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DISCUSSION:

SETS, SPECIES, AND EVOLUTION: COMMENTS ON PHILIP KITCHER'S "SPECIES"*

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Philosophers have a hard enough time reconstructing the theoretical role of dead concepts like phlogiston and caloric and of living concepts like, perhaps, distance and entropy that, at least temporarily, have found their way into theories that are reasonably canonical. But the species concept poses even greater difficulties, since it is currently undergoing a transformation in its foundations.

In retrospect, we realize how risky it was for philosophers to take up the epistemology and metaphysics of geometry, just when the idea of a nonEuclidean geometry was beginning to emerge. The occupational hazard then was claiming that certain propositions are a priori true, when later scientific developments showed them to be empirically false. In this case, the problem was that philosophers had too narrow a picture of what is possible. My hunch is that philosophers now face something like the opposite problem for the species concept. The danger is that we are apt to take seriously a possible interpretation of the species concept that no longer plays a role in evolutionary theorizing.

It isn't that this interpretation is a priori incapable of theoretical development. It's just that it currently constitutes a mere hope and lacks any serious degree of theoretical articulation. Yet, at the same time, there is another species concept that is a going concern, presupposed by an active and inventive research program. This latter avenue of thinking is preempting the word "species" for itself. Even if the other idea were to make any headway, I doubt that the units identified from that perspective would be called "species".

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†This paper is a descendent of one I read at the 1982 Eastern APA symposium on species, commenting on an ancestor of Philip Kitcher's present paper. I thank Philip Kitcher for stimulating discussion, which helped create the selection pressure that guided the evolution of the ancestral paper into the present descendent. I also am grateful to the Graduate School of the University of Wisconsin, Madison for financial support in the form of a Romnes Faculty Fellowship.

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Let me be more specific. Michael Ghiselin (1974) and David Hull (1976, 1978) have argued that species are chunks of the genealogical nexus—they are individuals whose parts are organisms. This opposes an older tradition according to which species are natural kinds—like gold—which have associated with them natures that are spatiotemporally unrestricted. Philip Kitcher takes seriously both these ideas; he sees a role for the genealogical/historical conception and also for the one that is (in Carnap's sense) "purely qualitative". I am not so nonpartisan. The idea that species are populations is currently a presupposition of what is most promising in systematics. The idea that species are natural kinds, in my opinion, is not. Not that there couldn't be a theoretical development of the natural kinds idea. But it would take a very long time to get going. And I wouldn't even want to bet that any research program will really give it a shot. And even if this idea were given a run for its money, the individuals idea has a head start. One prize that is at stake here is the use of the word "species". I'm guessing that the species-are-individuals perspective will win.

It isn't that evolutionary biology has no interest in identifying natural kinds. We already have a number of nice candidates. Predator may be one; sexuality may be another. But these are not *species* concepts. It is no surprise that these natural kinds cross-classify each other; presumably, there is an overlap relationship between the predator populations and the populations that reproduce sexually.

This raises an important question for Kitcher's pluralism. I see the genealogical conception at work in biological discussion of species. I also see biologists trying to find natural kinds in terms of which they may state laws. But why think that the search for natural kinds will result in characterizations of *species* as natural kinds?

Kitcher's discussion of Williamson (1980) is a case in point. Certainly, Williamson's data can be discussed strictly from the point of view of describing how morphological change occurs in lineages, without bringing in the issue of reproductive isolation. But why this approach should be fitted out with its own *species* concept escapes me. Suppose a population had a great deal of unexpressed genetic variability, and that a sequence of selection events was thereby able to cause a series of morphological changes in the population. Magnitude of morphological change does not, by itself, settle the question of whether *speciation* has occurred, although it is an interesting biological phenomenon in its own right.

My doubts about the idea that species are natural kinds may suggest an unseemly scepticism on the part of a philosopher of science. After all, there are three main schools of systematics, and only two of them—cladism and evolutionary taxonomy—can be viewed as straightforwardly committed to the idea that species are chunks of the branching evolu-

tionary tree.[1] The third—pheneticism—groups organisms by so-called "overall similarity", and if this approach made sense, it would be an alternative to the genealogical conception. I am doubtful about nongenealogical species concepts in part because I am dubious about pheneticism.

Even if pheneticism were correct, I don't see that it is in the business of finding *natural kinds*. If I classified the living world by first listing equally weighted attributes assembled without consulting evolutionary theory, it would be a miracle if my division coincided with distinctions at the level of natural kinds. As philosophers have long realized, similarity without theory is empty. And abandoning this misplaced idea of "objectivity" and choosing traits for their so-called "biological importance" would offer little more hope for the natural kinds idea. A pheneticism attuned to the needs of contemporary evolutionary theory would, I think, end up choosing characteristics so that the resulting classifications reflect phylogenetic relationships.

Ghiselin (1974) and Hull (1976, 1978) have clearly discerned where the species concept now stands. Before the consolidation of Darwinism in the Modern Synthesis, the idea that species are natural kinds was a real possibility. If there were Laws of Forms—principles that delineate the set of possible structures that living things may assume—then species might be natural kinds. The species that have evolved and will evolve would occupy slots in biology's "periodic table"; a theory would tell us that there are only so many ways to build an organism, and each of these ways is a (possible) species.

Whatever the attractions are of the idea of Laws of Form, we must bear in mind that the Modern Synthesis discarded this idea as so much idealist morphology (Allen 1975). The guiding idea of the Modern Synthesis was that of an incredibly heterogeneous but integrated breeding population, shaped by the fortuitous whims of natural selection. Populations are putty in the hands of a tinkering Mother Nature. Systematics has assimilated this perspective into its conception of species. There are no theoretically preordained slots for populations to drop into; natural selection is "opportunistic" and the assemblages of traits we observe are Rube Goldberg devices (Gould and Lewontin 1979; Kauffman 1983; Sober 1983b, 1983c). That species are *populations* (i.e., physical objects—individuals—of a certain kind) was the idea that filled the vacuum left by the demise of the view that species are natural kinds.

We now are seeing challenges to the Modern Synthesis, and some biologists are toying with the idea of Laws of Form (Gould and Lewontin 1979; Kauffman 1983). But, at this historical juncture, it is unlikely that

[1]See Hull (1970) for discussion of the general features of the three main approaches.

the types of organization hypothesized in the search for such laws will be accorded the status of species. If the history of science had proceeded differently, it is conceivable that this might not be true. The kinds of structure that may be discovered in the search for Laws of Form might have been termed "species", if the term had not been preempted by another research program.[2] So whether something counts as a species concept is itself a historical question. Not only are species individuals, but concepts themselves are individuated by their place in history.

I now want to take up Kitcher's criticisms of David Hull's arguments that species are individuals. Kitcher is perfectly happy to grant that one sound conception of species individuates them genealogically. But he rejects the idea that this species concept implies that species are individuals. Kitcher wants all possible species concepts to view species as sets, and to differ with respect to the criterion of membership invoked.

I have no objection to comparing species concepts by seeing what properties they demand of two items if they are to be conspecific. But a species is not literally identical with a set of organisms. And this is true, regardless of whether species are individuals or natural kinds. Consider the set of organisms in *Homo sapiens*. I am one. If I did not exist, that set would not exist. Yet the species would. A population is not to be identified with the set of organisms in it. And the same holds true of natural kinds; gold would still exist and be numerically the same natural kind, even if my wedding ring and the matter of which it is made had never existed.[3]

Kitcher (footnote #5) avails himself of a simple reply to this modal argument. A species, he claims, is only *contingently* identical with a set of organisms; one and the same species may be identical with different sets of organisms in different possible worlds.[4] Though sorting out the issues involved in this idea involves stepping into some muddy metaphysical waters, I'll briefly take the plunge.

First, if the genealogical view of species is right, then species are sets of organisms no more than an organism is a set of cells. Organisms are not sets. They are *made of* cells; they don't have cells *as members*. No matter that claims about an organism can be "translated" into claims about

[2]Notice that the problem described here that Darwinism poses for the view that species are natural kinds is independent of the question of whether evolution is gradual or is "punctuated". Uneven evolutionary rates, of the kind hypothesized, for example, by Eldredge and Gould (1972) would not, I think, make the species concept safe for essentialism. I defend this position in Sober (1980) and Sober (1984).

[3]Here I borrow an argument that Hambourger (1977) has made against the Russell/Frege identification of each number n with the set of all n membered sets.

[4]Kitcher remarks that "just as the extension of 'car' varies from [possible] world to world, so does the referent of '*Homo sapiens*'." I, of course, do not contest this *semantical* fact.

sets; Quine (1969) made much of the fact that claims about physical objects can be "translated" into claims about numbers. Such "translations" change the subject, or so it seems to me. Since I don't think of organisms as sets, I find it hard to think of species—at least on the genealogical conception—as sets. But maybe the answer is that I ought to think of *everything* as a set.

Second, contingent identity seems to have some troublesome properties in this case. Suppose the species *Homo sapiens* is identical with set *A* in the actual world and with set *B* in another possible world. This is how we represent in the jargon of possible worlds the idea that the species is identical with *A*, but might have been identical with *B*. Does it follow that set *A* is not identical with set *B*, but might have been? If so, the view that species are sets is in trouble. For the essential identity of a set is given by its containing just the members it does. I don't see how set *A could* be identical with set *B*, if they are as a matter of fact distinct.

My third question is borrowed from an objection that Benacerraf (1965) raised against the logicist identification of numbers with sets. If species are sets, why should we identify them with sets of *organisms*, rather than with sets of *local populations*, *families*, *generations*, or *cells*? I suspect that clever "translation" will allow us to formulate whatever we naively wanted to say in any of these idioms. If a species is a set, it must be some particular set. Is there any hope of choosing between these candidates? I think not. And the reason is not that there is a fact here that shall forever remain mysterious. I suspect the reason is that species aren't sets at all.

So I am unhappy with the idea that a species is identical with a set of organisms. What of Kitcher's criticisms of Hull's arguments for the species-are-individuals view? His remarks about the fallacy of incomplete translation are entirely correct, although I doubt that Hull places much weight on this rather "linguistic" line of argument.

Kitcher next describes Hull as defending the idea that species are individuals on the ground that it explains why "all swans are white" isn't a law of nature. But my picture of Hull's position is that he is simply noting a consequence of the individuals view rather than providing any independent argument for it. And he is right: if a statement is law-like only if it contains no term that names an individual, then "all swans are white" isn't law-like, if "swan" is the name of an individual.

Kitcher's own explanation of why "all swans are white" isn't law-like is worth considering. He suggests that "all *S* are *P*" will be law-like in three cases—first, if a zygote born into species *S* will be inviable, if it lacks property *P*; second, if lacking *P* makes an organism incapable of producing viable gametes; and third, if the introduction of property *P* into species *S* causes a speciation event. I think that none of these proposals will do.

Inviable zygotes are, as far as I know, members of the species of their parents. At least in the theory of natural selection, one considers the life-history of a population as extending from the egg stage to the adult stage, with selection potentially acting anywhere along the way. Zygote inviability is a kind of mortality selection; it just happens early. Zygotes are in the population. And if they're in the population, they're in the species. This, anyhow, is how the head-counting works in the parts of evolutionary theory with which I'm most familiar.[5]

In a footnote (#8), Kitcher modifies this claim about inviable zygotes "for those who are inclined to believe that the inviability of a zygote because of some genetic disruption does not signal a species boundary". The revision is the proposal that "mutations or chromosomal novelties giving rise to the absence of *P* generate inviable gametes". This would seem to mean that genetically caused sterility (due to inviable gametes) excludes an organism from the species of its parents, a conclusion I find wholly unmotivated. In the social insects, for example, workers are sterile for genetic reasons, but are conspecific with related, fertile, organisms nonetheless.[6]

As to the third possibility, it strikes me that there is no characteristic of an offspring that in itself is necessary or sufficient for its being in the same species as its parents. The reason is that species individuation is retrospective. Consider Ernst Mayr's (1963) much discussed "founder effect". Suppose a flood separates a small number of isolates from the main part of the population. Selection leads this group to diverge from the parent population, and thereby to count as a distinct species. When did this new species come into existence? One natural answer is that it began at the time of the isolation event, even though the isolated organisms may have been no different from the organisms in the main population. The founders were founders of a new species precisely because of what happened later, and not in virtue of anything special about them. In the same way, an offspring may be as different as you wish from its parents. Whether it falls into a new species depends on what happens later.[7]

Of course, if there *were* some phenotypic or genotypic characteristic

[5]This has some relevance to one argument about the morality of abortion. Whatever may be said against counting foetuses as *persons*, I see no biological motivation for denying that they are *organisms* in the same species as their parents. They are members of *Homo sapiens*, and are human beings in that sense.

[6]It is true, of course, that the sterile castes don't have inviable *gametes*, but the point is simply that sterility *per se* does not exclude an organism from species membership.

[7]On the old TV program "The Show of Shows", there was a skit in which Syd Ceasar opens a newspaper whose headline reads "WORLD WAR I BREAKS OUT!" This headline was funny, not because it was false, but because the conditions that made it true came into existence only twenty-five years later; whether a conflict counts as the *first* (but not the last) World War is settled retrospectively, not simultaneously. Whether an organism is in the same species as its parents is settled the same way.

that automatically excluded an organism from a particular species, then lacking this characteristic would be necessary for species membership. The claim that species are individuals does not rule out this possibility. Individuals may conceivably have essences too. Kripke (1980), for example, thinks that human beings necessarily come from the sperms and eggs that produce them.

Kitcher's last argument against Hull's position deals with the example of multiple origination. He describes a unisexual lizard species that arose by hybridization and considers two possible scenarios. The first is that multiple hybridization events introduced parthenogenic individuals into the new species, and that these "founders" each started up parent/offspring lineages that persisted. The second is that several hybridization events took place followed in all cases but one by extinction of the unisexual line. In both these cases, Kitcher concludes that exactly one species exists; but, he claims, Hull's theory is committed to there being as many species as there were origination events.

The problem is that this interpretation is not forced on the view that species are individuals. An individual may have parts that had their separate origins; a fleet of ships may have its component boats constructed in different ship yards. Indeed, there is nothing in the founder principle that requires that the founder population be a single parental pair. And as for the second problem, the view that species are individuals need not maintain that every small budding on the tree of life counts as a distinct species. The retrospective point of view mentioned before in fact suggests that a novel organism does not a species make. So Kitcher's third argument leaves me unpersuaded that the idea that species are individuals is mistaken.

Kitcher mentions three possible "unity relations" that the genealogical notion of species might exploit. Although I am dubious about a pluralism that countenances both species as natural kinds and species as individuals, I am a bit more circumspect about how things stand for this problem that is internal to the historical conception. When we turn to the problem of individuating enduring physical objects, the difficulties that come into focus when we consider Hobbes' example of the ship of Theseus force us to at least consider the possibility that there is no unique criterion of individuation in this case.[8] What may hold for ordinary physical objects may hold as well for species. It would be difficult to argue directly that this is so; if repeated attempts to provide a definitive answer consistently fell on their faces, we might take this to be some evidence that the problem is insoluble. Yet, the possibility always remains that the fault is not

[8]This is the position that Wiggins (1980, p. 97) seems to adopt in his remark that the situation is "irreclaimably paradoxical".

in the nature of our concepts, but in our lack of insight into their determinate structure. In the present case of the species concept, however, I would suggest that evolutionary developments are still unsettled enough and our philosophical attention to them has so far been so scant, that a few more centuries of frustration (at least) would be needed before we can declare the project hopeless. For the foreseeable future, pluralism is the "null hypothesis" that we should attempt to refute.

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