NATURAL KINDS AND CROSSCUTTING CATEGORIES*

There are many questions that one can ask about categories in science and in common sense, and there are many ways of construing the claim that some categories are more "natural" than others. One can ask whether a system of categories is innate (for example, up/down) or acquired by learning (bourgeoisie/proletariat), whether it is theoretically based (vertebrate/nonvertebrate) or ad hoc (under one kilogram/over one kilogram), whether it pertains to a natural phenomenon (plant/animal) or to a social institution (legal/illegal), whether it is lexicalized in natural language (red/blue) or requires a compound linguistic expression (red-and-round/blue-and-square), and whether it is projectible (green/blue) or nonprojectible (grue/bleen). These distinctions are all relevant to the question of whether a certain system of categories is *natural* or *artificial.* The existence of such divergent construals of what makes a category or system of categories *natural* renders suspect any univocal answer to this question in any particular case.

Yet another question one can ask which some authors take to have a bearing on the issue of the *naturalness* of categories is whether a system of categories constitutes a unique way of organizing a particular set of entities or phenomena, or whether there are other legitimate classification schemes that can coexist with it. Another way of putting this is by asking whether systems of categories can cut across one another, and if so, under what circumstances. Some philosophers have claimed that crosscutting systems of categories cannot exist as genuine natural kinds. Richmond Thomason[1] has claimed that natural kinds are arranged in a hierarchy, such that higher categories in the taxonomic system do not trespass on the boundaries among the categories at the lower levels. In other words, two kinds can only overlap if one of those kinds is wholly subsumed under the other. So an individual can belong to two or more kinds, provided they can all be put in subsumption relations with each other. The

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[1] "Species, Determinates, and Natural Kinds," *Noûs*, III (1969): 95-101. For some criticisms, see my "Carving Nature at the Joints," *Philosophy of Science*, LX (1993): 100-13.

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natural-kind categories should form a nested hierarchy of categories which are *disjoint*, or, in my terminology, which do not *crosscut*.

Peter Gärdenfors[2] is another contemporary philosopher who has advocated the idea that natural kinds do not cut across one another. As part of an attempt to respond to Nelson Goodman's "new riddle of induction," he has argued that natural properties are those which do not crosscut our existing scientific conceptual scheme. Using the standard Linnaean taxonomy in biology as his illustration, he points out that the biological classification scheme can be represented by a tree, in which a property is natural only if it applies to all parts of the classificatory tree which lie below one particular node in the tree. Relative to this tree, all other properties are nonnatural. "For example," he writes, "the properties 'marsupial' and 'vertebrate' will be natural properties in the phylogenetic classification, while 'featherless' and 'biped' will not" (*ibid.*, p. 127). This position leads him to rule out grue-like properties as nonnatural, thereby enabling him to propose a solution to Goodman's riddle.

Similarly, Ian Hacking[3] has articulated the view that scientific categories are arranged in taxonomic hierarchies. While acknowledging that some scientific categories cut across others, he has posited that these crosscutting categories are not "real kinds" (a notion borrowed

[2] "Induction, Conceptual Spaces, and AI," in Douglas Stalker, ed., *Grue!: The New Riddle of Induction* (Chicago: Open Court, 1994), pp. 117-34. Gärdenfors's position raises a question as to the original provenance of our existing scientific conceptual scheme, which allegedly cannot be crosscut. His view is that it has an evolutionary origin. My concern here is not with whether Gärdenfors's move helps to solve Nelson Goodman's riddle, but rather with whether it is a reasonable way to distinguish natural from nonnatural scientific categories.

[3] "Working in a New World: The Taxonomic Solution," in Paul Horwich, ed., *World Changes: Thomas Kuhn and the Nature of Science* (Cambridge: MIT, 1993), pp. 275-310. Hacking cautions that some of Thomas Kuhn's writings upon which he is relying are marked: "Draft: not for distribution, quotation or paraphrase," and suggests that these may not be Kuhn's considered opinions (p. 307n). In what follows, I shall not be directly concerned with whether Hacking's is a reasonable construal of Kuhn's views, or of incommensurability, but with whether theories with crosscutting categories should generally be regarded as rivals, or merely as ways of organizing the same set of phenomena that are capable of coexisting. It is not clear whether Hacking endorses these views wholeheartedly or whether he expounds them merely on Kuhn's behalf. Be that as it may, it is not important that these views should be either Kuhn's or Hacking's, for they have considerable appeal for philosophers who are concerned to make the distinction between natural and nonnatural categories or kinds. Other philosophers have regarded this thesis as implicated in the notion of natural kinds. For a supporter, see Douglas Browning, "Believing in Natural Kinds," *South West Journal of Philosophy*, IX (1978): 135-48; Browning writes: "Things may be sorted into numerous overlapping types, but not into several natural kinds, unless they stand in a hierarchical arrangement, as of species to genus to higher genus" (p. 136). For a dissenter, see Ronald de Sousa, "The Natural Shiftiness of Natural Kinds," *Canadian Journal of Philosophy*, XIV (1984): 561-80.

from John Stuart Mill). Hacking uses this line of argument to explicate Thomas Kuhn's idea of incommensurable scientific theories. On his interpretation, the reason that some systems of categories are incommensurable with others is that they cut across each other. Since crosscutting is not allowed among scientific categories that are "real kinds," classification schemes that do crosscut are to be considered incommensurable rivals.

In addition to its role in explicating the notion of a natural kind, there are two connected reasons to examine the claim that bona-fide categories do not crosscut. The first is that this claim is tied to the further claim that there is one best classification scheme in each area of human inquiry. As I have already suggested, this assumption of uniqueness is clearly related to the idea that there cannot be alternative classification schemes that trespass upon one another. The second reason for focusing on this thesis is that the injunction against crosscutting is involved in one influential derivation of essentialism. The assumption that natural categories are hierarchical seems to be needed in arguing that each such category has a unique essence associated with it across possible worlds.[4] For all these reasons, the thesis that natural categories do not crosscut is in need of further elucidation and examination.

In what follows, I shall often refer to the position that I am opposing—that natural categories cannot crosscut one another—as the *hierarchic thesis*. Before proceeding, it is worth distinguishing a strong and a weak version of the hierarchic thesis. There are those who might argue, more weakly, that categories cannot crosscut only within a single theory, as opposed to across the entirety of science or our entire collection of theories. The authors mentioned do not seem to have this weaker claim in mind. For one thing, both Thomason and Hacking envisage (apparently independently) carving up the world into the categories: *animal*, *vegetable*, and *mineral*. This system of categories, which is supposed to pave the way for noncrosscutting subdivisions, does not pertain to a single theory. Having said that, there appears to be nothing objectionable about the weaker claim that some self-contained systems of categories fail to be crosscutting.[5] The Linnaean sys-

[4] I argue elsewhere (*op. cit.*) that the hierarchic thesis is implicated in an influential argument for essentialism. This suggests that the claim that natural kinds pick out essential properties is ultimately dependent on the idea that natural kinds are hierarchic.

[5] Although the weaker claim is prima facie plausible, there is a possible difficulty with it. Someone who makes the weak claim that categories cannot crosscut *within* a given theory can attempt to deal with purported counterexamples simply by pronouncing them to pertain to a different theory. If this move is not to be ad hoc, however, it must rely on an independently motivated account of the demarcation of theories, an account that is not readily available.

tem in biology is the most notable such system of nonoverlapping categories. Biologists and philosophers of biology commonly label such systems *hierarchies*; they are also sometimes referred to as *taxonomic systems*. 'Hierarchy' is the term used by George Gaylord Simpson[6] to refer to nonoverlapping classificatory schemes, and he reserves the term 'key' for a classification system that contains within itself crosscutting categories, though he adds that this is not the usual understanding of the word. I shall argue here that whereas hierarchies or taxonomic systems may exist within particular scientific theories, crosscutting abounds among the categories of science and lack of crosscutting is not a good criterion for the naturalness of a category.

I. CROSSCUTTING

Hacking has put forward a definition of a scientific *taxonomy*, which is his term for a hierarchy of nonoverlapping, nonsubdividing categories that culminate in a set of basic categories. He states that a taxonomy is determined by a class of entities and a transitive asymmetric relation. Such a pair, consisting of a class C and a relation K, is a taxonomy if and only if: first, C has a head, a member of the class that does not stand in relation K to any other member, but such that every other member stands in this relation to it; and second, every member except the head stands in the relation K to *some* other member. Hacking also says that such taxonomies sometimes terminate in a bedrock of basic categories that cannot be further subdivided (*op. cit.*, pp. 286-89). To make this more concrete, suppose that C is the class of biological taxa, and K is the relation of lower taxa to higher taxa—say, species to genus, or family to order. Then, C has two heads, the animal kingdom and the plant kingdom, neither of which stands in relation K to any other taxon. Moreover, every other taxon in the phylogenetic scheme is related to some higher taxon as species is to genus. The taxonomic claim is tantamount to the idea that scientific categories are arranged in a hierarchy, such that higher categories in the taxonomic system do not trespass on the boundaries among the categories at the lower levels. In Hacking's terminology, scientific categories cannot overlap; in my terminology, scientific categories cannot crosscut. I prefer to use the terminology of crosscutting instead of overlapping, since superordinate categories that wholly include others might be said to overlap with their subordinate categories but not to crosscut them.

Hacking conjectures that scientific categories belong to taxonomic hierarchies in order to explicate the Kuhnian claim of incommensu-

[6] *Principles of Animal Taxonomy* (New York: Columbia, 1961), p. 13.

rability.[7] As reported by Hacking, Kuhn wants to use the hierarchic thesis to explain why incommensurability arises. According to Hacking, whenever we have two scientific taxonomies, their categories either overlap one another, or the categories of one taxonomy subdivide the lowest categories of the other, or else their categories coincide. In the first two cases, overlapping and subdividing, Hacking uses the hierarchic thesis to rule out the possibility of translation. In the case of overlapping categories, there cannot be a translation between new and old, since they belong to different taxonomies. In the case of subdivision, the kind in the old science is a category with no scientific subkinds, "so the old name cannot be translated into any expression in the new science that denotes a scientific kind" (*op. cit.*, p. 295). Only in the third case, when categories coincide, can there be translatability.

Thus, Hacking offers a concrete reason as to why two rival scientific theories may not be translatable one into another or both into a common language by appealing to the notion of hierarchic scientific taxonomies. To support this claim further, he cites Fred Sommers[8] as having proposed a theory of predicates in natural language which is equivalent to this one. Sommers's theory of types holds that our ontological system is arranged in a hierarchy of nested types. Among the least inclusive types is the category of persons, which are the only things that can be *angry*, or can be *philosophers*. More inclusive is the category of physical things, which consists of the types of things that can be *red* or *heavy*. The most inclusive type is the category of things that can be *thought about*, or can be *interesting*, since these predicates can be applied to virtually anything in our ontology. Note that the entities included in each type or category are not the ones to which these predicates actually apply, but ones to which the predicates are applicable, which is to say that it would not be a category mistake to apply them.

[7] Although he uses it here to support Kuhn's views on incommensurability, Hacking's attitude toward the hierarchic thesis is more ambivalent in other work. In one article, he notes that "most of what we readily recognize as natural kinds do have this [taxonomic] feature," and goes on to ask whether this feature is "just part of our ancient predilection for natural history," or whether it is, more significantly, "somehow a fact about the functionally relevant groupings in nature"—see "Natural Kinds," in B. Barrett and R. Gibson, eds., *Perspectives on Quine* (Cambridge: Blackwell, 1990), pp. 129-41, here p. 133. Elsewhere, however, Hacking casts doubt on the related idea of a unique taxonomy of nature: "The idea of a complex exhaustive taxonomic framework does not make sense, not even as an ideal to which we strive"—see "A Tradition of Natural Kinds," *Philosophical Studies*, LXI (1991): 109-26, here p. 111.

[8] "Types and Ontology," *Philosophical Review*, LXXII (1963): 327-63.

Sommers's theory says that, if C_1 and C_2 are any two categories, then either C_1 and C_2 have no members in common or C_1 is included in C_2 or C_2 is included in C_1. That is, if a predicate is applicable to some entities of a certain type, and some of another type, then one of those types must be wholly contained within the other. No predicates are applicable to some entities of one type and some but not all of another type. Now, it might seem that 'tall' is such a predicate. Some things that can be true can be tall (for example, stories) but not all (for example, theories). At the same time, some things that can be tall cannot be true (for example, buildings). But this just shows that the predicate 'tall' is ambiguous when applied to stories and buildings. When the ambiguity is eliminated, 'tall' can be interpreted as two different predicates applicable to different types of entity. Eliminating the ambiguity eliminates the crosscutting.

Sommers's scheme appears to be corroborated by some empirical evidence presented by cognitive psychologists concerning language learning. If so, that would make the theory of hierarchic categories something more than a claim about the predilections of scientists and their ways of classifying things; it would make it a fundamental cognitive constraint on human classification schemes generally. That is how the psychologist Frank Keil[9] conceives it in his research on language learning in children. Keil thinks that the principle of not having crosscutting predicates is obeyed throughout the language-learning process by young children. Even though such children sometimes misapply predicates, they almost never consider predicates applicable to some members of a certain type and not to others of that same type. He postulates that only predicates that obey this principle are "psychologically natural." As in Sommers's scheme, whenever the constraint appears to be violated, there is some ambiguity in the predicates involved. Keil cites a variety of experiments to show that there is no crosscutting throughout psychological development, and these lead him to conclude that the principle of hierarchic types is "a constraint on human cognition" (*ibid.*, p. 165).

[9] *Semantic and Conceptual Development: An Ontological Perspective* (Cambridge: Harvard, 1979). The Sommers-Keil claim has been regarded by some cognitive psychologists as a basic constraint on language learning, but it has also met with criticism in those quarters. Susan Carey has come up with purported counterexamples to Keil's claim, but I shall not assess her conclusions, since for my purposes it is enough that the (stronger) hierarchic thesis is false, as I shall argue in the following section—see Carey, "Constraints on Semantic Development," in W. Demopoulos and A. Marras, eds., *Language Learning and Concept Acquisition: Foundational Issues* (Norwood, NJ: Ablex, 1986), pp. 154-71.

II. REAL KINDS

Sommers's theory of types does not strengthen the hierarchic thesis, and Keil's evidence from developmental psychology does not either. There is a crucial difference between the hierarchic account of taxonomy and the Sommers-Keil theory of types: the latter does not consider sets of things to which a predicate applies or fails to apply, but those to which it would or would not be *applicable*. A predicate is said to be applicable of something if and only if it would not be a category mistake to apply it, not if it would be merely false. Thus, 'tall' is applicable to both buildings and dwarves, though it is presumably only *true* of some buildings and no dwarves. Even if there is no crosscutting when it comes to applicability, there still may be when it comes to actual application. Sommers's claim is notably *weaker* than the hierarchic thesis because it relies on the notion of a category mistake rather than mere falsity.

While Sommers's constraint rules out the existence of two predicates that are applic*able* to crosscutting sets of things, the hierarchic thesis rules out the existence of two such predicates that truly apply to crosscutting sets. There is some plausibility to the claim that the counterexamples to crosscutting types are ambiguous (such as the predicate 'tall'), but it is easy to find nonambiguous counterexamples to the hierarchic thesis among the predicates of natural language. Consider the predicates 'red' and 'round': there is some overlap between the two sets, since some balls are both round and red, but there are also members of each set that do not belong to the other. Some nonround things are red (for example, fire engines) and some nonred things are round (for example, snowballs). This does not constitute a counterexample to Sommers's and Keil's constraint, since the Sommers-Keil theory would say that the predicates 'red' and 'round' are applicable to all three things, though they are not all correctly applied to all of them. The categories of natural language may display a hierarchical structure when it comes to applicability, but it is clear that they do not when it comes to application. Whatever the merits of the claim that the hierarchy of applicability constitutes a basic cognitive constraint, the hierarchic thesis does not have the same status when applied to the predicates of natural language. Still, it might be claimed that it holds for scientific predicates. So the question arises: Is it possible to save the claim by restricting it to genuine scientific predicates or categories?

The hierarchic thesis implies that an individual can belong to more than one scientific category, provided all these categories can be put in subsumption relations with each other. The claim is false, however, if taken to apply to scientific categories in general. To cite just one example, the category *parasite* crosscuts the category *insect*. Tapeworms and fleas are classified together as parasites, and fleas and flies are both classified as insects, but neither category, *parasite* and *insect*, includes all three. Hacking seems to admit that there can be scientific categories that crosscut one another without belonging to incommensurable theories. As an example, he gives the category *poison*, which "overlaps" the categories *vegetable* and *mineral* but is surely not incommensurable with them.[10] Although he recognizes that *poison* is a legitimate scientific category (after all, the scientific field of toxicology is based upon it), he argues that it is not a "real kind." He borrows the notion of a real kind from Mill in order to come up with a modified version of his original claim: while some scientific categories can be crosscutting, real kinds cannot. To qualify as a real kind, there must be an inexhaustible number of things to find out about a category. Whereas this is true of, say, *arsenic* in Hacking's opinion, it is not true of *poison*, since "There is nothing much common to poisons except what puts them in the class in the first place, namely the potential for killing people after being ingested" (*op. cit.*, p. 300). By contrast, he quotes Mill as saying that when it comes to real kinds, we discover "new properties which were by no means implied in those we previously knew" (*op. cit.*, p. 301). Hacking thinks this distinction between real and nonreal kinds has some application in science and defends it on independent grounds. But his reason for introducing it is that it enables him to explain why some categories from different theories are incommensurable: they are real-kind categories that belong to different taxonomic hierarchies. To put it differently, incommensurable theories are those which carve the world into real kinds that crosscut one another.

Does the notion of a real kind rescue the hierarchic thesis? Hacking admits that he does not have a proof that all real kinds belong to taxonomic hierarchies or that all taxonomic hierarchies contain real kinds. Nevertheless, he thinks that the notion of a real kind is a useful one and that hierarchic taxonomies "still carry some cachet" in the sciences (*op. cit.*, p. 303). But it is neither clear that the notion of a real kind is a useful one in science, nor that all exceptions to taxonomic hierarchies are nonreal. Crosscutting categories abound in science and they cannot all be dismissed as nonreal kinds. A notable

[10] It is unlikely that *vegetable* and *mineral* are genuine scientific categories, but I shall grant this for the sake of argument. If not, *poison* can be seen to crosscut the categories *organic* and *inorganic*.

one is the example mentioned earlier, the category *parasite*, which crosscuts the Linnaean tree in biological classification. It is clearly a real kind, because there are certain things that have been discovered to be common to all parasites that were not built into the category in the first place. To quote some typical scientific findings from a standard textbook on parasitology: "Appropriate triggering mechanisms initiate the change from infective stages to parasitic stages. Once the parasite has begun its existence in a new host body, other triggering mechanisms initiate each change of the parasite during its development."[11] Such information was not part of what was initially put into the category *parasite*. In fact, there are few scientific categories that have nothing more discovered of them than what was put in, and Hacking is right to be concerned that his claim will bring on accusations that he is committed to the analytic-synthetic distinction. A category that had such a feature would be tied to an irrevocable definition of the kind that does not survive in the context of inquiry. Moreover, it is quite clear that this cannot be regarded as a proper case of incommensurability, since these two taxonomies (associated with zoology and parasitology) coexist comfortably, whereas incommensurable theories are supposed to be rivals.

III. INTERESTS

Let us retrace our steps. The hierarchic thesis has been proposed by several authors as a characteristic of natural kinds or natural scientific categories. These writers have thought of natural categories as being arranged in a noncrosscutting hierarchy. Hacking has conjectured that a category that crosscuts such a hierarchic taxonomy is incommensurable with that system of categories. In response to purported counterexamples, such as the category *poison*, which crosscuts certain chemical categories without being incommensurable with them, he modifies the claim so that it applies only to real-kind categories (in Mill's sense). The revised claim is that real-kind categories that overlap with taxonomic systems of real kinds are incommensurable with those systems. The revised claim is also subject to counterexamples, however: the category *parasite* clearly conforms to the characterization of a real kind. The same could be said for the entomological categories—*larva, pupa*, and *imago*—that cut across the Linnaean species categories, or of the phase categories—*solid, liquid, gas*—that cut across the categories of the periodic table. Or, for that matter, the chemical categories—*acid* and *base*—that cross-

[11] E. R. Noble and G. A. Noble, *Parasitology: The Biology of Animal Parasites* (Philadelphia: Lea and Feibiger, 1982, 5th ed.), p. 7.

cut the categories *organic* and *inorganic*. All of these categories are real kinds in the sense that we discovered things about them which were by no means implied when they were first introduced. Far from being incommensurable, crosscutting schemes are capable of coexisting in our total theory of the world. To say that they are *capable* of coexisting is not to say that they always *do*. Some crosscutting categories may be rejected for the same reasons that scientific categories are generally rejected—but how and why this happens is a topic for another discussion. It is quite correct to say that crosscutting taxonomies cannot be translated into one another, but incorrect to think that they are incommensurable rivals. We should not expect crosscutting categories to be intertranslatable, any more than we should expect genetics to be translatable into cosmology.

But, it may be protested, there is a difference in the two cases: the relationship of genetics to cosmology is not the same as that of parasitology to zoology. There is still something of a problem in determining whether two theories are in competition with one another and can be translated into one another, or whether they merely coexist comfortably, though they share a subject matter. One needs to explain *why* there is no temptation in the case of parasitology and zoology to say that the two theories are incommensurable rivals, despite the fact that they are about some of the same entities. These two theories both concern living organisms, whereas genetics and cosmology do not. Why, therefore, are they not rivals? Their ability to coexist may be explained by saying that they embody different *interests* relating to a common subject matter. In the case of the Linnaean taxonomy, the governing interest is determining the diachronic-phylogenetic relations among organisms, while in the case of parasitology, the main interest is in determining synchronic-ecological relations among a subset of those organisms.

The idea that there are crosscutting taxonomies is closely related to the view that scientific classification is *interest relative*. If classification is always relative to certain interests, we would expect some taxonomies to reorganize some of the same entities in different ways without displacing existing ones. It is not entirely clear how to specify the exact role of interests in grounding crosscutting classification schemes, but this is what makes some of these schemes capable of cohabitation, unlike say classical mechanics and relativistic mechanics, which had roughly the *same* interests and were hence rivals. Therefore, one need not rest with the brute fact of failure of translation to explain why certain theories do not come into conflict when they have the same subject matter, whereas others do. This fact can be explained in terms of the interest relativity of scientific theories. Although it jars with some of his other claims, Hacking may also have something like this interest relativity in mind when he writes (in a parody of Karl Marx): "But, of course, I can act out different lives, work in different worlds, splice genes in the morning and go to the homeopath in the afternoon" (*op. cit.*, p. 298).

It might be objected that some *rival* theories do have different interests; indeed, that some theories are rivals precisely because they have different interests, and that one is sometimes replaced by another when the governing interests change. For example, it might be said that astronomy displaced astrology when we were no longer interested in using the motions of the stars and planets to predict human behavior and became more interested in these motions for their own sake. This is a misleading account of the relationship between astronomy and astrology, however, since these interests were intertwined for many centuries, during which time the two disciplines were effectively inseparable, until it became clear that the two sets of interests could be detached and the two disciplines eventually diverged. In fact, interest relativity may help to account for the persistence of astrology and to explain the fact that it has not been entirely supplanted by astronomy to this day.

Disparity of interests is also a feature that often distinguishes folk classifications from those of the experts. The relationship between common sense and scientific taxonomy is a topic that has elicited considerable discussion recently among philosophers, psychologists, anthropologists, and others. In the biological realm, it has been extensively explored by Scott Atran,[12] who argues that many folk taxonomies survive and exist comfortably alongside scientific taxonomies. This fact is often obscured, because we assume that the mere appearance of a scientific classification of a particular realm of inquiry leads automatically to the displacement of the common-sense classification. Atran argues persuasively that this is not always true historically, not even in the modern (supposedly scientific) era, since, even today, common-sense concepts in biology are not always identical with scientific concepts. Scientific categories and common-sense categories often coexist amicably without clashing. Thus, the transition from natural history to biology "involved not so much a radical rupture with common sense, as maintaining a continuing access through its reevaluation" (*ibid.*, p. 13). He also states: "If laypeople accept modification of a folk taxon, it is because the scientific taxon proves com-

[12] *Cognitive Foundations of Natural History* (New York: Cambridge, 1990).

patible with everyday common-sense realism; if not, the scientific concept can usually be set aside, and the lay notion persists as a 'natural kind' regardless" (*ibid.*, pp. 6-7). To this I would add that when scientific categories displace those of common sense, that is because the two subscribe to roughly the *same* interests. This proposal is borne out by cases in which common-sense classifications drop out and are replaced by the scientific ones.

Replacement of the folk taxonomy by the scientific one is more prevalent, for example, in medicine and the classification of diseases than in other areas. As Atran notes, most naive concepts of disease give way to sophisticated ones, rather than continue to exist alongside them. It may not be immediately obvious why diseases should be less resilient to scientific advances than other folk categories and why common-sense disease categories generally give way to scientific ones. Atran suggests briefly that concern with taxonomic nosologies (classification of diseases) is a specialized affair "by and large restricted to doctors and naturalists of the seventeenth and eighteenth centuries, and to twentieth-century ethnolinguistics and ethnomedicine. Most folk have no need or use for it" (*ibid.*, pp. 311-12). The fact that the treatment of disease in many cultures is left largely to experts may help to explain the relative ease with which many traditional folk diseases have dropped out and given way to scientific categories. But this does not really answer the question as much as postpone it. We can go on to ask: Why is the classification of diseases left up to the experts? Interest relativity can help answer this question: in this case, the purposes and interests of the experts are by and large the same as the folk, namely, the treatment of diseases, the alleviation of their symptoms, and the health of persons afflicted with them. Since the folk and the experts have identical or at least highly similar interests, we should not expect the folk taxonomies to survive in this case and that is why in some instances they are displaced by expert taxonomies, whereas in other cases they are coopted by the experts and further refined.

These proposals may also be related to some of the debates among philosophers of mind concerning the status of folk psychology and its eventual fate in the face of future developments in psychology. Some have argued that the folk theory will be eliminated by a scientific psychology, whereas others have insisted that it is not a rival to a science of psychology and will continue to flourish alongside it. Although that is not usually how it is framed, the central question may be put in terms of interests: Do these theories have the same or similar interests, or are they pursuing different interests? Some writers who have tackled the question have touched on this issue. Daniel Dennett's[13] distinction between three different *stances* that one can take toward the mind-brain can be viewed in this light (*ibid.*, pp. 16-17). What Dennett calls the physical, design, and intentional stances are, at least in part, ways of specifying different interests when it comes to the study of the mind-brain. From the physical stance, one predicts the behavior of a system by determining its physical constitution and the physical impingements upon it, then by using the laws of physics to predict the outcome for any input. From the design stance, the organism is treated as a device or artifact that behaves as it is designed to behave under different circumstances. From the intentional stance, by contrast, the organism is regarded as an agent that has formed rational beliefs about the environment and reasons about the world in conformity with those beliefs. Similarly, John Dupré[14] has argued that neurobiology, psychology, and folk psychology have different concerns and are interested in explaining different facets of human beings. He writes: "(Human) neurobiology, I believe, is a science concerned largely with how people work rather than with what they do; psychology has some important concerns with both kinds of question; and folk psychology is mainly concerned with understanding what people actually do" (*ibid.*, p. 152).

Therefore, crosscutting categories are not only rife among bonafide scientific classification schemes, they are also in evidence in comparing scientific taxonomies with folk taxonomies. In both cases, the reason that categories from different taxonomies can cut across one another without being in competition is that they pertain to different interests. In the concluding section, I shall attempt to use this notion of interest to introduce the notion of a scientific domain.

IV. DOMAINS

Some philosophers, scientists, and others have thought of crosscutting systems of categories as being mutually exclusive or as pertaining to rival classification schemes. I have argued that scientific categories routinely cut across one another without being rivals. In examining these issues, we have been led to an idea that deserves more attention than it has hitherto received in the philosophy of science. That idea concerns the role of interests in delimiting the area of inquiry for scientific theories. Scientific disciplines and subdisciplines may be distinguished not only by their subject matter, but also by the *interests* relative to which they conduct their inquiries. This

[13] *The Intentional Stance* (Cambridge: MIT, 1987).

[14] *The Disorder of Things: Metaphysical Foundations of the Disunity of Science* (Cambridge: Harvard, 1993).

idea has some relevance to current discussions of objectivity, which sometimes assume that the mere presence of interests jeopardizes the objectivity of science, and use the expression 'interest theory' to describe the social constructivist view of science. The above considerations make clear that interest-free science is not an option, since all scientific theories involve a set of interests relative to which the inquiry is conducted. In other words, no inquiry is literally dis*interest*ed. Rather, objectivity is partly determined by the extent to which one lives up to the interests that one sets out to serve. But it also involves ruling on the appropriateness of those interests in the first place; for example, we might question the interest of profit making if it is the main guiding factor in pharmacological research. These considerations do not resolve our worries about objectivity; there are still difficult questions to be addressed concerning the manner of determining the interests operable in any given enterprise, the individuation of those interests, and the judgment that any particular interest or set of interests is illegitimate in a certain area of research. But this way of looking at things may be helpful in tackling the problem of objectivity.

Most real examples from science will not be capable of neat resolution, and the interests involved will not be easily classifiable as appropriate or inappropriate. To take a thornier example, consider Donna Haraway's[15] claim that the science of animal groups or animal sociology has been "unusually important in the construction of oppressive theories of the body political" (*ibid.*, p. 11). She argues that around the time of World War II, scientists studying the behavior and social organization of primates saw their subjects in terms of dominance and other oppressive social relations. This attitude toward their subject matter derived from the experiences of these scientists and their interests, which Haraway claims were sexist and geared to promoting "scientific management of every phase of society" (*ibid.*, p. 13). Given this background, she argues that it was natural for these scientists to create categories (such as *dominance*) that tended to serve their interests. How might such biases be corrected? Haraway notes that these sciences have already been revised to some extent: for example, by switching the focus from using primates as models of human beings to a deeper look at the primates themselves. Moreover, she proposes that such sciences should be built on "social relations not based on domination" (*ibid.*, p. 19). This pro-

[15] *Simians, Cyborgs, and Women: The Reinvention of Nature* (New York: Routledge, 1991).

gression in the development of primatology can be understood in terms of a changing background of interests that evolved in the years since the World War II *away* from domination and social control and *toward* liberation and a more intrinsic interest in the primates for their own sake. Haraway advocates the alternative approach, while admitting that the alternative that she favors is also an "avowedly interested approach to science" (*ibid.*, p. 23). There is clearly more work to be done in showing that one set of interests is more appropriate than the other if we are to make further progress on the notion of objectivity in science. But casting the issue in terms of a changing set of interests helps us to avoid some of the quagmires often associated with discussions of objectivity and subjectivity.

It may seem as if the acceptance of the interest relativity of scientific theories will lead to the following problem. Suppose that we use it to salvage discredited theories by saying of such theories that they are not false but that they satisfy different interests. We may even specify these interests in such a way that makes the whole notion of interest relativity look suspect. For instance, a pharmacological theory might be defended by saying that it serves the interest of securing funds from a pharmaceutical company. In response, it should be pointed out that a mere appeal to interests cannot automatically save a discredited theory. Interest relativity is involved in picking out a system of categories, but those categories will still need to account for the phenomena and withstand the usual evidential, explanatory, and predictive tests. If another system of categories does the job better than the first, then we cannot simply alter our interests to save the first system. Among other things, we expect the categories that we develop to be nonvacuous, have clear criteria of application, feature in regularities, and figure in explanations. If a system of categories fails badly in all or some of these ways, it cannot be rescued simply by invoking interests. In fact, once a set of categories does not satisfy our usual standards, that may legitimately cause us to doubt the appropriateness of the interests with which the categories were picked out.

One implication of this discussion is that it is not enough to point to a set of entities or phenomena in order to specify the focus or area of inquiry of a particular discipline or subdiscipline. The domain of a theory is not picked out merely by adverting to a set of entities in some theory-neutral way, but also by the specification of certain interests relative to which one undertakes the inquiry. In fact, it is not even clear that entities can always be picked out in a theory-neutral way, since our interests are often instrumental in specifying what the entities or objects in a given domain will be.[16] From the microbiologist's point of view, there are no such entities as genes, since the chromosome is not divided neatly into discrete sequences, yet the geneticist finds it necessary to divide it thus. But since interests, stances, or perspectives seem insufficient on their own to pick out scientific domains, we need to specify an additional parameter. William Wimsatt[17] has appealed to the notion of "levels," by which he means "compositional levels—hierarchical divisions of stuff (paradigmatically but not necessarily material stuff) organized by part-whole relations, in which wholes at one level function as parts at the next (and at all higher) levels" (*ibid.*, pp. 5-6). For example, he states that neurons are composed of parts, such as membranes, dendrites, and synapses, which in turn are made of molecules, which are made of atoms, and so on. Levels "are constituted by families of entities usually of comparable size and dynamical properties, which characteristically interact primarily with one another..." (*ibid.*, p. 7). The temporal factor is also important, since processes at higher levels tend to take place at lower rates than processes at lower levels. This raises the possibility of being able to specify entities by further adverting to size and tempo, since the entities would appear to drop out once we indicate the spatiotemporal dimensions. As Wimsatt points out: theories come in levels "because that's where the entities are" (*ibid.*, p. 11).[18] In addition to levels, Wimsatt also appeals to a notion of "perspective," which is closely related to what I have been referring to as a set of interests: "Anat-

[16] A similar point is made in John Haugeland, "Pattern and Being," in B. Dahlbom, ed., *Dennett and His Critics* (Cambridge: Blackwell, 1993), pp. 53-69. In commenting on Dennett's notion of a stance (mentioned in the previous section), he writes: "But a stance is more than just an attitude toward or a perspective on things, more even than a method and terminology for dealing with them. Adopting a stance is *taking a stand.* Why? Because it is this alone—*commitment* to constitutive standards—that allows that toward which the stand is taken to stand out as phenomena, to stand over against us as objects. Such standards determine the *being* of the objects..." (p. 65). Similarly, I have argued elsewhere that two stances toward the mind issue in two different concepts of *concept*, ones that pick out different entities—see my "Two Concepts of Concept," *Mind and Language*, x (1995): 402-22.

[17] "The Ontology of Complex Systems," manuscript (University of Chicago, 1994).

[18] Wimsatt is reluctant to identify levels in a brute spatiotemporal fashion. Although he thinks that size is important to the determination of levels, he makes clear that size is not everything: "consider bacterium-sized black holes, which definitely would not exhibit Brownian motion" (*ibid.*, p. 10) But the difference in level here may be explained by a difference in temporal rates of interaction and development, since these are very different for black holes and bacteria.

omy, physiology, and genetics are different perspectives on an organism..." (*ibid.*, p. 19).[19]

Since interests are not sufficient to specify an area of inquiry, I propose to characterize a scientific *domain* as a *level* coupled with a set of *interests* (or a perspective, or a stance—for these purposes, I am treating these three notions roughly interchangeably).[20] For example, pharmacology and toxicology investigate some of the same chemical compounds and biochemical processes at the same spatiotemporal level, but whereas the interest of the first is the use of chemicals to make humans healthier, the interest of the second is to determine the properties of chemicals that have a tendency to harm humans. It is not immediately clear how to relate this mode of differentiating scientific inquiries to the more traditional ones. At first, it might seem that different disciplines investigate different spatiotemporal levels, whereas different subdisciplines investigate the same levels relative to different interests. But there are many exceptions to this rule. Some disciplines cut a swath through a wide range of levels, as in the case of physics. Within some subdisciplines, different interests may generate more than one classification scheme. In toxicology itself, poisons may be classified in terms of the target organ they af-

[19] Tough metaphysical issues crop up here concerning ontology and causality. I shall not attempt to address them in any detail, but they should at least be mentioned. *Ontology*: Should we construe scientific domains as ontological realms (domains of being) or merely explanatory ones (domains of description)? *Causality*: Do causal relations obtain in only one basic domain or do they obtain in all domains concurrently (the former threatens epiphenomenalism and the latter causal overdetermination)?

[20] Wimsatt would not endorse this characterization: "[L]evels come out as a kind of special case of perspectives on this analysis—a class of perspectives which map compositionally to one another so that their entities are related without cross-cutting overlaps in a hierarchical manner" (*op. cit.*, p. 19). But he says that there are also perspectives that are not levels: "They may fail to be levels because they are too small—they are located mostly at levels, but aren't of sufficiently broad span to count as levels. Or they may fail to be levels because, in a way, they are too big—or rather they cross-cut levels: they are transverse sections which do not include more than a small fraction of the phenomena at any given level..." (*op. cit.*, p. 19). It is more economical to identify levels in a neutral spatiotemporal fashion, however, and to go on to distinguish *domains* by coupling levels with perspectives (or sets of interests), as I have done in the text. That would enable us to avoid saying that levels are in some ways special cases of perspectives, in other ways larger than perspectives, and in yet other ways smaller.

Note also that Dudley Shapere has made some use of the notion of a scientific "domain" in a number of writings. But he does not incorporate interests in his domains; rather, he takes a domain to be a "body of information" to be investigated in a certain area or field, and contrasts it with the body of background information relevant to the domain—see *Reason and the Search for Knowledge* (Dordrecht: Reidel, 1984), Introduction.

fect, in terms of the chemical mechanism they exploit, in terms of their poisoning potential, in terms of their route of absorption into the body, and so on.

The image of the scientific enterprise that emerges from these considerations is of a patchwork of overlapping theories or taxonomies, some of which inquire into different spatiotemporal levels, whereas others inquire into the same level relative to different interests. It is futile to look for a single system of categories that uniquely classifies a certain set of scientific phenomena. For any given spatiotemporal level, there does not seem to be a limit on the number of taxonomic systems that may categorize it, so uniqueness is not a promising construal of what makes a system of categories "natural." Moreover, systems of categories that categorize the same level relative to different interests, as well as those which categorize different levels, are not incommensurable rivals, for they can coexist comfortably in a single scientific account of the world. What is the upshot of this discussion for the notion of a natural kind? As I remarked in the opening paragraph, naturalness can be interpreted in various different ways.[21] But the particular notion of natural kind examined here—which pertains to a system of categories that cannot be crosscut by another system of natural categories—should have been rendered suspect by the above discussion. If equally legitimate categories can crosscut one another, then there is no hope for the dream of a single overarching taxonomic hierarchy for the whole of nature. Therefore, this notion of natural kind is not in tune with real ways of classifying things and ought to be rejected, though some other notion of natural kind may yet be articulated.

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[21] Hacking has also pointed out on more than one occasion that there are many different notions of natural kinds—see, for example, "A Tradition of Natural Kinds," p. 122.