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# AGAINST THE MONISM OF THE MOMENT: A REPLY TO ELLIOTT SOBER\*

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In his "Discussion" (1984), Elliott Sober offers some criticisms of the view about species—*pluralistic realism*—advocated in my 1984. Sober's comments divide into three parts. He attempts to show that species are not sets; he responds to my critique of David Hull's thesis that species are individuals; and he offers some arguments for the claim that species are "chunks of the genealogical nexus." I consider each of these objections in turn, arguing that each of them fails. I attempt to use Sober's insightful critique to explain and defend pluralistic realism more fully.

There are rumblings of a new orthodoxy in philosophical systematics. The proposal is that all division of organisms into kinds must reflect evolutionary history, even when evolutionary history cuts across other biological characteristics. Once this proposal is accepted, some of its proponents suggest that, if we are really to take it seriously, then species should be viewed as individuals. In my 1984, I dissent from the emerging orthodoxy, arguing that several traditional concepts of biological species have something going for them and that there are no compelling reasons for counting species as individuals. My heresy prompts Elliott Sober (1984) to some comments which raise interesting issues. My present task is to explain why I remain unrepentant.

Sober's comments divide naturally into three parts. He offers some reasons for thinking that the old-fashioned idea that species are sets which contain organisms as members is a non-starter. He replies to some of my objections to arguments that have been offered in defense of the thesis that species are individuals. And he gives some general reasons for believing that no account of species can succeed, unless it follows Michael Ghiselin (1974) and David Hull (1976, 1978) in taking species to be "chunks of the genealogical nexus." I shall take up these three topics in turn.

**1. The Requirements of Modality.** Sober's argument against the claim that species are sets begins with a simple appeal to modal intuitions. Sup-

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pose species were sets. Assume specifically that *Homo sapiens* is a set. Elliott Sober is a member of this set. But there are worlds in which Elliott Sober does not exist but in which *Homo sapiens* does exist. In these worlds, the set, by hypothesis *Homo sapiens*, does not exist, since one of its members does not exist. Contradiction. Hence we have a *reductio* of the thesis that species are sets.

One might quibble with the details of this argument in a number of ways, but in my 1984 I offered an intuitively obvious reply. In our world, *Homo sapiens* is one set. In those unlucky Soberless worlds, *Homo sapiens* is another set. The species exists but is identical with a different set. No contradiction threatens here.

Sober claims that this reply fails. He offers three objections. He thinks (I think) that each of these is fatal to my view. I think that none of them causes any damage.

The first objection seems quite unrelated to Sober's original modal argument. Translations of discourse about species into set-theoretic language are alleged to change the subject. I reply that a simple and systematic reconstruction of a body of discourse is a standard way of making clear our ontological commitments: simple assertions that reconstruction of talk about real numbers in terms of Dedekind sections "changes the subject" do not cut much ice with those concerned with the foundations of analysis, and rightly so. But Sober's worry is not simply based on naive assertion; he is concerned that the strategy of using set-theoretic discourse to reformulate parts of physical and biological theory will be too broadly applicable; Sober raises the specter of fanatical neo-Pythagoreans construing *everything* as a set. But we need not be haunted by this specter. There is nothing in my account to suggest a *general* way of replacing talk of objects with talk of sets, and I hold no brief for the view that organisms are just sets of cells. (Even if one were clear on the cells that are supposed to belong, it is evident that changed relationships among these cells could destroy the organism.) My proposal was *local*; I sketched a simple way to understand our talk about evolving species by construing species as sets. Blanket Pythagoreanism is not my project, and I shall be quite happy to join with others in resisting the wholesale application of the Löwenheim-Skolem theorem—or any other general device that may be proposed by those who wish to dispense with organisms and other physical objects in favor of sets.

Let me bypass Sober's second objection, for the moment, and take up the third point. There is a familiar point, pressed forcefully in Paul Benacerraf (1965), which objects to set-theoretic accounts of number on the grounds that there are too many alternative arithmetically equivalent ways of giving set-theoretic explications of the natural numbers. Sober tries to adapt this idea. "If species are sets, why should we identify them with

sets of *organisms*, rather than with sets of *local populations*, *families*, *generations*, or *cells*?" Answer: in the arithmetical case (and in the case of the ordered pairs—see my 1978), the proposals for reduction are genuinely equivalent; there is no reason to prefer any of the explications on the grounds that it alone yields correct truth conditions in some cases or on grounds of simplicity. The case at hand is not analogous. Someone who followed Sober's alternative explicative strategies would have to struggle to accommodate the fact that our talk of species presupposes that the same relation holds between organism and species, between organism and family, and between organism and population. Populations, families, generations are sets; they are subsets of species. Thus when biologists talk of an organism belonging to a population which is included in a species, the first relationship is the familiar set-theoretic membership relation and the second is set-inclusion. A similar ontological view is reflected in ascriptions of cardinality—as when biologists talk about family size, population size, and species size. If Sober sees a way to reconstruct this discourse along one of the alternative lines he proposes while retaining the simplicity of the scheme that I would prefer, then he has spotted something that I have missed. (I can't help noting that the idea of multiple admissible reductions of discourse about species in set-theoretic terms isn't clearly favorable to the idea of species as individuals. A parallel claim about numbers was offered by Mark Steiner in his 1975, but it seems vulnerable to straightforward objections (see my 1978). Far more natural is the approach of trying to reformulate the discourse in a way that will bring out the common properties of the rival explications, an approach pursued in the arithmetical case by Nicholas White (1974) and Hartry Field (1974).)

I have postponed discussing Sober's second objection because it seems to me to be the most interesting. The worry is that, if species are sets, and if *Homo sapiens* is set *A* in one world and set *B* in another, then we shall be forced to say that set *A* might have been identical with set *B*. But "the essential identity of a set is given by its containing the members it does." So, once again, the view that species are sets will encounter a *reductio*.

This argument does develop Sober's original modal concerns. I take it to be parasitic on a point made by Robert Hambourger about the Frege-Russell account of number (Hambourger 1977). I shall first explain what I think is wrong with Hambourger's argument, and then apply my analysis to the case at hand.

Frege and Russell proposed that the number *n* is the set of *n*-membered sets (and, of course, they showed how this explication is noncircular). Different possible worlds will contain different objects, so that the set of *n*-membered sets will typically be different in different worlds. If nu-

merals are rigid designators, the Frege-Russell analysis will therefore fail.

But why should we believe that numerals are rigid designators? It won't do simply to announce that numerals are proper names and proper names are rigid designators. For it is legitimate to reply that if the thesis that proper names are rigid is taken to be a matter of definition, then we can regard the numerals as *apparent* proper names: they are convenient shorthand for nonrigid descriptions. If, however, the thesis that proper names are rigid is not taken as a matter of definition, then a defender of the Frege-Russell approach may just propose that the numerals are a counterexample to it, a species of proper names which do not designate rigidly.

To convict Frege and Russell one must find some feature of our use of proper names which requires that they be taken as rigid designators. Hambourger believes he has isolated an appropriate feature. He claims that, in advancing such statements as

- (1) The number of John's children in  $w_1$  is identical with the number of John's children in  $w_2$ .<sup>1</sup>

we are maintaining that a particular number is the same object in two distinct possible worlds. The claim depends crucially on reading (1) as a genuine identity statement. Defenders of the Frege-Russell approach are not committed to that reading. They may propose that, like transtemporal "identity" statements (for example, statements about the "identity" of an organism through time), statements like (1) need reparsing. A simple paraphrase of (1) is available. Define the relation of *correspondence* between sets in different worlds so that it relates the set of  $n$ -membered sets in one world to the set of  $n$ -membered sets in each other world. Now interpret (1) as

- (2) The number of John's children in  $w_1$  is the (unique) correspondent of the number of John's children in  $w_2$ .

(2) like (1) implies the fundamental claim that the set of John's children in  $w_1$  can be put into one-one correspondence with the set of John's children in  $w_2$ . Moreover, (2) can be used in place of (1) in reasoning to any conclusions we might have hoped to obtain from (1). What part of the content of (1) has been lost?

<sup>1</sup>Here, and in the following sentences, the phrase 'in  $w_1$ ' should be taken as modifying both "the number" and "John's children." Thus, a more perspicuous rendering of (1), would be

(1\*) The number-in- $w_1$  of John's children-in- $w_1$  is identical with the number-in- $w_2$  of John's children-in- $w_2$ .

David Lewis has pointed out to me that a different way of distributing the modifier could yield a different answer to Hambourger.

The same strategy can be applied in the case of species. Sober's worry can be formulated in a precise way as follows: Let  $w_1$  be a world in which both Sober and I exist; let  $w_2$  be a world in which I exist but Sober does not. Consider now the statement

- (3) The species to which Kitcher belongs in  $w_1$  is identical with the species to which Kitcher belongs in  $w_2$ .<sup>2</sup>

(3) appears to be true because the terms flanking the identity both refer to *Homo sapiens*. But the sets identified with *Homo sapiens* in  $w_1$  and  $w_2$  will be different. So we appear to have trouble.

The way out is exactly parallel to the Frege-Russell escape route. However, there are complications stemming from the admissibility of a number of different species concepts. Pick any concept of species, for example, the conception that individuates species by their place in evolutionary history. (Analogous considerations will go through for other conceptions.) Define the relation of correspondence so that it relates sets in different worlds just in case they occupy the same place in evolutionary history. Then the content that was supposed to be captured in (3) is presented more explicitly as

- (4) The species to which Kitcher belongs in  $w_1$  is the correspondent of the species to which Kitcher belongs in  $w_2$ .

As in the number-theoretic case, content is preserved and contradiction is avoided.

Generalization: the set which is *Homo sapiens* in  $w_1$  is distinct from the set which is *Homo sapiens* in  $w_2$ , and, indeed, these sets are necessarily distinct. What we are saying when we claim that *Homo sapiens* might have had different members (or been a different set) is just that the set which actually is *Homo sapiens* bears an important relation (the relation of correspondence) to different sets in other worlds. The requirements of modality do not force us to abandon the view that species are sets. They merely oblige us to take care to formulate our modal claims about species in a perspicuous way.

**2. Individualism in Retreat.** I turn now to Sober's critique of my critique of Hull's arguments for the thesis that species are individuals. I want to begin by distinguishing two versions of the species-as-individuals doctrine. The first version is a bare ontological proposal, rooted in a predilection for mereology rather than standard set theory. I don't doubt that the same biology and philosophy of biology can be done using either of

<sup>2</sup>As with respect to (1), (3) can be rendered more perspicuously as

(3\*) The species-in- $w_1$  to which Kitcher belongs-in- $w_2$ .

these ontological alternatives. My critique of Hull's thesis was directed at a more exciting doctrine which connected the mereological approach to claims about the existence of laws and the impossibility of historically disconnected species. I argued that the claims were wrong and the connections bogus.

First, there is an issue concerning laws about particular species. Hull and I agree that statements like "All Swans are White" are not laws of nature. We seem to disagree with respect to two points:

- (5) "All Swans are White" fails to be a law of nature because 'Swan' (or, more exactly, names like '*Cygnus olor*') names an individual.
- (6) There are no laws of the form "All *S* are *P*" where *S* is a species (except, perhaps, derivative laws that attribute a property found in all organisms).

Hull appears to advocate (5) and (6). I reject (5) and I have argued that (6) may also be false.

Since Hull and I agree that "All Swans are White" isn't a law, the difference between us turns on *why* it fails to be a law. Hull seems to think that a proper analysis of "All Swans are White" will give it the form

- (7) All parts of Swan (more exactly, perhaps, all those parts of *Cygnus olor* which are whole organisms) are White.

This analysis is supposed to help us to see why "All Swans are White" isn't a law. Laws don't make reference to particular localized objects, and, from (7), we see that "All Swans are White" does make reference to a particular localized object, to wit, Swan (more exactly, perhaps, *Cygnus olor*).

There are all kinds of murky issues surrounding the universality of laws (see John Earman's 1978 for lucid discussion), and one might legitimately wonder whether hailing (7) as an analysis of "All Swans are White" yields the consequence that this statement is not a law. But what seems to me evident is that the envisaged explanation isn't the *right* explanation. "All Swans are White" isn't a law first because it isn't true; and second, even if it were true, it would be an accidental generalization, a cosmic analog of a generalization about the onetime contents of Nelson Goodman's pocket. Most statements of the form "All *S* are *P*" that spring readily to mind aren't laws because they attribute to the members of a species a property that could easily have been absent in some members of the species if slightly different eggs and sperm had combined to form zygotes, or if easily possible events of mutation had occurred. Our difficulty in finding distinctive laws about particular species has nothing to do with the idea

that species are individuals; it is the result of the fact that most of the properties that distinguish all and only the organisms belonging to a species could all too easily have been missing in some members of the species.

So I don't think that the doctrine that species are individuals can take any credit from its ability to generate the conclusion that statements like "All Swans are White" are not laws. (I assume, for the sake of argument, that this is a consequence of the doctrine.) To be sure, the doctrine has other true consequences—the laws of logic, for example—but we don't think of its having these consequences as lending it support. Neither should we regard it as receiving evidence from a consequence which is properly explained in a quite different way.

Furthermore, if we grant that the thesis of individualism entails that there are no laws about particular species, then I take it to foreclose a possibility that ought to be left open. In my 1984 I argued that there are two ways in which a statement of form "All  $S$  are  $P$ " might qualify as a law. Suppose first that  $P$  is a property such that, if members of  $S$  began to produce an offspring lacking  $P$  then the process would inevitably go awry. This is imprecise, so, in my 1984, I formulated it as the claim that zygotes lacking  $P$  would be nonviable. In his original comments, Sober objected, on the grounds that, according to evolutionary theory, even nonviable zygotes belong to the population (and thus to the species); the complaint is repeated in his 1984. There's a nice point here: in the sense in which 'population' is used when evolutionary biologists are doing head-counting across the generations there seems to be no implication that if  $x$  and  $y$  belong to the same population then they are conspecific. So I think Sober's complaint overinterprets evolutionary theory. But let that pass. In my 1984 I tried to formulate the original intuition in an uncontroversial way. I imagined that attempts to produce organisms lacking  $P$  from members of  $S$  break down at the stage of gamete formation. Sober misunderstands me here—although I am willing to allow that my formulation was not as clear as it should have been. I did not intend that "genetically caused sterility (due to inviable gametes) excludes an organism from the species of its parents" (Sober 1984, p. 339), so that the point about worker sterility in social insects is not relevant to my claim. What I had in mind can best be explained by imagining gamete formation in members of  $S$ : whenever a sperm cell or ovum is being produced, if the process starts to go in the direction of giving rise to a cell that would yield a zygote lacking  $P$ , the process malfunctions and no gamete is formed. The malfunction can occur as early as you like. My basic claim is that there *may* be properties which are so deeply constitutive of the species that attempts to eliminate them from descendants inevitably fail. Laws about particular species could then be understood as prohibitions, telling



us that you can't get there from here (or, at least, not in one evolutionary step).

The second possibility for laws of form "All  $S$  are  $P$ " corresponds to situations in which absence of  $P$  would generate a new species. I shall take up Sober's remarks about this in the next section, for I take them to bear on a different issue, namely, the question of exactly when speciation occurs. For the moment, let me simply motivate my claim by appeal to example. There are well-known cases in which speciation occurs in plants through the mechanism of polyploidy (and there are less well-known examples involving vertebrates, frogs in particular). In such cases, I suspect that there will be some property involving chromosome number and structure which can be attributed to the species concerned so as to yield laws. Here is a first shot. Suppose  $S_2$  with twenty-four chromosomes evolves from  $S_1$  with twelve chromosomes. Assume further that we can pick out a property of whole chromosomes which will enable us to distinguish members of  $S_2$  with their double complement of whole chromosomes from aberrant members of  $S_1$  which happen to have twelve whole chromosomes and twelve miscellaneous chromosome fragments. Then I take it that it might be a law that all members of  $S_1$  have less than twenty-four whole chromosomes, and the lawlike status of this generalization would be founded on the evolutionary fact that if members of  $S_1$  give rise to an offspring with twenty-four chromosomes then that organism belongs to  $S_2$ . There are lurking complications which have led me to be cautious in my claim—perhaps there is no distinctive property of chromosome architecture to which we could appeal in differentiating members of  $S_2$  from the aberrant members of  $S_1$ , for example—but I know of no *a priori* argument advanced by any biologist or champion of individualism which would foreclose the possibility that I have envisaged.

With respect to other parts of Hull's argumentation, Sober is inclined to make concessions. He allows that the claim that species evolve can be presented in language which construes species as sets, but doubts that "Hull places much weight on this rather 'linguistic' line of argument" (Sober 1984, p. 338). Two points should be noted here. First, the objection that treating species as sets is incompatible with a central claim of evolutionary theory plainly is a serious one; showing that the objection rests on the fallacy of incomplete translation clears away an important obstacle. Second, Hull and the many biologists who have been influenced by his defense of individualism (for example, Niles Eldredge and Joel Cracraft, E. O. Wiley, Steven Stanley) have perceived individualism to be a doctrine that liberates macroevolutionary theory from unnecessary constraints. The idea is that the old-fashioned construal of species as sets doesn't allow species as objects of selection: species selection presupposes that species are individuals. But, like the more general claim that

the old-fashioned view won't permit evolving species, this argument is also faulty. Only incomplete translation seduces one into thinking that sets (more exactly those sets which are species) cannot be selected. Once the fallacy of incomplete translation is exposed, another part of the supposed biological excitement evaporates.

Finally, in several places (most notably in his 1978), Hull appeals to the individualism doctrine to explain why species cannot re-evolve. I argued that historically disconnected species are permissible, and I described a hypothetical case, based on what we know of the evolution of lizards of the genus *Cnemidophorus*. (Other cases can easily be imagined: consider the possibility of constantly remaking the same bacterial species in the lab. Moreover, the morabine grasshoppers may furnish an actual case of the type I envisaged.) Sober's response is that individualists can allow for historically and spatially disconnected individuals. And that, of course, is true. Mereology countenances a myriad objects. There are individuals to suit all comers.

But we have now retreated a long way from Hull's original interesting claims. Let us see where the doctrine now stands. Let us first note that the thesis of individualism does not provide any solution (let alone a "radical solution") to the old species problem. It does not tell us how to carve the genealogy into species. If Sober's concession about disconnected objects is accepted, it tells us even less: the individuals which are species don't even have to be genealogically connected. The old-fashioned idea that species are sets turns out to be perfectly compatible with the main thesis of evolutionary theory and with the provocative proposals of some contemporary macroevolutionary theorists. Individualism can't be hailed as finally explaining why generalizations like "All Swans are White" fail to be laws, and it may be in trouble because it may rule out the possibility of any laws about particular species (though I suspect that a clear example of a law about a species would be deflected by exploiting the obscurities of the view that laws cannot make reference to individuals). What's left?

Let me conclude the discussion of individualism by offering an olive branch. Pick any organisms you wish, and you can form the set containing just those organisms. Equally you can form their mereological sum. If we began with all humans, past, present and future, we could thus form two objects. Which one is *Homo sapiens*? It seems to me that my original arguments, Sober's response, and my reply, combine to show that it doesn't make much difference whether you opt for the set or the sum. (Mereologists may have minor troubles in formulating cardinality ascriptions about species, but they can easily adapt the proposal of Glenn Kessler (1980) for understanding cardinality ascriptions to aggregates.) You can do the same biology and philosophy of biology in terms of mereology or in terms of standard set theory. Once the ambitious claims

originally made on behalf of individualism have been carefully examined, nothing distinctive is left to make any further fuss about.

**3. Genealogical Imperialism.** The final issue to be addressed concerns the extent to which a pluralistic view of species is defensible. As I understand Sober, he believes that current programs in evolutionary biology cast doubt on the viability of some species concepts. In particular, we must reject the many species concepts which allow for the possibility that there should be individuals *a*, *b*, and *c* such that *a* and *b* but not *c* are conspecific even though *a* and *c* are genealogically closer than *a* and *b*.

This view allows a limited pluralism. As I argued in my 1984, genealogical connection (what I called the “principle of continuity”) does not prescribe genealogical division. Hence there remain several rival species concepts. In the terms of my taxonomy, they are those historical species concepts in which continuity is paramount.

But I don’t want to settle for limited pluralism. I think Sober is far too sanguine about the latest thoughts in systematics, and that his enthusiasm should be tempered by historical reflection on earlier movements which have claimed to uncover the ground of organismic diversity. Before I defend this judgment, I want to reply briefly to some objections Sober raises for my version of pluralism.

Consider the Lake Turkana molluscs studied by Peter Williamson. I suggested that Williamson’s data could be understood from the perspective of two different species concepts. We can regard the fossil record as revealing a pattern of morphological diversification, independently of any claims about the attainment of reproductive isolation, or we can use the observed morphological changes as evidence for the attainment of reproductive isolation. Sober asks why the first approach should be considered as generating a species concept. I reply that the traditional theoretical problem of systematics is to map the organismic diversity in nature by producing nested sets of organisms which coincide with divisions produced by attending to biologically significant properties. (This problem is acutely in need of philosophical analysis, as I shall emphasize below.) My treatment of the Williamson data was motivated by the view that both morphology and attainment of reproductive isolation are contenders as criteria for dividing up organisms so as to produce the minimal units of the partitioning into nested sets, minimal units which are, traditionally, called “species.” Mayr has frequently reminded biologists and philosophers that appeals to morphology will sometimes lead to disaster: sibling species and species with pronounced sexual dimorphism together provide an argument that we might christen “Mayr’s pincers.” But reproductive isolation also faces troublesome cases, and, as I noted, the division that it generates will not solve our problems with asexual organisms. Thus

*neither* of these two proposals for mapping the diversity of nature will succeed in solving the theoretical problem of systematics. The heart of my pluralism is the thesis that the problem rests on a false presupposition: there is no one, privileged way to divide up the totality of organisms so as to pick out the true minimal units of diversity. Instead, we should recognize a number of different ways to produce divisions of the sort at which traditional systematics aimed. In each case, trouble will erupt somewhere. But, despite the troublesome cases each of the divisions corresponds to an objective pattern in nature.

Let me sum up by giving a direct answer to Sober's query. When should we talk about species and speciation—and why? The concept of species was introduced to answer certain theoretical desiderata, never explicitly announced, which prove to be unsatisfiable. Philosophical systematics should analyze these desiderata and help to construct the constraints on a realistic account of the species category, acknowledging that, while all of the traditional proposals fail to meet the traditional standards, some of them achieve partial successes. Admissible species criteria turn out to be those that are partially successful and thus meet the adequacy conditions imposed by the revised philosophical systematics. I claim that both Mayr's biological species concept and the kind of morphological species concept that is the stock-in-trade of many paleontologists will be admissible.

At this point I want to take up Sober's interesting idea that whether or not something counts as a new species is to be settled retrospectively (the appeal to Sid Caesar; Sober 1984, p. 339, footnote 7). Suppose that a small subpopulation becomes isolated, and that "Selection leads this group to diverge from the parent population, and thereby to count as a distinct species" (Sober 1984, p. 339). When did the new species begin? Sober thinks that it is natural to say that the species begins at the time of isolation. The founding group counts as part of a new species not because of intrinsic characteristics but because of what happened later.

I disagree. (I also find it odd that someone who seems sympathetic to certain Hennigian ideas should advocate a view of species which is analogous to the Simpsonian conception of higher taxa that Hennig vigorously repudiated.) Two words in Sober's story bear great weight: 'diverge' and 'thereby'. How exactly do the populations diverge, and by what means do they come to rank as separate species? If we knew then we could make a much more precise claim about the time at which speciation occurred. Crucial to Sober's account is the idea that the criterion for ultimately counting the populations as divergent is not known, so that, in our ignorance, we fall back on the time of geographical isolation as the moment at which speciation occurred.

Let me illustrate. Suppose that the descendant population ultimately becomes reproductively isolated from the contemporary population descending from the original parent population. Then someone who favors

the biological species concept or a species concept that divides lineages when two daughter populations become reproductively isolated will count two distinct species at this later time. But the time of speciation will depend on the fashion in which reproductive isolation was attained. If both the isolate and the parent population remained reproductively compatible for thousands of years after the geographical isolation occurred, then Sober's proposal will be rejected—speciation did not begin until long after the event in which the geographical isolation occurred.

The point generalizes. Once we have a way of identifying populations as sufficiently divergent to count as distinct species then we have a criterion which can be employed to determine the time and the way in which speciation occurred.

My remarks so far have been intended to cast doubt on a presupposition of Sober's reply to my defense of pluralism. The problems of dividing organisms into species are deep and longstanding. They are not to be dismissed with breezy assurances that contemporary evolutionary theory is on to something good, or that we can appeal to geographical isolation to divide lineages up for us. My final aim will be to sharpen the dispute between Sober and me, and I will conclude by indicating some of the areas in which I think genealogical species concepts are likely to be inappropriate and by explaining more precisely how I view pluralism about species as advancing systematic theory.

Usually propinquity of descent will go hand in hand with similarity in a number of biologically important properties. That is the way of evolution. To identify the places in which emphasis on genealogical connection may prove troublesome, we need to look at cases in which organisms which would be ranked closely together in terms of common descent differ markedly in other respects. One obvious type of case concerns very simple organisms such as bacteria and viruses. We can easily imagine that, in some lines of these organisms, mutations and reverse mutations should arise frequently enough to produce organisms which are genetically very similar in ways that cut across genealogy. The genetic differences may be reflected in divergent abilities to replicate on certain natural hosts or in certain naturally occurring media. No reasonable virologist or bacteriologist whose project was to understand the different ways in which replication, sheath synthesis, attachment, and so forth occur in these organisms would be interested in separating them into species that would reflect their descent. What would be crucial would be the genetic structures underlying the patterns of replication, sheath synthesis and attachment observed among the organisms in question. It would not even matter that, in some cases perhaps, the microorganisms in question had not "evolved" from ancestral types but had been deliberately manufactured.

So what? Genealogy may be irrelevant when we are dealing with mi-

croorganisms, but it is crucial when the topic is some group of *real* organisms—like the vertebrates we all know and love. But here too we may rightly sacrifice genealogy in the pursuit of other objective patterns in nature. First, let us note that the traditional biological species concept may cut across the division into species that is favored by those who stress the primacy of genealogy. Suppose that a lineage splits at  $t_1$ , forming two reproductively isolated branches, but that reproductive isolation breaks down (perhaps as the result of habitat disturbance) so that, at  $t_2$ , the descendants of these temporarily isolated lines are no longer isolated from one another. Assume further that, after  $t_2$ , no interbreeding actually occurs, so that, although the lines are reproductively compatible there is no gene flow between them. (This may be the result of geographical separation.) According to Mayr's biological species concept, the populations in the two lines at  $t_2$  are included in the same species. But, because of their descent from distinct species, the genealogical conception has to rank them as distinct.

Now, under what circumstances might one prefer to employ the Mayrian conception for grouping organisms into species? Consider the following scenario. The situation described in the last paragraph occurs frequently in some geographical region. A naturalist wants to compare species diversity in this region with species diversity in another region. How many kinds of animals do we have here? How many there? There is a traditional and powerful argument for basing the census on the biological species concept. What one wants to know is how many stable distinctions there are among the organisms that are around at present. Stable distinctions are marked by the attainment of reproductive isolation. Evolutionary history is likely to coincide with the division according to the criterion of reproductive isolation, but where it does not, we are concerned with the stable distinctions and not with the history of descent *per se*. Moreover, mere absence of actual mating does not reflect a biologically significant boundary between groups, but merely the accidents of geography. Hence we count species according to the number of groups that are reproductively isolated from one another, thereby producing (as Mayr has been fond of noting) a distinction which is recognized by the organisms themselves.

In sketching a scenario which would favor the biological species concept over the genealogical conception that Sober favors I have touched on the general motivation that led Mayr and Dobzhansky to articulate the biological species concept. Both writers see the attainment of reproductive isolation as crucial to the existence of organismic diversity because they believe in the homogenizing power of gene flow: diversity could only be maintained at a place if there were biological barriers preventing gene flow from reducing the differences among the organisms at that place

until those differences were extinguished. Hence reproductive isolation is hailed as crucial for the possibility of stable local diversity. However, a number of writers have recently questioned the idea that reproductive compatibility has the homogenizing power with which it has traditionally been credited (Endler 1977, White 1977). New approaches to the maintenance of the integrity of a species have invoked the idea of stable patterns of development: the species phenotype may be viewed as relatively invariant across a broad range not because of gene flow (which is, in many cases, demonstrably weak) but because it is very difficult for genetic changes to break up the developmental program. Considerations of this sort suggest a novel species concept, that concept which Sober ties to the notion of "Laws of Form." When the historical pedigree of the concept is traced, we can see it beginning from the same theoretical predicament that originally motivated the species concept of Mayr and Dobzhansky but diverging because of a different understanding of the genetic mechanisms that underlie species uniformity and species diversity.

It should now be clear that the idea that the term 'species' has been "preempted by another research program," so that one cannot argue for alternative conceptions of *species* is deeply flawed. In the first place, the issue does not concern *words* but the roles that certain words are intended to play in biological discourse. Second, thinking about species has a long and intricate history. A brief look at a recent part of that history should make clear how the latest attempt to link the species concept to cohesive patterns of development (or "Laws of Form") has its roots in an approach to the species category that has dominated most of twentieth-century biology. Thus, I think that Sober's brief for the priority of genealogical conceptions of species not only ignores the cases in which such conceptions prove troublesome, but also proves to be historically shortsighted.

Nevertheless, Sober raises an important and deep question. He concedes that biology has many kinds that cut across one another, and asks which of these kinds ought to be called "species." To give a complete answer to that question one would have to undertake the project I indicated above: Systematics is not an exercise in stamp collecting and labelling, but a discipline with broad significance for biology generally; its presuppositions need philosophical analysis. My pluralistic approach rests ultimately on the claim that, when the analysis is done, no one of the usual views of species answers to the traditional presuppositions of systematics. When the failures are apparent, we can see how to refashion the presuppositions of the discipline, and how each of a number of species concepts will count as adequate when judged by our new standards. In a nutshell, philosophers need to make clear what systematists mean by the enterprise of mapping the diversity of nature. I claim that once that enterprise is characterized precisely, we see that organisms are so diverse

that their diversity demands a diversity of diversities. Whether or not I am right in this latter claim, the preliminary task of trying to say clearly what systematics is supposed to be about (a task which I attempt in the work from which my 1984 is distilled) seems to me to be a very important one. We should not be led to ignore it by the allure of the monism of the moment.

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