regards natural kinds as extensional rather than intensional entities. He writes, "Kinds can be seen as sets, determined by their members. It is just that not all sets are kinds" (1969, 118). I will ignore this complication in what follows, since I take it as relatively uncontroversial that two (actually) coextensive sets may correspond to two genuinely different kinds.
6. This prediction of Quine's has been borne out by various measures of genetic distance that have been developed by geneticists. One of the simplest measures of genetic distance is based on the proportion of shared alleles summed over all genetic loci. This measure can be used for individuals, as well as for populations or taxa.
7. "Although the term tends to imply that there is a single cause for the illness, the common cold is actually a heterogeneous group of diseases caused by numerous viruses that belong to several different families" (Heikkinen and Järvinen 2003, 51).
8. Similarly, LaPorte (2004), who discusses the relationship of folk classification to scientific taxonomy, does not sufficiently investigate the reasons for deference and lack of deference, and ends up sending mixed signals on the issue. He says not only that the folk regularly defer to the scientists (2004, 31) but also that ordinary usage often persists and parts company with scientific nomenclature (2004, 68–69). He states that vernacular use is often adjusted to conform to science though not always (2004, 87–88), but nevertheless maintains that revision does seem to be the rule (2004, 89–90).
9. Dictionaries that have a separate entry (or subentry) for the loose usage of "fish" include the *American Heritage Dictionary of the English Language*, *Merriam-Webster's Online Dictionary* (11th edition), *Webster's New World College Dictionary* (4th edition), *Infoplease Dictionary*, and *Dictionary.com*. While the *Oxford English Dictionary* does not have two entries, it clearly distinguishes two senses of the term: "In popular language, any animal living exclusively in the water; primarily denoting vertebrate animals provided with fins and destitute of limbs; but extended to include various cetaceans, crustaceans, molluscs, etc. In modern scientific language (to which popular usage now tends to approximate) restricted to a class of vertebrate animals, provided with gills throughout life, and cold-blooded; the limbs, if present, are modified into fins, and supplemented by unpaired median fins."
10. http://www.fao.org/fi/glossary/default.asp
11. There is a further complication to this threefold classification of outcomes, which is nicely displayed by the examples cited. In the case of folk concepts adopted by science, sometimes the same term is retained but at other times a different term is introduced (as in the case of "consumption" and "tuberculosis"). Meanwhile, in cases in which a folk concept is retained alongside the scientific concept to serve some nonepistemic purpose, sometimes a different

term is used but at other times the same term is used and becomes ambiguous (as in the case of "fish").

12. Ereshefsky and Matthen (2005) also criticize HPC on the point that there is widespread dissimilarity among members of a species and that this is not just accidental but that it is central to any biological account of species. Indeed, some of the causal mechanisms in question are heterostatic in the sense that their job is to maintain variation in the population (e.g., dimorphism or polymorphism). Wilson, Barker, and Brigandt (2009) respond convincingly to some of their points.
13. Notably, Griffiths has excoriated "folk essentialism" in biology as follows: "Folk essentialism understands biological species as the manifestation of underlying 'natures' shared by all members of a species … Since folk essentialism is both false and fundamentally inconsistent with the Darwinian view of species, it should be rejected" (2002, 72). But what Griffiths objects to is not essentialism per se but a particular brand of it.
14. "Either the presence of some of the properties in [a family of properties] 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" (Boyd 1989, 16).

REFERENCES

Boyd, Richard. "What Realism Implies and What It Does Not." *Dialectica* 43 (1989): 5–29.

———."Realism, Anti-Foundationalism, and the Enthusiasm for Natural Kinds." *Philosophical Studies* 61 (1991): 127–48.

———. "Homeostasis, Species, and Higher Taxa." In *Species: New Interdisciplinary Essays*, edited by Robert A. Wilson, 141–86. Cambridge, MA: MIT Press, 1999a.

———. "Kinds, Complexity and Multiple Realization." *Philosophical Studies* 95 (1999b): 67–98.

Craver, Carl. "Mechanisms and Natural Kinds." *Philosophical Psychology* 22 (2009): 575–94.

Dupré, John. *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*. Cambridge, MA: Harvard University Press, 1995.

———. "Are Whales Fish?" In *Folkbiology,* edited by Douglas Medin and Scott Atran, 461–76. Cambridge, MA: MIT Press, 1999.

Ereshefsky, Marc. "What's Wrong with the New Biological Essentialism?" *Philosophy of Science* 77 (2010): 674–85.

Ereshefsky, Marc, and Mohan Matthen. "Taxonomy, Polymorphism, and History: An Introduction to Population Structure Theory." *Philosophy of Science* 72 (2005): 1–21.

Griffiths, Paul. "Squaring the Circle: Natural Kinds with Historical Essences." In *Species: New Interdisciplinary Essays,* edited by Robert A. Wilson, 209–28. Cambridge, MA: MIT Press, 1999.

———. "What Is Innateness?" *The Monist* 85 (2002): 70–85.

Hacking, Ian. "A Tradition of Natural Kinds." *Philosophical Studies* 61 (1991): 109–26.

Heikkinen, T., and A. Järvinen. "The Common Cold." *The Lancet* 361 (2003): 51–59.

Khalidi, Muhammad Ali. "Natural Kinds and Crosscutting Categories." *Journal of Philosophy* 95 (1998a): 33–50.

———. “Incommensurability in Cognitive Guise Categories.” *Philosophical Psychology* 11 (1998b): 29–43.
LaPorte, Joseph. *Natural Kinds and Conceptual Change*. Cambridge: Cambridge University Press, 2004.
Mill, John Stuart. *The Collected Works of John Stuart Mill*. Vol. 7 of *A System of Logic Ratiocinative and Inductive, Being a Connected View of the Principles of Evidence and the Methods of Scientific Investigation,* edited by John M. Robson. Toronto: University of Toronto Press, 1973.
Quine, Willard Van Orman. “Natural Kinds.” In *Ontological Relativity and Other Essays,* 114–38. New York: Columbia University Press, 1969.
Sober, Elliott. “Evolution, Population Thinking, and Essentialism.” *Philosophy of Science* 47 (1980): 350–83.
Wilson, Robert A., Matthew J. Barker, and Ingo Brigandt. “When Traditional Essentialism Fails: Biological Natural Kinds.” *Philosophical Topics* 35 (2009): 189–215.