Species Concepts, Individuality, and Objectivity

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ABSTRACT: Treating species as individuals makes it feasible to treat all the sciences from a unitary philosophical point of view. It clarifies the roles of history and laws of nature. Psychologism may prevent classification systems from meeting the criteria of scientific objectivity. Classification is not based upon putting similars together, but upon a scientific understanding of the objects classified. Biological species definitions can be treated coherently as reproductive communities, which are composite wholes, or individuals. Evolutionary species definitions, which treat species as ecological entities, are incoherent mixtures of individuals and classes, and have an undesirable subjective character. Species are ecological units only insofar as they affect the ecological aspects of reproduction.

KEY WORDS: Species concepts, individuality, classes, definition, psychologism, objectivity, taxonomy, phylogenetics, evolution.

INTRODUCTION

Many years ago I published a paper in *Systematic Zoology* entitled 'On Psychologism in the Logic of Taxonomic Controversies' (Ghiselin, 1966). It was there that I first brought up the point that biological species are not classes, but individuals. This thesis was long rejected as counter-intuitive, until it was arrived at, on the basis of independent lines of reasoning, by David Hull and Mary Williams. Although still controversial, it has been widely accepted, and widely discussed, by biologists, and has recently attracted considerable attention from professional philosophers (Holsinger, 1984; Kitcher, 1984; Sober, 1984). Some other points which I raised in that paper have received less attention, particularly the difficulties with similarity as a basis for classification, and "psychologism" as an undesirable feature of systematics. Such topics are very important, for they bear upon efforts to develop alternative definitions of the species and other categories. In this paper I will emphasize the differences between "biological" species concepts, as advocated by Dobzhansky and myself (less consistently by Mayr), and "evolutionary" species concepts advocated by Simpson, Wiley, and Van Valen. Actually these terms are not altogether fortunate, since either concept might be called "biological" or "evolutionary." The real issue is whether species should be conceived of, respectively, as reproductive communities or as ecological entities. At a more general level, I suggest that the biological species concept provides

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the basis for a fundamental integration of biology with the rest of science, and argue that there is no objection to it that would not equally apply to such basic terms as "atom," "molecule," and "cell."

DIFFERENT KINDS OF GROUPS

In order to make this essay intelligible to a broad audience, it will help to reiterate what we mean in saying that species are individuals (see also Hull, 1976, 1978; Ghiselin, 1974a, 1981). *Individuals* are single things, including compound objects made up of parts — such as ourselves, and also every cell and atom in our bodies. Such parts need not be physically connected — a baseball team is an individual made up of players. Individuals each have a definite location in space and time. In general they are designated by proper names — such as "Ernst Mayr" or "Canada." *Classes*, on the other hand, are spatially and temporally unrestricted, and their names may designate any number of objects — including none at all. The only thing the members of a class have to share is the criteria of membership — usually what are called the "defining properties." Both classes and individuals have "elements" or "subunits," but the relationships are not the same. Individuals are "parts" of larger individuals. (John is part of his family.) They are "members" of classes. (John is male.) Classes are not parts of anything, though they can be included in larger classes. (The sexes include male and female.) In general one can tell if a thing is a part of an individual or a member of a class by virtue of the fact that parts are not "instances." For example, we do not say "This thumb is a Michael Ghiselin," or "Ontario" is a "Canada." To some people it is not obvious that it is equally wrong to say "Trigger is an *Equus cabalus*".

In biological classification we have two kinds of groups: *categories* and *taxa*. Categories are classes, taxa are (or can be) individuals. Compare:

BIOLOGICAL HIERARCHY:

CATEGORIES	TAXA
Family	Hominidae
Genus	*Homo*
Species	*Homo sapiens*.

POLITICAL HIERARCHY:

CATEGORIES	TAXA
National State	U.S.A.
State	California
County	San Francisco County

Thus, in taxonomy it is clear that we have *both* classes and individuals. Individual species are populations. Individual higher taxa are branches of the phylogenetic tree — chunks of a genealogical nexus. At least they are if we chose to classify things that way. Some groups, called "artificial" taxa have to be viewed as classes (such as the warm-blooded animals, or Haematothermia = Aves + Mammalia).

GENERAL IMPLICATIONS

From such simple considerations, a great deal follows. For example, evolution is possible. If species were not individuals, they could not evolve. Indeed, they could not do anything whatsoever. Classes are immutable, only their constituent individuals can change. "National state" (a class) cannot wage war — but Canada (an individual) has waged war. Individual species may be able to do things other than evolve. They can speciate, become extinct, and compete, but perhaps not much else. This is a very important point, one to which Eldredge (1985) has devoted an entire book. That species are individuals means that they cannot be defined, in the sense of listing properties they simply must have — but they can be described (see Ghiselin, 1984). This of course has profound implications for the role taxonomic groups play in the thinking of biologists.

But perhaps the most important implication of the individuality of species thesis is that it holds out the prospect that we can develop a single body of knowledge for the entire universe. We can have one science, and one philosophy of science. This possibility has been rejected by many authors, including Mayr (1982) and other eminent evolutionists, on the grounds that there are no laws of nature in biology. It was the notion of Smart (1963) that biology is not a science because there are no laws for *Homo sapiens* that led Hull to realize that species are individuals. Of course there are no laws for taxonomic groups, but there are no laws for individuals in physics, either. Laws are generalizations about classes of individuals. We therefore must look for biological laws that generalize about classes of species and classes of other individuals. It is ironic that Mayr (1982), who denies that there are any important laws in biology, devoted much of a brilliant career to defending a law of allopatry in speciation (see Mayr, 1963). The law says that under ordinary conditions, speciation does not occur without an initial period of extrinsic isolation, such as a geographic barrier. If biology is to be a science, it needs both a body of descriptive facts, and a series of high-level generalizations. In other words, it needs both history and laws. Astronomy provides just such descriptive facts, and physics the laws of nature. In biology, the historical data are supplied by taxonomy, the laws of nature by evolutionary theory. Thus, in principle, there is nothing but the particular subject-matter to differentiate the physical from the biological sciences.

Some opponents of the thesis that species are individuals insist that there must be some sort of "essence" underlying the "form" which purports to be the subject-matter of taxonomy. Others have argued that if we accept it, then morphology cannot be the kind of nomothetic science that crystallography and chemistry are. A particularly good example from the recent anti-Darwinian literature is Webster (1984). And of course they are right, but this only means that their efforts to find "nomothetic" alternatives to Darwinism are futile. On the other hand, many who have been seeking to develop a better theory of macroevolution take the individuality of species as one of their fundamental premises (Eldredge and Cracraft, 1980; Vrba, 1974). In general it seems that those who are trying to relate evolutionary history to the underlying causal mechanisms accept the individuality of species. Those whose approach to evolution is ahistorical or who eschew the study of mechanisms are less sympathetic to it, if not downright hostile.

ON OBJECTIVITY

By clearly distinguishing between the historical and the nomothetic aspects of biology, between individuals and classes, it will be easier to make biology a "hard" science. But that is not enough. How do we differentiate between what is, and what is not, science? One way is in terms of objectivity (see also Ridley, 1985). Objectivity relates very nicely to the topic of "psychologism" earlier alluded to. Traditionally it has been discussed under the rubric of "natural" and "artificial" systems. In 1966 I exemplified "artificial system" by "a ranking of inkblots in the order of their obscenity." A natural system is something we discover, not something we create. Darwin (1859: 411) probably had something of the sort in mind when he wrote that the natural system "is evidently not arbitrary like the grouping of the stars in constellations." A better word than "arbitrary" would have been "subjective." A host of authors have, indeed, denied that there is any such distinction. They have asserted that a classification is "a construction of the intellect," something "merely for the sake of convenience," and so-forth. When species were conceived of as classes such opinion seemed not unreasonable. After all, individuals are "concrete," classes "abstract," and perhaps classes might be viewed as something "mental." Now that species are conceived of as individuals, they have to be absolutely concrete, and must be viewed as no more intellectual constructs than organisms are. Some people have tried to argue that an entity is a class or an individual depending upon how we consider it. But if this were so, the ability of an organism to develop or of a species to evolve would depend upon someone's thought processes. In some cases efforts to treat species as conceptual have been based upon "phenomenalism." According to one version, what we are supposed to classify is not organisms or populations, but sense-impressions!

Evidently such arguments appeal to persons with a certain kind of personality structure. According to Jung's (translation, 1971) classical theory of psychological types, we can divide people into introverts and extroverts. Introverts organize their lives in relation to the subject, extroverts in relation to the object. It has been shown that different endeavors attract different psychological types — professional psychologists generally classify themselves as introverts (Shapiro and Alexander, 1975). The importance of an objective approach in the sciences has been particularly well argued by psychologists (Brunswick, 1952).

On the other hand, the subjective aspect is indeed important in taxonomy; not, however, in classification, but rather in identification. Simpson (1961) made this point quite effectively, although he confused matters by saying that we classify groups not individuals, for some groups are individuals. When we identify objects, we relate them to a pre-existing system — as by dumping corpses into bins. Finding out the real, underlying order — classification in the strict sense of that word — is quite a different matter. Unfortunately our educational routine is so arranged that everybody is taught to identify, but virtually nobody is taught to classify, and then only at a late stage. In chemistry courses we were all taught to identify elements — but not to invent the periodic table. In biology everybody learns how to key out an organism — but not to revise a taxon. The few who do learn to classify have already been prejudiced by the experience of identification, which therefore becomes a stereotype, or Platonic Idea, of classification, the basis for a travesty of how objects are classified by scientists. Such a travesty is almost universally taken for granted in accounts of how we do it, whether presented by philosophers, or by taxonomists of whatever school. Namely, we are told that classification is somehow "based upon" characters. Of course, this is true in the trivial sense that counting scales and measuring apertures provides useful data. But this does not mean that sorting corpses into bins on the basis of shared attributes constitutes the entire rationale of all classification systems. In other words "observed traits of specimens" ≠ "evidence." I have repeatedly offered arguments and counter examples (Ghiselin, 1966, 1969a, 1969b, 1970; Gosliner and Ghiselin, 1984). Systematic biology ought to base its generalizations on the kind of historical reconstruction that has been general practice in geology ever since the time of Lyell, in which hypothesized sequences of events are evaluated according to how well they accord with the known laws of nature. And in fact the best workers, beginning with Darwin, have done so routinely. For example, vestigial organs have to be treated as something being lost, not as something preparing to become functional in the remote future. With a few noteworthy exceptions such as a new book by Ridley (1985) such operations have been studiously ignored by theoreticians of taxonomy.

Contrary, therefore, to what is generally asserted, I maintain that a scientific classification is based upon an understanding of the objects classified, and that any characters are therefore epiphenomenal. I hope that what I say will seem so obvious that people claim never to have believed otherwise. Of course, a classification can be based upon a *misunderstanding* of the objects classified, but this only means that we must have an objective criterion of whether the classification be true or false. One kind of psychologism results when we equate ontology with epistemology, and it is to be avoided here. Epistemologically, a natural system is based upon whatever scientific evidence legitimizes it. Ontologically, a natural system is based upon the objective reality to which it corresponds. And by an understanding, I mean a scientifically legitimate theory, not a mental condition, or what some philosophers (in the tradition of the southwest German school of Neokantianism) have referred to as *Verstehen*. To be more explicit, scientific classifications are etiological, or causal, not phenomenal, or based upon superficial appearances. In scientific medicine, diseases are classified upon an etiological basis — by what causes them, sometimes the agent, sometimes the physiological effects. They are diagnosed on the basis of symptoms, but these symptoms are not defining of the disease. This same distinction is evident in biological taxonomy: we too call the analogue of the "symptoms" a "diagnosis," and not a "definition" (Ghiselin, 1984). And again, I stress that the "symptoms" are not exhaustive of the evidence, which includes experiments, theories, and anything else germane.

The causes upon which etiological classification is based generally fall under two major headings: history and law. This implies that the groups correspond to individuals such as the French language, *Homo sapiens*, or the Palaeozoic on the one hand, and to classes, such as homeotherms, chlorine, and competition. The individuals are held together by physical or other forces, or at least have a common origin, while the classes behave in the same way because they are subject to the same laws of nature. The very fact that we recognize that the order in taxonomy is an historical one clearly shows that the evidence is not exhausted by "characters." So does the fact that our interpretations are influenced by such truisms as the point that the common ancestor of a group of lineages must have existed in a restricted position in space and time.

BIOLOGICAL SPECIES

I now propose to develop the thesis that classification is based upon an understanding of the objects classified by reference to familiar examples, getting to species in due course. We may begin with the periodic table of the elements, which as we all know consists of classes of elements, and treats each element as a mass noun, though atoms are obviously the individuals in the system. Larger groups such as the halogens include smaller ones such as chlorine. This periodic table provides a very powerful summary of chemical knowledge because it summarizes the important chemical properties, and can be related to the structure of the atoms, and not because of what the objects classified look like.

Some matter, but not all, is incorporated in atoms, a point to which I shall return. Some atoms form parts of molecules, some molecules form parts of larger wholes such as organisms. This gives us a familiar hierarchy, which we now need to examine more closely:

$$\begin{array}{l} \left.\begin{array}{l}\text{Species}\\ \ldots\\ \text{Deme}\end{array}\right\}\text{Population}\\ \text{Organism}\\ \text{Cell}\\ \ldots\\ \text{Molecule}\\ \text{Atom}\end{array}$$

Observe that entities can be ranked at more than one level (as in a unicellular organism). Also, "population," as the term is used here is a class of classes (levels), with "species" ranked highest and "deme" ranked lowest. ("Population" has other senses, for example, it may be equivalent to "deme.") It is obvious that different kinds of objects are ranked at each level. We should say a little about what kinds of objects these are.

Atoms and molecules are structural units. They have structure. Cells and organisms are organizational units. They are organized. Of course, cells have structure, and some molecules are organized too. At the populational level, we also find organized units, but of a different kind. (In this discussion I will not attempt the formidable task of defining "structure" or "organization".) The organismal level can be defined in terms of physiological autonomy, the cell as the minimum unit capable of having such autonomy. Can we treat populational units the same way we did the lower ones? Can we bring the definition of "species" in line with the definitions of the lower units? I think we can, but before I do, it will be necessary to say a bit about the theory of definitions, which becomes more difficult here.

Generally a definition is understood to be the criteria that determine whether a name is to apply to a given object or class of objects. Sometimes this is done by giving the extension — enumerating all the members of the class. I can do this in principle for "element" — listing "hydrogen, helium. . . . ." But generally, we call for an intensional definition — a list of the properties necessary and sufficient for the name to apply: a molecule is a group of atoms united by a continuous series of chemical bonds. (Individuals have to be defined "ostensively" — by "pointing" — for they have no defining properties whatsoever.)

There is another notion about definitions, which is that we are trying to define "concepts." The term "concept" is most problematic. In some cases it is tainted with psychologism, as when concepts are equated with our "ideas" about things. With respect to species, however, we have an important issue, because one might define the term in more than one way, and yet have it refer to precisely the same class of objects. This is one sense of "concept." Thus we might get "genetical" and "evolutionary" definitions that are equivalent, in the sense that the entities with a certain genetical structure are precisely those that evolve. The problem here is that "species" is defined in the context of a body of evolutionary theory, just as "molecule" is defined in a way that presupposes a lot of chemical theory. The terse "definitions" in glossaries and textbooks rarely if ever do full justice to the criteria of usage applied by scientists in thinking and communicating. An essay, or even a book, may not suffice. My position here is akin to the virtual truism that "species" is a theoretical term. However, one would be hard pressed, in science, to find any word the definition of which is totally free of theoretical content.

Another notion that should be mentioned here to clarify my position is Locke's distinction between "nominal" and "real" essences. My views are similar in some respects, but not others. In the first place, I do not believe in essences, or in "real definition." Definitions are all nominal in the sense that we define names and not the things named. I distinguish three levels at which definition occurs, the first two of which roughly correspond to Locke's nominal essences. In the first place we have "diagnosis," which is not definition proper. In the second we have definition in the sense of a minimal set of criteria necessary and sufficient for the name to apply. Finally, we have the deepest level, what might be called a full "explication" of the term, providing a complete set of rules for how it is used in discourse. It would appear that some people have this deepest level in mind when they speak of defining concepts. It does not seem necessary to invoke essences here, as is sometimes done in the theory of "natural kinds." The deepest level is simply a group of facts, laws, and principles, from which the others may be derived. When one discovers these facts, laws, and principles, one has not discovered the essence or definition of what the name designated, but merely put one's self in a position to structure one's language in an appropriate way. The development of evolutionary biology did not lead biologists to discover what known entities called "species" were. Rather, it was found that the groups traditionally recognized could be identified as a collection of artificial assemblages and different kinds of populations. We have chosen to use the word "species" to designate one such kind of population. Here, as elsewhere, we scientists do not attach a name to a class, then discover the defining properties which are its essence, but rather redefine our terms as knowledge advances. Therefore the view of Kripke (1980) and his followers (see Schwartz, 1977) that natural kinds terms are, like proper names, "rigid designators," should be dismissed as nugatory, and with it the accompanying essentialism.

We may also "conceive" of the same object or group of objects from different points of view. Now a point of view is neither true nor false, but it can be misleading or inappropriate for science. One point of view that virtually all of us would reject is the mystical point of view, a good example of which is the "quinary" system, a numerological approach popular early in the nineteenth century, which arranged everything in circles grouped in fives. But a fair number of reputable scientists advocate what may be called a "subjective" point of view in the sense already discussed earlier. Sometimes this takes the form of a "phenetic" species definition — the subjective notion of "degree of similarity" being treated as if it corresponded to some objective fact. In another form we get Tate Regan's aphorism that "A species is whatever a competent systematist says it is." A more recent example is Mishler and Donoghue (1982: 492) who deny that the biological species concept "will yield the same sets of organisms that would be recognized as 'species' by a competent taxonomist, or by a person in the street." A competent taxonomist, by a definition other than theirs, will of course see things quite differently than will a child on the street. Analogy with science in general shows the fallaciousness of the argument. We accept the heliocentric model of the solar system, not the geocentric model of the universe, in spite of what seems reasonable to non-scientists. Chemists do not put sucrose together with sugar of lead.

The position taken by many who advocate a subjective species concept might be labelled "pragmatic authoritarianism" — words are defined in terms of what is good for the experts. It is claimed, especially by certain botanists, that the biological species definition is somehow "impractical." The facts do not bear out these claims. Indeed the facts have been widely misrepresented. The biological species definition turns out to be fully applicable to those plants in which species actually do exist. Some people just feel disinclined, say, to synonymize a lot of oaks. The only relevant scientific facts are that sometimes populations diversify a lot before they speciate, and others are hard to tell apart when they have speciated. Therefore doing taxonomy well often requires a lot of work. If one is lazy, one may not feel inclined to do the work. If one is incompetent, one may not succeed. If one is dishonest, one may not wish to admit that one does not know all the answers. Therefore, a species is, by definition, whatever some expert finds it expedient to label with a specific epithet. That subjects differ in their ability or inclination to do cytology, experiments, electrophoresis, biogeography, or any number of other tasks, necessitates that such definitions will be as diverse, and as idiosyncratic, as human personalities. The experts will disagree, not because they differ upon a scientific issue, but because they are not engaged in science at all.

Some authors have argued for "pluralism" in this connection (Mishler and Donohue, 1982; Kitcher, 1984). In effect this means that one can pick and chose among a variety of criteria, such as reproductive isolation, and similarities and differences in this, that, and the other. But we are not told how to make the criterion of membership be an objective one. Such pluralism does not characterize such terms as "atom" and "molecule." It has been argued (Kitcher, 1984) that the intension of "gene" is not monolithic. It has been broken down into such entities as cistron, muton, and recon. But the analogy here is false. The situation is more like that in population biology, were "population" means a variety of things, including species and deme. There is nothing analogous to a subjectively-defined class of similars in genetics. "Gene" has always designated a class of functional individuals, even when these have been hypothetical. In the case of species, the pluralists ask us that it designate not just a range of classes, but an incoherent mixture of classes and individuals. It is like defining "nation" in such a manner that the French Nation means Frenchmen, plus some people who speak French, plus those with a French physiognomy, plus people who drive Renault automobiles *et cetera ad nauseum*.

From an objective, theoretical point of view, we might characterize the objects that fall under different levels according to what those objects do. Atoms may enter into chemical reactions, and become parts of molecules. Organisms do neither of those things, but may reproduce and engage in social behavior. Evolution is possible at the population level. But we need to restrict ourselves to a particular *kind* of population, namely, a whole integrated by sexual reproduction. (Yes, there are evolutionary changes in lineages of asexual organisms, and there are evolutionary changes in such things as languages and other cultural wholes.) Sexual populations, or reproductive communities, consist of populations and subpopulations, the largest of which are species, the smallest demes. We can, therefore, define "species" as the class whose members are the largest such units. And we do. Yet we can equally well define "species" by saying that species are by definition the populations that speciate (Ghiselin, 1969, 1974a). This definition may seem circular, but it does not have to be. In theory at least, one can explain what a population is, and how they speciate, without ever having used the word "species." Therefore we can conceive of the same class of objects from two different points of view, and have quite different "defining properties" for it. In the context of evolutionary theory, the two are perfectly interchangeable. Both are "biological," or "evolutionary" species concepts, insofar as the same individuals participate in two important evolutionary processes: they become transformed, and they speciate.

We have yet a third way of formulating what is fundamentally the same species concept. This is to say that species are those individuals that have to evolve independently of each other. For this to happen, it is a necessary condition that they form separate reproductive units, and a sufficient condition that they have speciated. Again, the same basic group of propositions about biology is being invoked, at least implicitly. By the same token, we can also specify what properties of a species would be sufficient for speciation to have occurred. This was the basis of Mayr's (1940 and later) formulations of the biological species definition: "groups of actually or potentially interbreeding natural populations which are reproductively isolated from other such groups." But one might equally well formulate one's definition from the point of view of some other feature of the very same populations. I (Ghiselin, 1974a: 538) did this using reproductive competition as the defining property: "the most extensive units in the natural economy such that reproductive competition occurs among their parts." It was felt that this definition had the advantage of not having to invoke such dispositional properties as "potentially interbreeding." (One encounters similar problems with various social groups — for example, a class that is not in session.) Also it focuses attention upon the evolutionary process, selection, that is so basic to the theory.

Others have preferred to emphasize what happens to genes. Dobzhansky (1935) deserves much of the credit for relating biological species to modern genetical theory. However, genetics has introduced a great deal of confusion, because of an unfortunate habit of overemphasizing its significance. This has led to a mistaken view that biological species definitions are "genetical." Actually, speciation can occur without genetical change, as when isolating mechanisms are the result of learned behavior. Another misconception has been that species are classes, which, by definition, share a certain degree of "genetical similarity," whatever that is supposed to mean. Some authors have treated species as if they were classes of populations, rather than populations that may contain smaller populational units. A species might be reduced temporarily to just one organism. Van Valen (1976: 235) wrongly emended my 1974 definition to read "the most extensive units in the natural economy such that reproductive competition [for genes] occurs among their parts." I had explicitly stated that by "reproductive competition," I meant that process which we observe in natural and sexual selection. It is not a competition "for genes" but investment of resources so as to maximize reproductive success, or Darwinian fitness. If it is "for" anything, it is "for" the ability to contribute most to the ancestry of subsequent generations. By definition it has nothing to do with interspecific competition, contrary to what Van Valen asserts.

Therefore, the definitions thus far considered may all be considered interchangeable verbal formulae defining the same class of entities, albeit from somewhat different points of view. Again we have just one "concept." There have been many objections to this "biological" species concept, but these can be answered by means of straight forward counter-examples. Among the most popular has been the point that not every organism falls under a species. In particular, asexual lineages do not form reproductive populations, and have to be considered "pseudospecies." It is sometimes said that a species definition which included all organisms would be better, because it would be "more general." True, but is it always desirable to maximize the generality of a term? If so, "virgin," ought to include those who have copulated as well as those who have not. Scientific classifications quite generally omit certain objects. Not every elementary particle in the universe is part of an atom. Not every part of an organism is a cell or part of one. And not every organism is part of an organization or society of a given kind. We may compare species to corporations, trade unions, or churches. A church is an organization, but not everybody belongs to one. "Religion" is more general than "church, but the former is not necessarily organized. One believes in a religion, one belongs to a church. Likewise, we ought not to conflate economic roles with economic organizations. The people who earn their living as barbers are one thing (a class); those who belong to the Barbers' Union, form another (an individual). If one does not belong to the union, one need not pay dues, but it may be harder to get work. Similarly, organisms that are parts of species have to pay the "cost of meiosis" in consequence of their sexuality — but for some mysterious reason they seem quite prosperous.

EVOLUTIONARY SPECIES

It is at precisely this point that we must take issue with the advocates of the so-called "evolutionary species concept." They try to define "species" in terms of "evolutionary role," or "niche." As a result, they confound professions with organizations. Thus a business firm, for them, would have to include those who had once worked for that firm, but had quit, and gone into private practice. Indeed, the firm might have long since been liquidated. In effect they are trying to have species be two quite different kinds of things at once. On the one hand they want species to be individuals — so they can evolve. On the other hand they want them to be classes of "ecologically similar" organisms. There are good metaphysical reasons why this will not succeed very well. If anything is to evolve, or do anything else, it has to be an individual — but an organization plus its former parts is just an historical unit, and cannot function as a whole either. Furthermore, an individual remains the same individual, irrespective of its activities. One does not become a different organism when one ceases to be a student and becomes a teacher. If, following Darwin, Elton, and Gause, we treat a niche as a "place in the economy of nature," it should be clear that where an object is located does not affect its being that object, irrespective of whether the place is a geographical or an economic one.

The simplest version of the ecological criterion occurs in Mayr's (1982: 273) latest emendation of the biological species definition. "*A species is a reproductive community of populations (reproductively isolated from others) that occupies a specific niche in nature.*" Actually Mayr rejects the ecological criterion as defining, but applies it to asexual clones and the like. In effect he is conferring a kind of "honorary" status on things that don't fit in. (Rather like an "honorary virgin!") Mayr is justified in emphasizing the fact that species must occupy niches, but to say that a species occupies a specific niche is a truism, rather like adding "occupies a domicilary location" to the definition of "home."

The most influential version of the evolutionary species definition is that of G. G. Simpson, who writes (1961: 153) "*An evolutionary species is a lineage (an ancestral-descendant sequence of populations) evolving separately from others and with its own unitary role and tendencies.*" The use of singular expressions ("a lineage," "its," and "unitary role") suggests that Simpson was conceiving of species as individuals. He also says that they can change roles, but must have one role at a given time. But do the asexual organisms form a lineage? Why not call them several lineages? And how does one decide how much similarity there must be in the "role" ("whole way of life")? He doesn't really tell us.

Wiley's (1978, 1981) formulation modifies that of Simpson. As Wiley (1981: 25) puts it "*An evolutionary species is a single lineage of ancestor-descendant populations which maintains its identity from other such lineages and which has its own evolutionary tendencies and historical fate.* He adds: "Identity is a quality that an entity possesses which is a by-product of its origin and its ability to remain distinct from other entities." He lists that which confers identity — the ability of organisms to recognize each other, similar niches, and "phenotypic or genotypic similarity." In effect he is saying that the elements of such species are "similar" one way or another. Evidently he wants these objects somehow to be "the same thing," but in what sense? When we speak of two or more individuals being identical, we usually mean that they share all the properties of some class. Two individuals cannot be absolutely identical, for that means that they are the same individual, not two, as is the case of the Morning Star and the Evening Star. So when we speak of the identity of a lineage or a species to itself, we are only repeating the point that it is one whole. Therefore Wiley is treating a whole lineage as if it were something more than the totality of descendants of an ancestor, or rather as less, because if some of those descendants become somehow different, they would not be parts of that whole. But he defines "lineage" in a strictly genealogical manner, as "holophyletic" units — i.e., ones that incorporate all of the descendants of the common ancestor. Thus, his species can be "paraphyletic" — i.e., they can leave out some of the descendants of the common ancestor. This means doing precisely what, as a good cladist, he tells us one ought not to do. What makes an entity a whole is not "identity," in Wiley's sense of mere distinctness from other things, but "integrity," or "integration." Wiley (1981) rightly asserts that such integration is a necessary condition for a group to do anything. Clearly, however, mere "similarity," be it genotypic, phenotypic, or whatever, does not integrate a group. According to my analysis, entities may be divided into classes and individuals. Wiley (1980) split "individual" into "individual" and "historical entity." By this he implied that integration is a necessary condition for individuality, whereas I say that it is sufficient, but not necessary. Be this as it may, his treatment of species as if they can be integrated wholes, merely historical entities, a mixture of the two, or even parts of what I call individuals, is incoherent and contrary to the more fundamental principles upon which the two of us agree.

Van Valen (1976: 333) writes that "A species is a lineage or a closely-related set of lineages, which occupies an adaptive zone minimally different from that of any other lineage in its range and which evolves separately from all lineages outside its range." He should have said that a species is *either* a lineage which occupies an adaptive zone minimally different from that of any other lineage in its range, and which evolves separately from all lineages outside its range, *or* a closely-related set of lineages which *occupy* an adaptive zone minimally different from that of any other lineage in *their* range and which *evolve* separately from all lineages outside *their* range. This rewording gives the game away. Van Valen was trying to make "species" be two different kinds of things at once.

But this raises a very interesting issue. What is the connection between the taxonomic hierarchy and ecological classification? Basically, there is no particular connection, though there are diverse correlations (Eldredge, 1985). Lineages and populations diversify through time, and tend to occupy different niches. But closely related organisms can have very different niches, distantly related ones virtually identical niches. Leaving aside the problem of the subjectivity of "amount of difference," it may be said that there is no clear limit to the amount of difference that can be built up within a species. Darwin (1859: 424) drew attention to the dwarf males of some cirripedes — an extreme case of sexual dimorphism. Two species can easily occupy identical niches when living in different places, and even when living in the same place, if certain unusual conditions are met. Van Valen (1976) wants to make the properties of other species in the range give the amount of difference. But note what happens when the local biota changes. We obviously have a rubber yardstick here. Likewise Shaposhnikov (1984) attempted to argue that species are neither classes nor individuals but "systems" — whatever that is supposed to mean. Evidently he intended to treat them as "superorganisms," something that most of us would reject as bad metaphysics, if he means that species must therefore have adaptations. (For a general discussion of the superorganism notion see Ghiselin, 1984b.) Shaposhnikov (1984: 1) says "A population is a group of individuals in a particular ecosystem whereas a species is a set of populations bound to a set of similar ecosystems." In effect he says that a species is a class of populations and organisms that are similarly adapted. So it all boils down to shifting from morphology, to genetics, and, as a last resort, to ecology, as a basis for defining "species" as classes of similars.

It would seem that species do very few things, and most of these are not particularly relevant to ecology. They speciate, they evolve, they provide their component organisms with genetical resources, and they become extinct. They compete, but probably competition between organisms of the same and different species is more important than competition between one species and another species. Otherwise, they do very little. Above the level of the species, genera and higher taxa never do anything. Clusters of related clones in this respect are the same as genera. They don't do anything either.

The shift toward defining species as a class of ecologically-similar organisms is an act of desperation, after the failure of efforts to define that category in terms of morphological, and then genetical, similarity. When, I repeat, we say that two individuals are identical, we mean that they share all of a certain *specified set* of properties. It is literally meaningless to say that two objects are "just plain" identical. There must be at least one thing not in common to them, if they are two individuals. In some cases by "identical" we really mean indiscernible, to us. But this is purely subjective and the criteria of identity are provided, if only implicitly. Now, similarity between two individuals mean that they share *at least some* of a specified set of properties. Identity is the limiting case. Apart from that specified set, it is nonsensical to say that two objects are similar. I can prove that any two objects in the universe are similar. My telephone number and Mt. Everest are similar, in that both are mentioned in this sentence. Two objects are objectively similar, insofar as the specified set of common properties is not a creation of the subject. Water is objectively similar to ammonia in containing hydrogen. Ammonia is subjectively similar to mushroom soup in that I don't like the taste of either. That water and ammonia contain hydrogen is a fact about those substances, how they taste is a fact about me. Many statements about the purported objective similarity of groups of objects are really statements that actually contain a subjective component. If I ask someone to sort flowers out according to color patterns, he probably will not do much with ultraviolet data.

It has repeatedly been presupposed that there exists a real set of attributes called "overall similarity," but this proposition has insuperable difficulties. For one thing, the various components would have to be commensurable, for another we would have to scale them, and find a way to mix qualities and quantities in a meaningful way. Often people think they are measuring "overall similarity" when they actually are doing something quite different. If we are doing molecular phylogenetics, we can trace changes, stepwise, from a common ancestral form. There is a general rule that as time passes the proportion of homologous sites that are identical decreases. This legitimizes DNA X DNA hybridization techniques. However, in different lineages the changes will be qualitatively different: substitutions, deletions, duplications, inversions, etc. The technique works because just one class of properties is being measured — the proportion of identical sites — and that happens to be a function of time elapsed since common ancestry.

When we move from mere molecular structure to trying to compare ecological properties of entire taxa, we get the same basic problem with a vengeance. Taxa differ ecologically from one another in various ways, and these are not fully commensurable. Consider, for example, the cnidarians with alternation of generations. In some cases we have medusae ("jellyfish") and polyps living in quite different places, and with different ranks assigned to synonymous taxa developed for the two stages of life history. In some cases the medusa is lost, in others the polyp. In many groups of marine invertebrates, a pair of "cryptic species" will occupy indistinguishable niches as adults, but one will have a larva, the other will not. How are we to decide whether the niches are minimally different? To this we may add the point that a species may itself remain unchanged, while the environment around it changes. Thus extinction of one species can readily advance another to the position of "top predator."

For any number of such reasons, there is no hope for treating species as classes of ecological similars. Species are important in ecology, because they are fundamental reproductive units, nothing more, nothing less. Ecology needs to generalize about reproduction, and has a legitimate place for species and classes of species. But it also needs to generalize about other processes, and ought to use such class terms as "primary consumer" and "homeotherm." Probably counting taxa of any rank is not the appropriate way to analyze diversity.

Furthermore, even though organisms are organized into species, they are also organized into other kinds of units, some of which are of considerable importance in ecology. An organism can be part of only one species, but it can be part of many kinds of social and ecological units as well. These include such things as families, societies, symbiotic unions, communities, and physically-constrained parts of the habitat. The very heterogenetiy of such units precludes there being any monolithic hierarchy such as that we find when passing from species downward, through organism, cell, molecule, and atom. There is structure here, but it is an economic structure, with all sorts of units engaging in a wide variety of economic activities. So if one wants to fit species into an ecological context, one has to treat them on an individual basis, deriving their ecologically interesting features through observations upon all sorts of particulars. In other words ecology may need a lot more descriptive natural history and taxonomy than some people would hope.

CONCLUSIONS

The analogy with physics and astronomy may be of some help here. Astronomy has its structural units — galaxy, star, planet, satellite, etc. That an object is classified as a planet tells us something about its position relative to other objects, but it does not tell us much else. Thus the fact that a given body is a planet, and a satellite of a star, tells us how it relates to certain other cellestial bodies. But it does not tell us how big it is, or how many moons it has, if any. Some of the planets in the Solar System are smaller than some of the moons of Jupiter and Saturn, for instance. By the same token, there is no reason to expect the rank of a taxon to tell us much about its important ecological properties. These, like the mass and position of a celestial body, have to be provided by naturalists. This does not mean that ecology will never develop the analogue of celestial mechanics. Rather, the laws of ecology are one thing, the particulars of evolution are another, and to know either it is crucial that we not confuse the two.

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