



## Philosophical Review

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Source: *The Philosophical Review*, Vol. 90, No. 1 (Jan., 1981), pp. 66-90

Published by: Duke University Press on behalf of Philosophical Review

Stable URL: <http://www.jstor.org/stable/2184373>

Accessed: 22/01/2009 07:43

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## NATURAL KINDS AND BIOLOGICAL TAXA<sup>1</sup>

John Dupré

The main topic of this paper is the theory of natural kinds that has been developed by Putnam<sup>2</sup> and Kripke.<sup>3</sup> One area to which this analysis has seemed particularly appropriate is that of general terms naming biological organisms. My strategy will be to compare the requirements of this analysis with some actual biological facts and theories. It will appear that these diverge to an extent which, I will claim, is fatal to the theory. Toward the end of the paper I will also make some more constructive remarks about the nature of biological classification.

In the first section of the paper I will outline the theory in question, particularly as it has been developed by Putnam, and touch on some related historical and contemporary issues. In the second section I will assume the interpretation of biological taxonomy most favorable to Putnam's theory, and show that even this is often not as Putnam needs it to be. In the third section I will move to a more defensible account of biological taxonomy that renders the theory increasingly untenable. In the fourth section I will make some more constructive remarks about the relations between different ways of classifying organisms, and in the fifth and final section I will discuss the nature of species. The account I will offer, I believe, lends support to the contentions of earlier sections.

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<sup>1</sup> I would like to thank Gordon Baker, who first suggested to me the philosophical interest of biological taxonomy, Nancy Cartwright and David Lewis, who made invaluable criticisms of earlier drafts of this paper, and most especially John Perry, who not only made many valuable criticisms of detail, but is also responsible for a much improved presentation of the entire paper.

<sup>2</sup> H. Putnam, *Mind, Language, and Reality*, II (Cambridge, 1975). Especially "The Meaning of 'Meaning'" (*MM*), "Is Semantics Possible?" (*SP*), and "Explanation and Reference" (*ER*).

<sup>3</sup> S. Kripke, "Naming and Necessity," in *Semantics of Natural Language*, ed. D. Davidson and G. Harman (Dordrecht, 1972), pp. 253-355. Also, "Identity and Necessity," in *Identity and Individuation*, ed. M. Munitz (New York, 1972), pp. 135-64.

## I

A good point of entry to the present issue is provided by Locke's theory of real and nominal essences. The distinction between real and nominal essence is, roughly, that between what accounts for the properties characteristic of a particular kind ("the being of anything whereby it is what it is"<sup>4</sup>), and the means whereby we distinguish things as belonging to that kind ("the abstract idea which the general, or sortal . . . name stands for"<sup>5</sup>). For something like a triangle, which Locke took to be a wholly conceptual object, the real and nominal essences coincide. Since the properties of a triangle flow only from the way it is defined, contemplation of the latter could provide insight into the former. But one point of the distinction was to emphasize the futility of the scholastic, contemplative view of science. Contemplation of forms, nominal essences if anything, would be a source of knowledge of real substances only if nominal essences were also real essences. But they are not, so it is not. In the case of material things Locke, like his successors, thought that the real essence was some feature of the microscopic structure; i.e., that the microscopic structure was the real source of the phenomenal properties of a thing, and that microstructural similarities accounted for the homogeneity of macroscopic kinds. Of the practical value of this notion, on the other hand, Locke was skeptical. Regretting, famously, our lack of microscopic eyes, he doubted whether knowledge of real essences was possible, and also whether real essences, if they were discovered, would coincide with the nominal kinds we had previously distinguished. Thus he held that sorts of things were demarcated by nominal essences only.<sup>6</sup> Subsequent scientific history has convinced some philosophers that Locke's skepticism was premature. Chemistry and physics have, since Locke's time, revealed a good deal about the microstructure of things, and antecedently-distinguished classes of things have proved to share important structural properties.

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<sup>4</sup> J. Locke, *An Essay Concerning Human Understanding*, ed. Fraser (Oxford, 1894), Vol. II, bk. III, ch. 3, 15, p. 26.

<sup>5</sup> Loc. cit.

<sup>6</sup> Op. cit., bk. III, ch. 6, 8, p. 63.

The contemporary theory I want to discuss may now be crudely stated in Lockean terms as follows: (1) real essences demarcate natural kinds; (2) such natural kinds provide the extension of many terms in ordinary language. The theory does not attempt to conflate real and nominal essence. As we will see, Putnam has a theory of meaning that incorporates, and sharply distinguishes, both real and nominal essence. But it is the real essence that is supposed to determine the extension of the term. It is with the feasibility of this role that I will be mainly concerned.

Henceforward, I will use the term "natural kind" to refer to a class of objects defined by common possession of some theoretically important property (generally, but not necessarily, micro-structural).<sup>7</sup> The traditional view, to which Locke may be counted a subscriber, is that terms of ordinary language refer to kinds whose extension is determined by a nominal essence, and hence not to natural kinds;<sup>8</sup> and that science, on the other hand, attempts to discover those kinds that are demarcated by real essences. It is compatible with this view that in some cases real and nominal kinds will coincide. But this would be largely fortuitous. This position does not require that ordinary language is entirely independent of science, for several reasons. First, the explanation of our recognition of a kind might, in some cases, trace back to a theoretical feature that defined a natural kind. Second, terms that originate in scientific theory may become incorporated in ordinary language; we should certainly not suppose that these are separated by a sharp or impassable boundary. And third, it is widely accepted that even the most straight-

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<sup>7</sup> It should be noted that the expression "natural kind" has sometimes been used in quite different ways. Quine, for instance, has used this expression in making the point that there are empirically discoverable distinctions in our "subjective quality space." (W. Quine, "Natural Kinds," in *Ontological Relativity and Other Essays* (New York and London, 1969), pp. 114-38.) These kinds, however, depend on the particular nature of human observers, and not necessarily on objectively significant properties of the objects. They might, perhaps, better be referred to as "innate nominal kinds." Also, the discussion of species in the final section of the present paper could justify referring to species as "natural kinds"; not, however, in the present sense.

<sup>8</sup> I use the term "nominal essence" here very broadly to include definitions, criteria, clusters of symptoms, etc. I do not mean to imply that every kind requires an essential property.

forwardly observational terms are to some extent “theory-laden,” though the exact extent of this is much debated. At any rate, the general picture is of science as a largely autonomous activity, in spite of subtle and pervasive interactions with the main body of language. It is one of the great attractions of Putnam’s essentialism that it promises to provide much stronger links between science and ordinary language, since many terms of the latter are shown to refer to kinds demarcated by the former.

Putnam’s theory resolves the meaning of a natural kind term into four components, referred to as a syntactic marker, a semantic marker, a stereotype, and an extension.<sup>9</sup> To illustrate, the term “elephant” might have as syntactic marker “noun,” as semantic marker “animal,” as stereotype “large gray animal with flapping ears, a long nose, etc.,” and an extension determined by the microstructural (or other theoretical) truth about elephants. It is with the last two of these, which are approximately equivalent to nominal and real essences, (the stereotype being the nominal essence, stripped of its reference-fixing function) that I will be concerned.

The distinction between the stereotype and the extension is reflected in a distinction between mere competence in the use of a term, and (full) knowledge of the meaning of the term. The former requires only the first three components of meaning. In fact, the stereotype is explained as the set of features that must be known by any competent speaker of the language, regardless of whether it provides a good guide to the actual extension of the term.<sup>10</sup> All this ignorant talk is facilitated by what Putnam describes as “the division of linguistic labour.”<sup>11</sup> If, for any reason, it is important that items be assigned to the correct classes, it is necessary that there be experts familiar with the really essential properties of the kinds in question, and who are therefore able to perform this function. We generally take it on authority, for instance, whether something is made of gold. We may note, however, that we can never be sure even that the experts fully know the meaning of the term. For there is no

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<sup>9</sup> *MM*, p. 269.

<sup>10</sup> *MM*, p. 250; *ER*, p. 204.

<sup>11</sup> *MM*, pp. 227–29.

guarantee that they have yet got right the real essence of the kind in question.

The central question raised by Putnam's analysis is how the nominal, or stereotypic, kinds of ordinary language are to be correlated with the natural kinds discovered by science. That is to say, granted that there are these real, empirically discoverable, natural kinds, how do we know which to assign to a particular term. Putnam answers this question by appealing to a previously unnoticed indexical component of meaning.<sup>12</sup> This consists in the reference, in using a natural kind term, to whatever natural kind paradigmatic instances of the extension of the term "in our world" belong. Such a paradigm may be identified either ostensively, or operationally through the stereotype. Having identified the paradigmatic exemplar, the kind is then defined as consisting of all those individuals that bear an appropriate "sameness relation" to this individual. This sameness relation is Putnam's exact equivalent of Locke's real essence. My fundamental objection to the theory as a theory of biological kinds is that no such sameness relations suitable for Putnam's theory can be found in it.

This concludes my exposition of Putnam's theory of natural kind terms. While I will argue that it is untenable, I should say now that there is much in it that I believe to be true. I am very ready to believe, in particular, that knowing the meaning of a term is something that admits of degrees, and that the higher degrees may only be achieved by experts. However, I do not think that experts can deliver on quite the task set for them by Putnam; and the task on which they can deliver, I think, is different in degree rather than in kind from what can be expected from a linguistically competent nonexpert. Before attempting to substantiate these claims, I will conclude the present section with a brief consideration of the arguments that have been adduced by Putnam (and Kripke) in support of this kind of theory.

The general methodology that Putnam adopts is to consider counterfactual situations in which we encounter an item that is in some interesting respect novel, and then to decide (intuit?) whether we would apply a particular term to it. The relevant

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<sup>12</sup> *MM*, pp. 229–34.

cases may be divided into two classes: first, those in which an object satisfies the stereotype, but for theoretical reasons is excluded from the extension, of a term; and second, those in which the theoretically important conditions are met, but part or all of the stereotype is not. I shall concentrate on cases of the first sort.

A favorite example of Putnam's is set in a place called "Twin Earth." This remarkable place is identical to Earth in every respect, except that what is there called "water," a substance that plays exactly the role that water does on Earth and shares all the phenomenal properties of Earth water, turns out not to have the chemical composition  $H_2O$ , but to be some other complicated chemical substance, which may be called XYZ. Putnam's contention is that when we discovered this fact we would have to say that what they called "water" was not water, since water is, necessarily,  $H_2O$ . Being  $H_2O$  is what constitutes the sameness relation for the natural kind, water. Since we have discovered that this is the appropriate sameness relation in our world, this fact has been incorporated in the very meaning of the term. The point I wish to emphasize here is a methodological one. If Putnam says "XYZ is not water," and my intuition is that it would be (another kind of) water, how is such a dispute to be settled? Who knows what we ought to say in such a fantastic situation? Of course, the claim that XYZ would not be water must itself be intuitively plausible if it is to support, not merely illustrate, Putnam's theory.

Perhaps it will be helpful to notice that scientific history encompasses similar, if less extreme, cases. Consider, for instance, the first European botanist to study North American trees. When he arrived he might have been interested to discover that there were beech trees on that side of the Atlantic. More careful investigation would have told him that these beech trees differed from those he had previously encountered and in fact belonged to a distinct species.<sup>13</sup> Since the most striking difference between the two species was (perhaps) the size of the leaves, this discovery was commemorated in the distinction between *Fagus sylvatica* and the newly recognized *Fagus grandifolia*. In view of overwhelm-

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<sup>13</sup> If it is objected that the concept of a species was very different when European botanists first reached America, I will make the modestly counterfactual assumption that America was first discovered in the 1970s.

ing similarities, there could have been little doubt about assigning these trees to the genus *Fagus* (= beech). Let us suppose that our botanist was also a linguist. If a native had asked him whether there were beech trees where he came from, what ought he to have said? My intuition, for whatever it is worth, is that he should have said that there were; though naturally if he were talking to a native *botanist*, he would go on to add that European beech trees belonged to a different species.

The purpose of this example is to suggest that plausible though some of Putnam's examples may be, they do admit of different interpretations. In the case of the water example it is also important to emphasize the great improbability of Putnam's hypothesis. All our scientific experience goes against the possibility of there being two substances that differed solely in having radically different molecular structures.<sup>14</sup> But this should not blind us to the fact that if we do take the possibility seriously, the best way of accommodating it might be to admit that there were natural kinds that encompassed such radical differences of structure. After all, it is surely just the absence of experiences like the one Putnam describes that makes it reasonable to attach to molecular structure at least most of the importance that Putnam ascribes to it.<sup>15</sup> Perhaps no one will be persuaded to take this case the way I have suggested. But I hope that I have at least said enough to motivate a closer look at how such issues are, and can be, treated in scientific practice.

## II

Putnam's theory requires that there be kinds discriminated by science appropriate for providing the extensions of certain kinds of terms in ordinary language. A very encouraging source of

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<sup>14</sup> Indeed, if there is really *no* other difference, it is impossible to conceive of any ground there could be for postulating this difference. This may be seen as an example of what Schlesinger has called the Principle of Connectivity. (G. Schlesinger, *Method in the Physical Sciences* (London, 1963), ch. 3, pp. 73-105.)

<sup>15</sup> Much of this importance may be attributed to the fact that Putnam is, or was, a reductionist. A classic statement of reductionist philosophy of science is Putnam and Oppenheim's "The Unity of Science as a Working Hypothesis," in *Minnesota Studies in the Philosophy of Science*, II, ed. H. Feigl and M. Scriven.

examples for this thesis is available in biology, and it is these examples that I want to consider. The part of biology that is concerned with the classification of biological organisms is taxonomy. Within taxonomy, an organism is classified by assigning it to a hierarchical series of taxa, the narrowest of which is the species.<sup>16</sup> Thus a complete taxonomic theory could be displayed as a tree, the smallest branches of which would represent species. Rules would be required for assigning individual organisms to species, and an individual that belonged to a particular species would also belong to all higher taxa in a direct line from that species to the trunk of the tree. (In practice, an organism is classified by assigning it to successively narrower taxa. But as will emerge, this does not reflect the theoretical relations of successive taxonomic levels.) Let us assume what might be called "taxonomic realism." This is the view that there is one unambiguously correct taxonomic theory. At each taxonomic level there will be clear-cut and universally applicable criteria that generate an exhaustive partition of individuals into taxa. Each individual will then have the essential properties of all the taxa to which it belongs. We may even assume that the appropriate number of taxonomic levels to recognize is somehow implicit in the nature of the organisms. The claim that there are natural kinds in biology demarcated by real essences (and *a fortiori* Putnamian privileged sameness relations) would thus be entirely sustained. My first aim will be to show that even under these circumstances Putnam's theory faces serious difficulties of application.

The central difficulty I have in mind is that it is far from universally the case that the preanalytic extension of a term of ordinary language corresponds to *any* recognized biological taxon. (Of course, I am not assuming that present biological theory includes the best possible taxonomy. But there can be no reason to anticipate a general trend towards coincidence with ordinary language distinctions.) In a sense this claim is not easy to substantiate, because the general terms in question are in fact extremely vague, and their application indeterminate. How-

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<sup>16</sup> For some purposes divisions into subspecies or varieties are required. For the present discussion these can safely be ignored.

ever, I think this indeterminacy can be seen to corroborate my thesis.

The richest source of illustrations for this difficulty is the vegetable kingdom, where specific differences tend to be much less clear than among animals, and considerable developmental plasticity is the rule. Any observant person who has explored the deserts of the Southwest United States will have little difficulty distinguishing a prickly pear from a cholla. Yet taxonomically both these kinds of cacti belong to the same genus, *Opuntia*. Several species of this genus are certainly (to the ordinary man in the desert) prickly pears, and several are certainly chollas. Taxonomy does not recognize any important relation between *Opuntia polyacantha* and *Opuntia fragilis* (two species of prickly pear) that either does not share with *Opuntia bigelovia* (a species of cholla). Ordinary language does make such a distinction, and on the basis of perfectly intelligible and readily perceptible criteria. Thus the property of being a prickly pear is just not recognized in biology.

Or consider the lilies. Species which are commonly referred to as lilies occur in numerous genera of the lily family (Liliaceae). To take a few examples from the flora of the Western United States again, the Lonely Lily belongs to the genus *Eremocrinum*, the Avalanche Lily to the genus *Erythronium*, the Adobe Lily to the genus *Fritillaria*, and the Desert Lily to the genus *Hesperocallis*. The White and Yellow Globe Lilies and the Sego Lily belong to the genus *Calochortus*; but this genus is shared with various species of Mariposa Tulips and the Elegant Cat's Ears (or Star Tulip). I would not want to undertake the task of describing the taxonomic extension of the English term "lily." However, it is fairly clearly well short of including the entire family. To include the onions and garlics (genus *Allium*, and, incidentally, another good example of the point of the previous paragraph) would surely amount to a debasement of the English term.<sup>17</sup>

It is not hard to find similar examples in the animal kingdom. The various species of chickadees and titmice share the same

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<sup>17</sup> All the preceding examples may be found in R. Spellenberg, *The Audubon Society Field Guide to North American Wildflowers, Western Region* (New York, 1969).

genus. Hawks probably comprise three of the four families in the order Falconiformae, though there are some questionable sub-families. Whether a kite, an eagle, or a caracara is a hawk is another futile debate I will not attempt to initiate, though I feel sure that a vulture is not. Moths are another particularly interesting example. The order Lepidoptera includes the suborders Jugatae and Frenatae. It appears that all the Jugatae are moths. The Frenatae, on the other hand, are further subdivided into the Macrolepidoptera and the Microlepidoptera. The latter seem again to be all moths. But the former include not only some moths but also (all) skippers and butterflies.<sup>18</sup> In this case it does seem possible to give a plausible account of the taxonomic extension of the English word. The trouble is that the grouping so derived appears to be, from the taxonomic point of view, quite meaningless.

A rather desperate attempt might be made to save the theory from such examples, by going for the best available taxon and accepting some revisionary consequences for ordinary language. Thus one might claim that the extension of "lily" was the whole family Liliaceae, or of moths the order Lepidoptera. We would just have to accept the fact that onions had turned out to be lilies, or butterflies moths. In defense of such claims, it could be pointed out that ordinary language has indeed come to accept such scientifically motivated changes as the rejection of the view that whales are fish in favor of the belief that they are mammals. But actually this example is by no means as clear-cut as is sometimes assumed. In the first place, "mammal" is more a term of biological theory than of prescientific usage. One cannot recognize mammals at a glance, but must learn quite sophisticated criteria of mammalhood. "Fish," by contrast, is certainly a prescientific category. What is more doubtful is whether it is genuinely a postscientific category, for it is another term that lacks a tidy taxonomic correlate. I assume that the three chordate classes Chondrichthyes, Osteichthyes, and Agnatha would all equally be referred to as fish (unless sharks and lampreys are just as good nonfish as whales). But unless there is some deep scientific reason for lumping these classes together but excluding the class Mam-

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<sup>18</sup> For moths, see D. Borror and R. White, *A Field Guide to the Insects of North America North of Mexico* (Boston, 1970), pp. 218 ff.

malia, the claim that whales are not fish might be a debatable one. Perhaps "fish" just means aquatic vertebrate, so that whales are both fish and mammals, and this well-worn example is just wrong. However, whales were never the most stereotypical fish, and it is easy to see the point of denying that they are fish at all: they do belong to a taxonomically respectable group most members of which do not remotely resemble fish. I see no parallel argument for the claim that butterflies are moths.

The second difficulty for the application of Putnam's theory that occurs even against a background assumption of taxonomic realism, concerns the hierarchical structure of taxonomy. Putnam's theory, it will be recalled, determines the extension of a natural kind term by means of a theoretical "sameness relation" to a suitable exemplar. Suppose we want to discover the extension of the English word "beetle." A suitable exemplar will no doubt have to satisfy the condition that it be readily recognizable as a beetle by a linguistically competent layman; but probably this would not eliminate a very large proportion of the approximately 290,000 recognized species. Any particular exemplar will belong to one particular species. Given taxonomic realism, there will then be some sameness relation that it displays to other members of that species, some relation that applies within its particular genus, and so on up, not just to the relation that holds between all members of the order Coleoptera, which is approximately coextensional with the term "beetle," but beyond, as far as the relation that holds between it and all animals but no plants. One may well wonder how the appropriate sameness relation is supposed to be selected from these numerous alternatives.

One kind of solution to this difficulty does suggest itself. If we collected a sufficiently large number of beetles, as different from one another as was consistent with the stereotype, we could try to find the narrowest sameness relation that held between every pair of our specimens. This methodology would, of course, force us to identify moths with the order Lepidoptera, and accept the consequence that butterflies were a kind of moth. It also seems to me that collecting the set of samples would involve attaching a lot of significance to the stereotype; if the stereotype were not a good guide to the real extension, it could hardly

work. Rather than pursue this suggestion, however, I will now take a more critical look at taxonomic realism.

### III

I have not meant to deny that very many general terms for living organisms do have a reasonably clear taxonomic correlate. But to investigate the extent of this correlation, it is first necessary to say something about the word "ordinary." For almost all species of birds and large vertebrates, for many flowering plants, and for some species of fish and insects, there is something (or sometimes a list of things) referred to as a common name. It is not obvious whether these should be thought of as part of ordinary language, or as part of a technical vocabulary. Certainly if competence in English does not require enough biological knowledge to distinguish a beech from an elm,<sup>19</sup> then surely it cannot require an awareness even of the existence of the Solitary Pussytoes, the Flammulated Owl, or the Chinese Matrimony Vine. If such charming terms are assigned with their Latin equivalents to scientific taxonomy, and we restrict our attention to terms with which the layman can reasonably be expected to be familiar, then one thing we will find is that where there is a recognizable corresponding taxon, it is generally of higher level than the species.

For the case of large mammals, where human interest (and empathy) is at its highest, most familiar terms do refer to quite small groups of species; and common specific names are often widely known (as Blue Whale, Indian Elephant, or White-Tailed Deer). Most well-known names of trees refer quite neatly to genera, as, e.g., oak, beech, elm, willow, etc. (The various cedars, by contrast, are not closely related. It is reasonable to suppose that the term "cedar" has more to do with a kind of timber than with a biological kind.) With birds the situation is highly varied. Ducks, wrens, and woodpeckers form families. Gulls and terns form subfamilies. Kingbirds and cuckoos correspond to genera, while owls and pigeons make up whole orders. The American Robin, finally, is a true species, though it is interesting

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<sup>19</sup> See *MM*, pp. 226-27.

that in Britain “robin” refers to a quite different species, and in Australia, I am told, it refers to a genus of flycatchers. For insects, where the number of species is much greater, and the degree of human interest generally lower, the mapping is predictably coarser. Such things as hump-backed flies, pleasing fungus beetles, brush-footed butterflies, and darkling beetles make up whole families (the last-named, for instance, having some 1,400 known North American species). More familiar things, like beetles and bugs, refer to whole orders. (Must the competent speaker of (American) English know that a beetle is not a bug? Or is the word “bug” ambiguous?)

The significance of the preceding point is that whereas there is an interesting case to be made for the reality of the species, there seems to be almost no case for taxonomic realism at any higher level of classification. Among biologists, “lumpers” and “splitters” do indeed dispute such questions as how many genera are to be distinguished within a family. Such disputes may be based on estimates of morphological or physiological similarity within groups of species, or on considerations of practical utility for field classification; they do not appear to involve deep theoretical interests, or to embody the assumption that such questions admit of true or false answers. (There is a possible claim that such distinctions reflect phylogenetic matters of fact, but I will postpone consideration of this suggestion.)

It will be recalled that Putnam’s theory requires that there be some sameness relation between any two members of a natural kind. This might be called a “privileged sameness relation” since it is not supposed to be just any relation that happens to demarcate the kind, but rather some discoverable relation that constitutes the real nature of that kind. But biological theory offers no reason to expect that any such privileged relations exist, since higher taxa are assumed to be arbitrarily distinguished and do not reflect the existence of real kinds. This claim will be reinforced in the final section of this paper, where I will argue that even for the case of species no privileged sameness relations exist. Since this is a rather more controversial question, however, I should emphasize that I do not think the argument against Putnam depends in any way on this question. For as I have

indicated, a species is seldom a candidate for the extension of an ordinary language term.

To clarify my position on the relation between the species and higher taxa, I actually believe that the species is the only taxonomic level to which essential properties cannot be attributed. This is not meant as a paradox. It is merely that higher taxa, having no real existence, are defined in scientific vocabulary by nominal essences. Thus I would hold that such statements as that birds have feathers, mammals suckle their young, or spiders have eight legs are analytic.<sup>20</sup> But a nominal essence is not, in Putnam's sense, a privileged sameness relation. The reason that the same cannot be said of the species is that species, while lacking a real essence, do have a kind of real, objective existence. This will be explained in the final part of the paper. Meanwhile, there is one technical difficulty that may be raised against this account. Unless species have at least those properties that are essential to the higher taxa under which they are subsumed, it would appear that the relation between the species and the higher taxa cannot be subsumption. It might be sufficient to reply that a real essence is intended as a condition both necessary and sufficient for membership in a taxon, and this argument only shows that there are some necessary conditions. Since I am reluctant to admit that there are even strictly necessary conditions for species membership, I prefer a different line. One may assume that species are assigned to higher taxa *in toto*, because a sufficient majority of their members display the appropriate properties. (Thus, for instance, women who feed their infants from bottles would still count as mammals, since they belong to a species of mammals.) Then one would fail to assign an individual to a higher taxon only in case one failed to assign it to a species. This does not seem an objectionable failure.

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<sup>20</sup> This statement is considerably oversimplified. More strictly, we should say "anything that has feathers is a bird," etc., to accommodate plucked birds, male mammals, and paraplegic spiders. However, "anything that has eight legs is a spider" is neither analytic nor true. Further conditions that eliminate octopuses, crabs, etc., would be needed to construct a genuinely sufficient condition. Presumably the definition of a high level taxon will typically be quite complex, and perhaps sometimes disjunctive.

IV

In this section I will make some more constructive suggestions about the relationship between the classifications of organisms in ordinary language (OLC) and in scientific taxonomy (TC). The natural way to contrast these classificatory schemes, it seems to me, is in terms of the different functions that they serve.

The functions of OLC, unsurprisingly enough, are overwhelmingly anthropocentric. A group of organisms may be distinguished in ordinary language for any of various reasons: because it is economically or sociologically important (Colorado beetles, silkworms, or Tsetse flies); because its members are intellectually intriguing (trap-door spiders or porpoises); furry and empathetic (hamsters and Koala bears); or just very noticeable (tigers and giant redwoods). This list could no doubt be extended almost indefinitely, which merely reflects the immense variety of human interests. From this standpoint many apparent anomalies between OLC terms and TC terms are readily explicable. An example I mentioned earlier is illustrative here. It would be a severe culinary misfortune if no distinction were drawn between garlic and onions. But we have seen that this is not a distinction reflected in TC. Presumably there is no reason why taxonomy should pay special attention to the gastronomic properties of its subject matter.

A slightly more elaborate example is the following. The taxonomic classes birds and mammals are both part of ordinary language (though the latter less clearly). By contrast, the much larger class of angiosperms (flowering plants) receives no such recognition. There is a very familiar term of ordinary language, "tree," the extension of which undoubtedly includes oak trees and pine trees (though perhaps not their seedlings). The extension of the TC term "angiosperm," on the other hand, includes daisies, cacti, and oak trees, but excludes pine trees. It is no surprise that such a grouping finds few uses outside biology; for most purposes it is much more relevant whether something is a tree or not than whether its seeds develop in an ovary. This seems sufficient to explain why there is no taxonomic equivalent of "tree" and no ordinary language equivalent of "angiosperm."

Where organisms are of little interest to nonspecialists, they

are typically coarsely discriminated in OLC. Thus it is that despite the vastly greater number of arthropod than vertebrate species, OLC distinguishes many more kinds of the latter. The factors I mentioned before may all apply here. Vertebrates are more likely to be useful (nutritious), interesting (empathetic), furry (useful), noticeable (big), etc. Thus arthropod classifications in OLC typically cover enormous numbers of species. In fact, the useful distinctions tend to be more on the model of "small red beetle" and "large black beetle," than of specific identification. Still with this functionalist viewpoint in mind, we can also see that there may be other, specialized vocabularies that do not coincide with either TC or OLC. The vocabularies of the timber merchant, the furrier, or even the herbalist may involve subtle distinctions between types of organisms; there is no obligation that these distinctions coincide with those of the taxonomist. (Recall, for instance, my earlier suggestion about the term "cedar.")

TC, hopefully, avoids this anthropocentric viewpoint. The number of species names is here intended to reflect the number of species that exist. Nonetheless, even here there is an anthropomorphic aspect. For an adequate taxonomy must not only meet theoretical constraints, but should also be practicably usable. The strongest theoretical constraints apply at the level of the species, for the obvious reason that this is the level with the greatest theoretical significance. Thus it has recently been recognized that a large number of groups that had been taken for species were in fact groups of very similar but distinct species.<sup>21</sup> There is no requirement that taxonomy must be easy.

A taxonomic system is not merely a list of species, but must also include a selection of features by which they are to be recognized. Such features may be called "diagnostic." If it were possible to discover some privileged sameness relation for species, then clearly this relation should be used as diagnostic for the species. In the final part of this paper I will consider and reject some candidates for such a relation. For now I will assume that the existence of a species consists in the general cooccurrence

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<sup>21</sup> For a discussion of these so-called "sibling species," see E. Mayr, *Populations, Species, and Evolution* (Harvard, 1975), ch. 3.

of a large number of characteristics. If this is right, then the selection of diagnostic features must be greatly underdetermined, and hence, in a sense, arbitrary. Of course, there will be certain desiderata for such a choice, such as minimal developmental plasticity, or just ease of determination. In practice, a suitable feature or set of features is generally taken as providing a conclusive identification. But this should not be taken as showing that the features selected are privileged. And indeed, a slight acquaintance with field biology suggests that even the best selected diagnostic features will occasionally fall foul of atypical specimens or obscure hybrids.

If the contrast I have suggested between species and higher taxa is well founded, it would be misleading to apply the term “diagnostic feature” both to species and to higher taxa. For the latter, a better term would be “defining feature.” As for the species, the fact that such a feature is not dictated by discoverable properties of the objects does not imply that there are no appropriate standards for selecting defining features. Maximal evolutionary invariability is one desideratum that comes to mind.

The position I would like to advocate might be described as promiscuous realism.<sup>22</sup> The realism derives from the fact that there are many sameness relations that serve to distinguish classes of organisms in ways that are relevant to various concerns; the promiscuity derives from the fact that none of these relations is privileged. The class of trees, for example, is just as real as the class of angiosperms; it is just that we have different reasons for distinguishing them. It is true that in the case of species there is a largely, though not wholly, determinate range of classes that we are aiming to identify. The existence of species, I suggest, may be seen as consisting in the following fact. If it were possible to map individual organisms on a multidimensional quality space, we would find numerous clusters or bumps. In some parts of biology these clusters will be almost entirely discrete. In other areas there will be a continuum of individuals between the peaks. It can then be seen as the business of taxonomy to identify these peaks. This picture also makes it easy to see why the deliverances of taxonomy need not provide the distinctions that are

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<sup>22</sup> I am grateful to John Perry for suggesting this term.

relevant for more specialized interests. As is demonstrated by the existence of sibling species, the properties that covary in a species and distinguish it from other similar species may be very subtle (at least subtle enough to have escaped biologists for a long time). When the classificatory problem is approached from a more restricted point of view, that is, with an interest only in a certain range of properties, many peaks will disappear, while others may be emphasized. As an example of the former, analysis of the vocalizations of frogs have revealed numerous sibling species. But this hardly need be a matter of concern to the gourmet unless there are also variations in the texture or flavor of frogs' legs. Again, the gourmet puts more emphasis on the distinction between garlic and onions than is implicit in taxonomy. Even within biology different interests call for the emphasis of different distinctions. Thus the primary unit of significance in ecology is not the species but the population.

## V

In this concluding section I will defend the claim that privileged sameness relations cannot be found for the demarcation of the species.<sup>23</sup> At the same time, I hope to lend support to the positive characterization of the nature of species that was outlined in the previous section. While this account is certainly important for the general metaphysical position that I have just sketched, I should make clear that I do not think that the earlier claims about the relation between taxonomy and ordinary language depend upon the success of the present enterprise. These claims I take to be sufficiently established by the arguments adduced in Sections II and III of this paper.

I will now review and criticize three strategies that might be attempted for identifying privileged sameness relations between

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<sup>23</sup> The nature of species has received some discussion in the philosophical literature. For a view quite close to that presented here, see M. Ruse, "Definitions of Species in Biology," *British Journal for the Philosophy of Science*, 20 (1969), 97-119. See also, e.g., H. Lehman, "Are Biological Species Real?" *Philosophy of Science*, 34 (1967), 157-67; D. B. Kitts and D. J. Kitts, "Biological Species as Natural Kinds," *Philosophy of Science*, 46 (1979), 613-22.

the members of a species. These strategies are based, respectively, on intrinsic properties of the individuals, on reproductive isolation of a group of individuals, and on evolutionary descent of a group of individuals. They will be considered in that order.

A traditional assumption that dates back at least to Aristotle is that organisms could be unambiguously sorted into discrete kinds on the basis of overt morphological characteristics. Since the theory of evolution undermined the belief in the fixity of species, this assumption has become increasingly untenable. It is now widely agreed that gross morphological properties are not sufficient for the unambiguous and exhaustive partition of individuals into species.<sup>24</sup> Crudely, this is because there is considerable intraspecific variation with respect to any such property, and the range of variation of a property within a species will often overlap the range of variation of the same property within other species.

At the same time it is still sometimes thought that a more covert, probably microstructural, property could be discovered that would be adequate for the unambiguous assignment of individuals to species.<sup>25</sup> More specifically, it may be thought that some description of the genetic material could capture a genuinely essential, or at least privileged, property.<sup>26</sup> It is assumed that the morphological and physiological properties are causally conditioned by interaction between the organism's genetic endowment and its environment. Thus it is imaginable that all members of a species do share the same genetic blueprint, or one with certain essential features, but that intraspecific differences are attributable to differences in environmental factors. But it is equally possible that there should be as much or more genetic variability as morphological variability. That is, intraspecific genetic variability may overlap interspecific variation as much as, or more than, morphological variability does. In fact, there are good reasons for supposing this to be the case.

There are various reasons why evolution should favor species with a high degree of genetic variability. In the first place, a

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<sup>24</sup> For a much more detailed discussion of this fact, see Mayr, *op. cit.*, ch. 2.

<sup>25</sup> Kitts and Kitts, *op. cit.*, argue that there must be such a property, while admitting that we do not yet know what it is.

<sup>26</sup> E.g., by Putnam. See *SP*, p. 141.

reserve of genetic variety may enable the species to survive changing environmental conditions. A species, in other words, may be able to produce individuals suited to a variety of environmental situations. Second, it appears that heterozygous individuals (i.e., individuals with pairs of different genes at various loci) are often better adapted than homozygous individuals. (The more invariant the genetic material, of course, the less heterozygosity is possible.) A classic example of this is provided by sickle cell anaemia. Only those individuals that are heterozygous with respect to this gene are able both to produce viable blood cells, and to exhibit a high resistance to a form of subtertian malaria which is prevalent in those areas where the gene in question is commonest. More generally, it is supposed that heterozygosity provides a way of increasing the diversity of the biochemical resources of an individual. Finally, it is believed that there are homeostatic developmental mechanisms whereby differing gene combinations approximate the production of the same phenotype.<sup>27</sup> This last point both accounts for the possibility that genetic variation might exceed phenotypic variation, and also emphasizes why it would be mistaken to suppose that the genetic material was in any way privileged with respect to intraspecific homogeneity. Of course, there are other microstructural features that could be supposed to be especially favored in this respect, such as the presence of particular proteins, lipids, or whatever. But there is no reason to expect that any such properties enjoy a privileged status with respect to variability.

Much importance is attached in theoretical biology to the notion of reproductive isolation. It is suggested that a species can be defined as a group of interbreeding individuals, reproductively isolated from all other individuals; this is often referred to as the "biological" species concept, and may be considered a second candidate for providing a privileged relation between members of a species. Set against the desirability of genetic variation, there is a need for a species to maintain the integrity of a well-adapted gene pool. This requires insulation against the introgression of alien genes. Furthermore, it is generally sup-

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<sup>27</sup> See Mayr, *op. cit.*, p. 133. Not surprisingly, sibling species are particularly likely to display strong developmental homeostasis.

posed that the process of speciation is not completed until effective mechanisms have been established to prevent such introgression.<sup>28</sup> Thus there is a certain sense in which reproductive isolation is an essential property of the species: the species would not have come into existence if it had not, to a sufficient degree, acquired this property. The important point here is that this is a property of the species, or gene pool, but only secondarily of the individuals that make up the species. An obvious way to make this point is to observe that bullocks or worker bees are not disbarred from species membership merely by virtue of being reproductively isolated from everything. This consideration is not, of itself, very convincing. Elaboration of the proposed criterion in terms of ancestral or other reproductive links to members of the interbreeding, but isolated, group might accommodate such cases. But deeper obstacles stand in the way of such a course.

Adequate reproductive isolation of a species does not require complete isolation of all its members. Hybridization occurs throughout the natural world, though more particularly among plants, fishes, and amphibians. (A recently publicized case of successful mating between two monkeys of different species has brought this fact to more general attention.) This need not lead to significant gene introgression. In some cases hybrid individuals are sterile. In more complex cases there may be a band of hybridization where the geographic ranges of two species meet. (A readily noticeable example occurs with primroses and cowslips.) In such cases the continued existence of the two species is made possible by the competitive superiority of each within its preferred range. Since this superiority will normally apply also over hybrids, the alien genes will not penetrate much beyond the area of overlap. Thus the suggestion that this criterion for species provides a privileged relation between its constituent individuals fails on two counts. First, there will be individuals that would not be assigned to any species on this criterion; and second, there will be reproductive links connecting individuals that certainly belong to different species. The latter point is reinforced by the fact that the ability to produce viable offspring is not transitive. There exist chains of species, any two adjacent members of which

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<sup>28</sup> For a discussion of speciation, see Mayr, *op. cit.*, chs. 15 and 16.

can produce viable offspring, but the terminal members of which are not able to interbreed. Finally, as has often been observed, this criterion is completely useless for asexual species, since it would imply that every asexual organism constituted an entire species.

The third, and final, proposal I will consider is one based directly on evolutionary history. The underlying idea is that it should be possible, in theory, to construct a family tree for all life on Earth. It is then hoped that the classificatory taxonomic tree could converge on this phylogenetic tree. Hence any taxon will correspond to an historically real evolutionary process. This proposal has the considerable advantage that it appears to be equally applicable to the species and to higher taxa. Since it is certainly hoped that taxonomy and phylogeny should at least be mutually illuminating, this suggestion is in some sympathy with biological theory.

Two preliminary points should be made about this proposal. First, the essential or privileged property in question is highly extrinsic to the individuals to which it may be supposed to apply. Not only does it offer no hope of examining individuals and determining to which taxa they belong, but indeed nothing short of the entire evolutionary history of the organism will suffice for such a determination. The second point is related, though more speculative. The vast improbability that such a phylogenetic tree could ever be constructed does not seem wholly irrelevant. Much of the necessary theorizing depends upon traces left by organisms in the very distant past. The circumstances under which such traces remain are quite unusual, and the vast majority of organisms that ever existed probably left no trace whatever. (I suppose a determinist might be driven to deny this. But that, I think, is a problem for determinism.) Thus this is a case in which the underdetermination of a theory by all available evidence seems particularly probable. It is very likely that insisting on a phylogenetic criterion of taxon membership would make taxonomy literally impossible.

To assess the present hypothesis it is first necessary to explain how a taxonomic tree could also be interpreted as an evolutionary tree. This requires that something be said about speciation. *Qua* taxonomy, each taxon also includes all the lower taxa

“descended” from it. Thus the American Robin belongs simultaneously to the species *migratorius*, the genus *Turdus*, the family Turdidae, etc. The present suggestion interprets this as also embodying an evolutionary hypothesis. A species is composed of a number of populations that may be more or less differentiated from one another, both genetically and morphologically. When a population acquires some characteristic that isolates it genetically from the rest of the species, it is said to have achieved the status of a species. Thus the relevant evolutionary hypothesis would assert that at one time “turridae” would have referred merely to a population of a larger species. Subsequently, this population would have achieved full species status, and still later divided into further species which now constitute the various genera in the family Turdidae. The particular genus *Turdus*, in turn, must have divided into further species, of which one is *migratorius*.

It remains to be seen whether this phylogenetic interpretation of the taxonomic tree can do anything to supply the taxon with a real essence, or privileged internal relation. Against the suggestion that evolutionary history could be essential to members of a taxon, one might deploy a Putnam-Kripke type argument. If, say, a chicken began to lay perfectly ordinary walnuts which were planted and grew into walnut trees, I would not wish to refer to this result as the production of a grove of chickens. If accepted, this intuition shows that the right ancestry is not a sufficient condition for taxon membership. My intuition, moreover, is that the trees in question might prove to be genuine walnut trees, which is to deny that ancestry is even a necessary condition. However, having expressed suspicion of this style of argument, I do not want to rest any weight on this example of it. A more general argument is the following. Any sorting procedure that is based on ancestry presupposes that at some time in the past the ancestral organisms could have been subjected to some kind of sorting. One can imagine drawing up a phylogenetic tree and naming some branch of it; but the objective reality of the branch can be no greater than the objective reality of the grouping of organisms that constitutes the beginning of the branch. But I have claimed that, given all the organisms existing at a single time, there are no privileged properties or relations by means of which these can be sorted unambiguously

and exhaustively into objectively significant classes. In short, the phylogenetic criterion must be parasitic on some other, synchronic, principle of taxonomy. It cannot generate privileged properties on its own.

As I have tried to stress, I do not mean to claim that species are unreal; only that they lack essential properties, and that their members cannot be distinguished by some privileged sameness relation. In fact, the existence of discrete species is one of the most striking and least disputable of biological data. If one examines the trees or birds in a particular area, it is apparent that these fall into a number of classes that differ from one another in numerous respects. But the essentialist conclusion that one might be tempted to draw from this fact is dissipated first by more careful study, which reveals that these distinguishing characteristics are by no means constant within the classes, and second by extending the scope of the investigation in both space and time, whereupon the limitations both of intraspecific similarity and interspecific difference will become increasingly apparent.

I think that a closer look at the nature of evolutionary history may help to clarify the position I am trying to present. Evolutionary history may indeed be perspicuously displayed in the form of a tree. Forks in the tree may be taken to represent the establishment of mechanisms for reproductive isolation between populations of a species, and unbroken lines to represent species that exist at a given time. If we interpret this tree as a graph whose abscissa is a measure of time, and whose ordinate represents some very complicated property measure,<sup>29</sup> then the lines can be taken as representing average members of the species. If we were to try and plot individual organisms on the same graph, presumably these would be distributed around the lines in a normal statistical way. Here we may note various complications of which the model should take account. First, the distance between the lines will be highly variable. In the case of higher mammals, for instance, the lines are generally well spaced. Thus there are few borderline cases for the application of such terms as "man" or

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<sup>29</sup> Strictly, this must be envisaged as multidimensional. The present simplification is merely for expository ease. The present account offers at least a partial justification for the program of numerical taxonomy, for which see R. Sokol and P. Sneath, *Principles of Numerical Taxonomy* (San Francisco, 1963).

“tiger.” If we take a number of sibling species of fruit flies, on the other hand, the lines may be very close together. This enables us to see how the number of species can be a determinate matter, whereas the assignment of individuals to species may be only partially determinate. For an analysis of the distribution of various properties of these fruit flies could reveal a series of sharply defined means, whereas some individuals might lie between the means for most properties. On the graphic model, some individuals will occupy positions intermediate between two (or in a multidimensional model, many) lines.

A further complication is that the process of speciation is not an instantaneous one. Also there are rare cases of two species merging. This, too, will be a gradual process. What this implies is that even when we draw a line through the taxonomic tree at a precise moment in time, the number of species in existence will not be wholly determinate. For where there are species in the process of dividing or merging, it will not be a determinable question whether there are two species or only one in existence at that time. This complication reveals a curious analogy with the preceding one. For again, there is a slightly different perspective from which essentially the same question does admit of a determinate answer. Retrospectively, at least, it should be possible to say how many species existed during a certain period. For we can see whether species did in fact succeed in separating or merging during that period.

It is satisfying that this picture indicates a role for each of the proposed defining characteristics I have been discussing, in accounting for the existence of, and describing the nature of, the species. I believe that it also vindicates the reality of the species in a way that shows why none of these features can be sufficient to define the members of a species. And finally, as a consequence of this last point, it appears that even if terms of ordinary language did refer to species, Putnam’s theory of natural kind terms could still not be applied to them.

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