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ELIMINATIVE PLURALISM*

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This paper takes up the cause of species pluralism. An argument for species pluralism is provided and standard monist objections to pluralism are answered. A new form of species pluralism is developed and shown to be an improvement over previous forms. This paper also offers a general foundation on which to base a pluralistic approach to biological classification.

1. Introduction. The species category plays two intimately connected roles in biology. The first occurs in biological systematics. Systematists attempt to provide a taxonomy of life using, for the most part, the Linnaean framework. Species taxa are the basal units in that taxonomy, higher taxa (such as genera, families, and classes) are composed of species taxa and form more inclusive units. The second major role of the species category occurs in evolutionary biology. While systematists attempt to provide a taxonomy of the organic world's diversity, evolutionists attempt to explain why that diversity exists. An essential part of that explanation is that species taxa are "the evolutionary units" of the organic world—groups of organisms that evolve as units due to their exposure to common evolutionary forces (Mayr 1970, Dobzhansky 1970).

Given the fundamental role of the species category, a proper definition of that category would seem crucial for systematics and evolutionary biology. Unfortunately biologists widely disagree on how to define the species category. A recent anthology on species (Ereshefsky 1992) contains no less than eight prominent definitions, and these eight are just a small sample of the dozens of definitions found in the current biological literature. Of course disagreement over the nature of species is nothing new. Since and before Linnaeus, biologists have disagreed on the nature of species. For example in a letter to botanist Joseph Hooker, Darwin writes:

> It is really laughable to see what different ideas are prominent in
> various naturalists' minds, when they speak of 'species'; in some,
> resemblance is everything and descent of little weight—in some re-
> semblance seems to go for nothing, and Creation the reigning idea—
> in some, descent is the key,—in some sterility an unfailing test, with
> others it is not worth a farthing. (Darwin 1887, vol. 2, 88)

Biologists and philosophers have taken one of two approaches to the
diversity of species definitions found in the biological literature. Some
consider the species problem an unfinished debate in which the proper
definition needs to be weeded from the improper ones (see, e.g., Hull
1987, Ghiselin 1987, and Mayr 1987). Others hold that there is no com-
mon and distinctive attribute of all species taxa, thus the species category
is heterogeneous (see Ruse 1969, 1987; Dupré 1981; Mishler and
Donoghue 1982; Kitcher 1984a, 1984b, 1987; Mishler and Brandon 1987).
The first group of authors advocate species monism, the second promote
species pluralism.

In this paper, I take up the cause of species pluralism. Though others
have already advocated species pluralism, their versions are defective.
Thus I offer an alternative brand of pluralism. In particular, my aim in
this paper is fourfold: first, to provide a comprehensive argument for spe-
cies pluralism; second, to answer various monist objections to pluralism;
third, to offer an alternative form of species pluralism, which I call "elim-
inative pluralism"; and fourth, to show that eliminative pluralism is an
improvement over previous forms of pluralism.

2. The Case for Species Pluralism. Biologists offer various definitions
of the species category. Many of those definitions fall within three general
approaches to species.[1] The first approach, the interbreeding approach,
is best known through Mayr's biological species concept. According to
Mayr (in his most widely accepted version of that concept), "species are
groups of interbreeding natural populations that are reproductively iso-
lated from other such groups" (1970, 12). In other words, a species is
the most extensive group of organisms that interbreed and produce fertile
offspring. Furthermore the members of a species are separated from all
other organisms by "isolating mechanisms" (ibid., 55ff.) These mecha-
nisms either prevent interbreeding between interspecific organisms or pre-
vent the production of fertile offspring if such interbreeding does occur.
A more recent species concept that falls within the interbreeding approach

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[1]These approaches do not contain all of the definitions currently proposed by biologists
(for a more complete survey see Ereshefsky 1992). My case for pluralism turns on there
being more than one viable approach to species. Thus I have limited my survey to those
approaches that I take to be the most viable. The addition of further viable approaches
only strengthens my argument.

is Paterson's (1985) mate recognition concept. Paterson utilizes the interbreeding half of the biological species concept but drops any reference to reproductive isolation. According to Paterson, species are interbreeding groups whose members contain similar mate recognition systems, namely, behavioral and morphological characteristics that allow organisms to recognize conspecific mates (e.g., the chemical signals of wasps, the light signals of fireflies, even the stigmas of orchids). Despite their differences, both Mayr's and Paterson's species concepts capture the heart of the interbreeding approach: Species are groups of biparental organisms that share common fertilization systems. (For a Mayrian view of the difference between Mayr's and Paterson's concepts, see Mayr 1988.) Other proponents of the interbreeding approach to species include Dobzhansky (1970), Carson (1975), Ghiselin (1974), and Eldredge (1985).

According to proponents of the interbreeding approach, species are stable taxonomic units because the members of a species exchange genetic material through sexual reproduction. Ehrlich and Raven (1969), Van Valen (1976), and Andersson (1990), however, disagree. They argue that the stability of a species is primarily due to environmental forces rather than interbreeding. Thus these authors promote an ecological approach to species. For example, according to Van Valen, "A species is a lineage . . . which occupies an adaptive zone minimally different from that of any other lineage in its range and evolves separately from all lineages outside its range" (1976, 235). In other words, each species occupies its own distinctive adaptive zone, or niche, and the distinct set of selection forces in each zone is responsible for the maintenance of species as separate taxonomic units. It is worth emphasizing that according to the ecological approach, species must be lineages and not merely groups of organisms that occupy the same adaptive zone.

The interbreeding and the ecological approaches to species stem from work in evolutionary biology, whereas the third approach to species, the phylogenetic approach, flows out of biological systematics. According to Mishler and Donoghue (1982), Cracraft (1983), and Mishler and Brandon (1987), organisms should be classified according to propinquity of descent. In particular, each taxonomic group, whether it be a species, a genus, and so on, should contain all and only the descendants of a common ancestor. Such taxonomic groups are called "monophyletic taxa". The notion of monophyly, however, is not enough to provide a phylogenetic definition of the species category. Monophyletic taxa occur up and down the evolutionary continuum; according to the above authors, all taxa, species, genera, orders, even all life on this planet (assuming a common origin) form monophyletic taxa. Thus Cracraft, Mishler, Donoghue, and Brandon offer various ranking criteria for distinguishing which monophyletic taxa are species taxa. For example, according to

Mishler and Donoghue, "species ranking criteria could include group size, gap size, geological age, ecological and geographical criteria, degree of intersterility, tradition and possibly others" (1982, 499). In other words, some monophyletic taxa are ranked as species because their organisms interbreed or share common ecological and developmental factors. Other monophyletic taxa are ranked as species on the basis of morphological gaps between their organisms and those of other taxa. Cracraft, on the other hand, defines a species taxon as "the smallest diagnosable cluster of individual organisms within which there is a parental pattern of ancestry and descent" (1983, 170). According to Cracraft diagnosable clusters of organisms are, for the most part, "defined by uniquely derived characteristics" (ibid.). (Cracraft allows that some diagnosable clusters may be defined by "unique combinations of primitive and derived characteristics" [ibid.].) Though proponents of the phylogenetic approach offer different ranking criteria, they tend to agree that species are basal monophyletic taxa.

These three general approaches to species (the interbreeding, ecological, and phylogenetic) are diverse, but they all assume that species are lineages. By "lineage" I mean either a single descendent-ancestor sequence of organisms or a group of such sequences that share a common origin. In philosophical jargon, these approaches assume that species are spatiotemporally continuous or historical entities. (See section 4 below on why this assumption is essential for any post-Darwinian definition of the species category.) Beyond the assumption that species are lineages, the three approaches provide different pictures of species and the organic world. In particular, they provide incompatible taxonomies of the organic world. Consider a small scale example.

Suppose we want to determine the correct taxonomy of the insects that live on the side of a mountain. The insects consist of three populations A, B, and C (figure 1a). Each population forms a single basal monophyletic taxon (in other words, each is "the smallest diagnosable cluster of individual organisms within which there is a parental pattern of ancestry and descent" [Cracraft 1983, 170]). The organisms in B and C share a common ecological niche, while the organisms in A occupy their own niche. Concerning breeding behavior, the organisms in A and B can successfully interbreed and produce fertile offspring. But the organisms in C reproduce asexually; their females reproduce via parthenogenesis, thus their eggs do not require fertilization. So what is the correct taxonomy of the insects in this plot? According to the phylogenetic approach, it is a taxonomy consisting of three species: A, B, and C (figure 1b). According to the ecological approach, it is the taxonomy consisting of two species: A and $B + C$ (figure 1c). According to the interbreeding approach, it is the taxonomy consisting of a single species: $A + B$ (figure
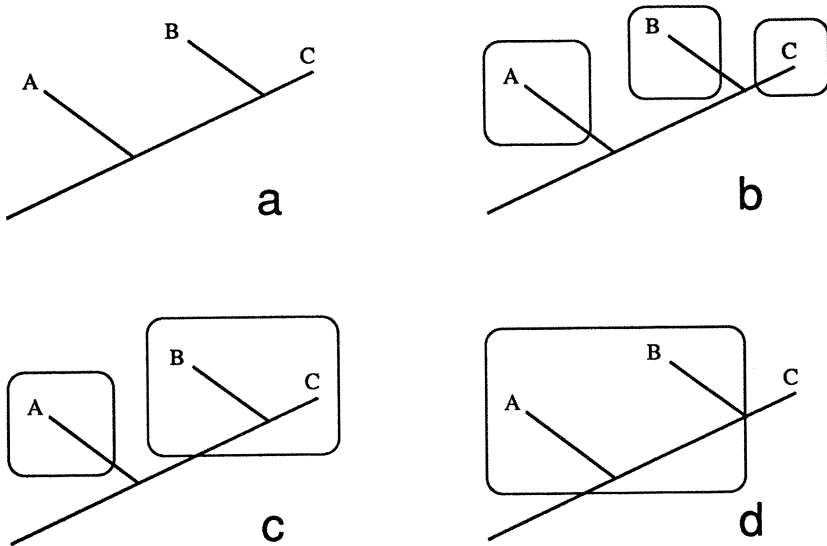
Figure 1.   A phylogenetic tree with three populations, A, B and C.
1a. The phylogenetic tree.
1b. The phylogenetic tree with three phylogenetic species, A, B and C.
1c. The phylogenetic tree with two ecological species, A and B + C.
1d. The phylogenetic tree with one interbreeding species, A + B.

1d). (Because the organisms in C reproduce asexually, that population forms no species according to the interbreeding approach.) Hence these general approaches to species provide three different taxonomies of the insects in the plot.[2]

Consider this problem on a much larger scale, namely, that of trying to provide a taxonomy of all the organisms on this planet. Because biologists disagree on the correct approach to the species category, they provide different taxonomies of the organic world. Moreover, these taxonomies are *incompatible* in that they often classify the same organisms into different lineages (see references in footnote 2). Such incompatibility occurs in two ways (and can be illustrated with figure 1). First, an organism may belong to two lineages where one lineage is properly contained in another; for example, a member of the phylogenetic species A

[2]Though this example is hypothetical, it is based on empirical studies showing that interbreeding, ecological, and monophyletic lineages often do not correspond in nature. For the discontinuity between interbreeding and ecological lineages, see Templeton (1989) and Grant (1981). For the discontinuity between interbreeding and monophyletic lineages, see de Quieroz and Donoghue (1988) and Frost and Hillis (1990, 96–97). For the discontinuity between ecological and monophyletic lineages, see Mayr (1982, 230) and Ridley (1986, 32ff.).

is also a member of the interbreeding species $A + B$. Second, an organism may belong to two lineages that are disjoint; for example, an organism in population B belongs to both the ecological species $B + C$ and the interbreeding species $A + B$. The type of pluralism I am highlighting here should be distinguished from a more moderate form suggested in the literature. According to Mishler and Donoghue (1982) and Mishler and Brandon (1987) (see section 4 below) there are a number of legitimate species approaches, but different approaches apply to different organisms and no more than one approach is applicable to an organism. The result is a unique taxonomy of the organic world. I am presenting a more radical picture of the organic world exists. Different species approaches often classify the same organisms into different lineages. Consequently there are a number of incompatible taxonomies of that world.

What does a monist make of all this? A monist would insist that only one correct approach to species exists and consequently only one correct taxonomy of the organic world exists. I disagree on both counts. In what follows I argue for a plurality of equally legitimate though incompatible taxonomies of the organic world.

First, I should point out that the argument for pluralism offered here is ontological, not epistemological. Species pluralism, according to current evolutionary theory, is a real feature of the world and not merely a feature of our lack of information about that world. Others (e.g., Cartwright 1983 and Levins 1968) provide epistemologically motivated arguments for pluralism. According to these arguments, the world is exceedingly complex and we have limited cognitive abilities, thus we posit a plurality of simplified and inaccurate models and taxonomies. These arguments nevertheless allow that there may be a single correct taxonomy of the world and perhaps in time we will acquire enough information to discover that taxonomy.

An ontological argument for species pluralism, however, can be found in contemporary evolutionary theory. Specifically, evolutionary theory provides the following picture of the organic world. All of the organisms on this planet belong to a single genealogical tree. The forces of evolution segment that tree into a number of different types of lineages, often causing the same organisms to belong to more than one type of lineage. The evolutionary forces at work here include interbreeding, selection, genetic homeostasis, common descent, and developmental canalization (see Templeton 1989 for a discussion of these forces). The resultant lineages include lineages that form interbreeding units, lineages that form ecological units, and lineages that form monophyletic taxa. (Interbreeding units are the result of interbreeding [Mayr 1970]; ecological units are the result of environmental selection [Van Valen 1976]; and basal monophyletic taxa owe their existence to common descent [de Queiroz and Donoghue

1988].) So the forces of evolution segment the tree of life into a plurality of incompatible taxonomies: one taxonomy consisting of interbreeding units, another consisting of ecological units, and a third consisting of monophyletic taxa. Of course this picture of evolution could be wrong; perhaps some of the above-mentioned forces do not exist, or those forces lack the ability to produce stable taxonomic entities. These are, after all, empirical matters. But given what current evolutionary theory tells us, the forces of evolution segment the tree of life into different and incompatible taxonomies. (Holsinger 1984 presents a similar picture of evolution.)

Proponents of monism may allow different types of basal lineages in the world, but they would contend that one type of lineage is more important for understanding the course of evolution, thus only that type of lineage should be designated by the term "species". For example, Eldredge (1985, 200–201) and Ghiselin (1989, 74–75) argue that lineages with sexual organisms are much more important in the course of evolution. As a result, they suggest that only interbreeding units should be called "species".

This suggestion and others like it should be rejected. If we are to understand how evolution has occurred on this planet, we must study the various types of theoretically important lineages in the world. No particular type of lineage is prior in that study. Consider Eldredge and Ghiselin's suggestion that sexual organisms are the most important in the course of evolution on this planet. As Eldredge (1985, 200–201) and Ghiselin (1989, 74) point out, the occurrence of recombination in sexual organisms provides sexual species with greater genetic flexibility than asexual species. Consequently, sexual species frequently outcompete asexual ones. I agree that the important differences between sexual and asexual species cannot be ignored. However, the competitive ability of sexual organisms (in certain circumstances) should not blind us to the fact that most organisms in the history of this planet are asexual.[3] Nor should it cause us to ignore the existence of stable taxonomic lineages consisting of asexual organisms (see the proceedings of a recent symposium on asexual organisms [Mishler and Budd 1990]). A taxonomy containing only interbreeding units provides an inadequate framework for studying life's diversity. The same applies to a taxonomy consisting of only monophyletic taxa or just ecological units. A proper systematic study of life requires each of these taxonomies. Consider the sorts of theoret-

[3]According to Hull (1988), "[F]or the first three-quarters of life on Earth, the primary, possibly sole form of reproduction was asexual" (p. 429). Furthermore, if one looks at Earth's present biota, sexuality "turns out to be rare on every measure suggested by evolutionary biologists—number of organisms, biomass, amount of energy transduced, and so on" (ibid.).

ically important information each taxonomy offers. A taxonomy of monophyletic taxa provides a framework for examining genealogy. A taxonomy of interbreeding units offers a framework for examining the effect of sex on evolution. And a taxonomy of ecological units provides a structure for observing the effect of environmental selection forces. A systematic study that considers just one of these taxonomies provides an overly coarse-grained picture of evolution.

Thus far I have argued that the tree of life consists of three types of basal lineages and that these lineages give rise to distinct taxonomies of the organic world. One might accept my representation of current evolutionary thinking, yet be hopeful that a fourth parameter common to all three types of base lineages will be discovered. Such a parameter would define a fourth type of base lineage to which the other types of lineages could be reduced, resulting in a single correct taxonomy of the organic world. I cannot foreclose the possibility of such an empirical discovery, but a closer look at current biological thinking offers reasons for doubting the existence of such a parameter.

Suppose, for example, one were to suggest that once biologists have performed enough genetic analyses (e.g., like the current human genome project), they will find that overall genetic similarity is a parameter common to interbreeding, ecological, and monophyletic lineages. This suggestion, however, is problematic. If different species concepts classify the same group of organisms such that one lineage is fully contained in another, then it is impossible that both lineages consist of organisms with the most overall genetic similarity. Recall the example illustrated in figure 1. A is a monophyletic taxon and $A + B$ forms an interbreeding unit. Consequently the organisms in A and $A + B$ cannot both have the most overall genetic similarity. For if the organisms in A have the most overall genetic similarity, then the organisms in $A + B$ must have less overall genetic similarity. Given that some basal lineages are contained in others, then not all basal lineages consist of organisms with the most overall genetic similarity. Nature also gets in the way in the attempt to align interbreeding and monophyletic lineages with lineages containing the most overall genetic similarity. In some situations, lineages with the most overall genetic similarity do not form monophyletic taxa (Frost and Hillis 1990, 96; Futuyma 1985, 311). In other situations, lineages with the most overall genetic similarity do not form interbreeding units (Futuyma 1985, 220; Mayr 1970, 321; and Frost and Hillis 1990, 95).[4]

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[4]Thus Mayr, a proponent of the interbreeding approach, writes, "Species difference [cannot] be expressed in terms of the genetic bits of information, the nucleotide pairs of the DNA. That would be quite as absurd as trying to express the differences between the Bible and Dante's *Divina Commedia* in terms of the difference in the frequency of the letters of the alphabet used in the two books. The meaningful level of integration is well above that of the base code of information, the nucleotide pairs" (1970, 321–322).

Alternatively, a reductionist might complain that I have addressed this problem from the wrong direction. I should be looking for some common genetic factor in the three types of lineages in question, rather than overall genetic similarity. For as Futuyma points out, "species owe their existence to specific characters governed by specific genes" (1985, 223). However, this approach does not provide a common basis to reduce the three types of lineages either. The genes that Futuyma (a proponent of the interbreeding approach) thinks define species are those that affect sexual behavior. Yet in some instances, an alteration in the genetic content of an organism can affect its ecological adaptiveness but not its sexual behavior. For example, being heterozygous rather than homozygous for hemoglobin S in a malaria zone affects one's ecological adaptiveness but not one's choice of mates (see Futuyma 1985, 75–76). Furthermore, in instances where genes controlling morphological distinctiveness and reproductive behavior are separable (Mayr 1970, 322), mutations affecting the former but not the latter can cause the existence of new monophyletic taxa that are not distinct interbreeding units. And, in instances where genetic material governing morphology and ecological behavior are separable (Futuyma 1985, 238), mutations affecting the former but not the latter can cause the existence of new monophyletic taxa that are not distinct ecological units. The upshot is that the genetic factors governing the distinctive features of interbreeding, ecological, and monophyletic lineages are separable. Thus the reduction of these types of lineages to their underlying genetic bases results in three separate genetic taxonomies. In other words, the plurality of types of lineages at the macroscopic level is just transferred to a plurality of types of genetic factors at the microscopic level. The attempt to find a common genetic factor that unifies the three types of lineages in question fails.

The results of this section can be summarized as follows. The forces of evolution produce at least three different types of basal lineages (interbreeding, ecological, and monophyletic) that cross classify the organic world. Each of these lineages is equally important in the evolution of life on this planet. Moreover, according to current biological thinking, there is no fourth parameter to which these types of lineages can be reduced. Consequently, the tree of life on this planet is segmented into a plurality of incompatible but equally legitimate taxonomies.

3. Criticisms of Pluralism. Some philosophers (e.g., Hull 1987, 1988, 1989; Sober 1984) and biologists (e.g., Hennig 1966; Ghiselin 1969, 1987; Cracraft 1983, 1987) take a dim view of species pluralism. In this section I consider and answer their objections to pluralism. In doing so, I further explicate eliminative pluralism and I lay a general foundation for a pluralistic approach to taxonomy.

3.1. The Communication Objection. Systematists often point out that "the necessity of classifications has long been recognized . . . for the very communication of general ideas" (Ghiselin 1969, 79; also see Mayr 1969, 89; Eldredge and Cracraft 1980, 165ff.). With this presumption of the goal of systematics, Ghiselin provides the following argument against pluralism:

> Whatever standard one does take for ranking taxonomic groups, it should be clear that systematists work at cross purposes when they do not agree on any such criteria. If a common standard were recognized, the system would be more informative by far, and the goal of natural classification would be better served. (1969, 85)

Similarly, Hull writes that "terming a hodgepodge of different units 'species' serves no useful purpose . . . . If pluralism entails confusion and ambiguity, I am forced to join with Fodor's . . . Granny in her crusade to stamp out creeping pluralism" (1987, 181; also see Hull 1989, 313). This objection to pluralism can be codified in the following argument. Species pluralism entails that the term "species" is ambiguous. If the term "species" is ambiguous, then confusion will set in when biologists discuss the nature of species, for biologists will mean different things by "species". Such confusion should be avoided. Thus species pluralism should be avoided.

I agree with Kitcher's (1984b, 326–327) response to this argument: Species pluralism does not "unlock the doors of Babel" and plunge biological discourse into confusion. Different species concepts often pick out different taxa in a single biological situation. To guard against confusion, biologists merely need to be explicit concerning the concept being used when referring to a group of taxa as "species". Indeed, in professional journals biologists usually are explicit concerning what they mean by "species".

I would like to offer a stronger response to the communication objection. Instead of referring to basal lineages as "species", biologists should categorize those lineages by the criteria used to segment them: interbreeding units, monophyletic units, and ecological units. The term "species" is superfluous beyond the reference to a segmentation criterion; and when the term is used alone it leads to confusion. The term "species" has outlived its usefulness and should be replaced by terms that more accurately describe the different types of lineages that biologists refer to as "species". For example, Grant (1981) suggests using the terms "biospecies" and "ecospecies" for the lineages picked out by the interbreeding and ecological approaches. Add to these the term "phylospecies" for the lineages highlighted by the phylogenetic approach. Replacing "species" with such terms better serves the goal of communication placed on system-

atics. Accordingly, I call the form of species pluralism advocated here "eliminative pluralism": Eliminate the term "species" and replace it with a plurality of more accurate terms.

   3.2. The Inconsistency Objection. Some authors (e.g., Hennig 1966, Hull 1987, and Cracraft 1983, 1987) are unhappy with the pluralistic result that there are incompatible taxonomies of the organic world. Hennig, for example, writes that "if systematics is to be a science it must bow to the self-evident requirement that objects to which the same label is given must be comparable in some way" (1966, 154). In a similar vein, Hull objects to Kitcher's version of species pluralism because it does not provide "a consistent treatment of the evolutionary process" (1987, 180). However, I would counter that the existence of incompatible taxonomies of the organic world does not provide an inconsistent view of the evolution. Eliminative pluralism assumes that there is one genealogical tree of life, but that tree is segmented by different evolutionary forces into different lineages (often with the same organisms belonging to more than one type of lineage). As a result, the tree of life is segmented into different taxonomies: one taxonomy consisting of interbreeding units, another consisting of ecological units, and a third consisting of monophyletic taxa. The resultant taxonomies are taxonomies of *different aspects* of the tree of life. Hence they are not inconsistent with one another. Moreover, each of these taxonomies is internally consistent: One taxonomy consists only of interbreeding units, another consists only of basal monophyletic taxa, and so on. So eliminative pluralism provides a fully consistent treatment of evolution.

   3.3. The No Criteria Objection. Ghiselin (1987, 135–136) and Hull (1987, 180; 1989, 313; personal communication) believe that pluralism is an overly liberal approach to science. In particular, they contend that pluralists provide no criteria for discerning legitimate from illegitimate taxonomies. What, for example, discriminates between taxonomies based on current evolutionary theory from those based on idealistic morphology or even creationism? As Hull sees it, pluralism places no checks on science. In rougher terms, Ghiselin views pluralism as an instance of lazy thinking that results in an attitude of "anything goes" (see Ghiselin 1987, 135–136). To answer this objection, I offer candidate criteria that a pluralist can use for determining whether a taxonomy is legitimate. These criteria are similar to those standardly used in determining whether a theory is scientific (see, e.g., Laudan 1984). Before I present the criteria, I need to introduce some terminology and state explicitly some common presumptions of scientific classification.

   A taxonomy is produced by what I call a *taxonomic approach*. Such

an approach constructs a taxonomy by a set of principles. Those prin-
ciples come in two forms. *Sorting principles* sort the constituents of a
theory into basic units. *Motivating principles* justify the use of sorting
principles. Consider the biological species concept. It constitutes a tax-
onomic approach for producing a taxonomy of the organic world. Its sort-
ing principles roughly assert: Sort organisms that can interbreed and pro-
duce fertile offspring into a single species, sort organisms that reproduce
sexually but cannot interbreed into different species, and sort organisms
that reproduce asexually into no species. The motivating principle of the
biological species concept assumes that the process of interbreeding causes
stability within lineages of organisms that interbreed. (By "stability" I
mean that the organisms of lineage evolve as a unit or share a common
stasis.) In brief, the motivating principle of the biological species concept
sets out the causal factor responsible for the existence of the lineages in
question.

The general idea behind motivating principles can be described as fol-
lows. A taxonomy (biological or otherwise) consists of entities that are
the nodes of causal processes. Those entities are either the result of a
common type of causal process, or they are objects that have a similar
causally efficacious property. So motivating principles either cite the causal
processes that give rise to lineages or the similar causally efficacious na-
ture of those lineages. The three taxonomic approaches considered here
(the interbreeding, ecological, and phylogenetic) contain motivating prin-
ciples that cite the causal process primarily responsible for the type of
lineages in question. The interbreeding approach cites the process of in-
terbreeding, the ecological approach highlights environmental selection
pressures, and the phylogenetic approach focuses on the process of de-
scent from common ancestry.

Notice that this conception of motivating principles does not violate the
common observation that no universal generalizations exist whose pred-
icates are the names of species taxa (see, for example, Hull 1976, 1978).
Nothing in the conception of motivating principles forces a taxonomic
approach to assign some qualitative property to all the members of a spe-
cies taxon. However, the notion of motivating principles does suggest
that there may be universal generalizations whose predicates are the names
of *types* of basal taxonomic units. That is, there may be universal gen-
eralizations containing such predicates as "biospecies", "phylospecies",
or "ecospecies". So though no laws exist about particular species taxa,
there may very well be laws about types of species taxa.

I now turn to the criteria that a taxonomic approach must satisfy to be
considered legitimate. Ideally, such a list of criteria would provide in-
dividually necessary and jointly sufficient criteria. The following list,

however, is merely a first stab; further criteria may be needed to properly complete the list.

First, the motivating principles of a taxonomic approach should be empirically testable. Put simply, such principles should have an empirical basis. For example, in determining the legitimacy of the interbreeding approach, biologists should be able to determine empirically if interbreeding is an important causal factor in the stability of lineages of organisms that interbreed.

Second, the sorting principles of a taxonomic approach should produce a single internally consistent taxonomy. In other words, a taxonomic approach should be unambiguous. Ambiguity can occur in two ways: The base units of a taxonomic approach can be ambiguous; and a taxonomic approach can produce an ambiguous classification, that is, it can produce more than one taxonomy with no way of discriminating which is the correct one (see Ridley 1986, 6–7). Ambiguity of the first type can give rise to ambiguity of the second type: If a taxonomic approach allows a heterogeneous class of base units, then that approach will produce inconsistent taxonomies. The criterion of internal consistency is designed to avoid both kinds of ambiguity. All taxa designated as "species" (or more precisely, "phylospecies", "biospecies", and so on) *within* a taxonomic approach should be comparable along the appropriate parameters. If that requirement is met, then a taxonomic approach provides a single consistent taxonomy of the organic world.

Third, the motivating principles of a taxonomic approach should be consistent with well-established hypotheses in other scientific disciplines. For example, a taxonomic approach in biology should not violate any well-established laws in biochemistry or geology. This criterion opens a nest of standard problems in the philosophy of science. The question of what is a well-established hypothesis is none other than the problem of confirmation. The question of what constitutes a scientific discipline brings up the demarcation problem of discerning science from nonscience. I mention these problems in passing to indicate that questions concerning the legitimacy of a taxonomic approach are intricately tied to central questions in the philosophy of science.

Fourth, the motivating principles of a taxonomic approach should be consistent with and derivable from the tenets of the theory for which the taxonomy is produced. In particular, a taxonomic approach in biological systematics should be derivable from well-established tenets in evolutionary theory. For example, in the case of the interbreeding approach, the motivating principle that interbreeding can cause stability in lineages should be an extension of what evolutionary theory tells us about the stability of lineages in general.

Though this list of criteria may be incomplete as it stands, it never-

theless does a good job at ruling out paradigm illegitimate taxonomic approaches. Consider three such approaches. A creationist taxonomic approach contains motivating principles that are neither empirical nor consistent with the tenets of evolutionary theory or well-established tenets in other disciplines (e.g., carbon dating in geology). Thus a creationist approach is illegitimate because it violates criteria 1, 2, and 3. Taxonomic approaches based on idealistic morphology (for example those advocated by Goethe and Richard Owen; see Mayr 1982, 457–458) rely on typological thinking. Typological thinking, however, is incompatible with current evolutionary biology and has been replaced with population thinking (see Sober 1980). Consequently, approaches based on idealistic morphology are illegitimate because they violate criterion 4. Phenetic taxonomic approaches (e.g., Sneath and Sokal 1973) produce a number of inconsistent taxonomies of the organic world (see Hull 1970 and Ridley 1986). Hence such approaches are illegitimate because they violate criterion 2.

On the other hand, I contend that the three approaches to species discussed in this paper—the interbreeding, ecological, and phylogenetic—do satisfy the four criteria for legitimate taxonomic approaches. Of course proponents of a particular taxonomic approach argue that the other approaches are defective (see, for example, Cracraft 1983, Ghiselin 1987, and Ridley 1990). For the most part, these arguments are based on the false premise that a single taxonomic approach is supposed to provide a universal definition for all basal lineages. (That premise was cast in serious doubt by the arguments of section 2 above.)[5] I would like to make one final point concerning the above criteria. By adopting such criteria, a worker is not committed in an a priori fashion to the existence of a number of legitimate taxonomies, for it is possible that only one taxonomic approach satisfies the above criteria. The empirical world ultimately decides whether pluralism within a particular discipline is appropriate.

In this section, I have completed two tasks. First, I have answered several objections to taxonomic pluralism. Second and more importantly, I have further explicated eliminative pluralism and laid a general foundation for a taxonomic pluralism.

4. Species Pluralism and Species of Pluralism. As mentioned earlier, the idea of species pluralism is not new. A number of philosophers and biologists (Ruse 1969, 1987; Dupré 1981; Mishler and Donoghue 1982;

[5]Some proponents of the phylogenetic approach (e.g., Ridley 1986, 1990) argue that the ecological and interbreeding approaches are ambiguous and thus violate criterion 2. Perhaps some versions of those approaches are ambiguous, but some are not. Templeton's (1989) notions of genetic and demographic exchangeability, for example, provide unambiguous definitions of basal interbreeding and ecological units.

Kitcher 1984a, 1984b, 1987; Mishler and Brandon 1987) have already advocated species pluralism. In this section, I sketch some problems facing earlier forms of species pluralism. I end the section by indicating how eliminative pluralism avoids those problems.

Ruse (1969, 1987) offers the most conservative form of species pluralism. Ruse acknowledges that different species concepts provide different criteria for sorting organisms. But Ruse claims, "There are different ways of breaking organisms into groups, and they *coincide!* The genetic species is the morphological species is the reproductively isolated species is the group with common ancestors" (1987, 238). In other words, Ruse believes that the various species concepts offered by biologists pick out the same set of taxa. Ruse's motivation for establishing a coincidence among species concepts is his belief that such a coincidence would indicate the naturalness (or reality) of species taxa. Following Whewell and Hempel (see Ruse 1987 for references), Ruse takes consilience to be a mark of reality, that is, an indication that a classification is natural rather than artificial. So according to Ruse, if various species concepts citing different biological properties pick out the same taxa, we have good reason to believe that those taxa are real.

The consilience of various species concepts is the ideal that some evolutionists hoped for (e.g., Mayr 1969, 28). However, as illustrated in this paper, nature has stymied that ideal. Groups of organisms that have the most overall genetic similarity often are not groups of interbreeding organisms (Futuyma 1985, 220; Mayr 1970, 321; and Frost and Hillis 1990, 95). Many monophyletic taxa are not interbreeding units (Mishler and Donoghue 1982 and Frost and Hillis 1990). Many groups of organisms that form ecological units are not interbreeding units (Templeton 1989 and Grant 1981). This lack of consilience is not limited to a few borderline cases. Consider the case of asexual organisms. Most organisms in the history of this planet are asexual (see footnote 3). As a result, a major discrepancy divides species concepts that recognize both sexual and asexual taxa (e.g., ecological and phylogenetic concepts) from concepts that recognize only sexual taxa (e.g., interbreeding concepts). Given this lack of consilience, a form of species pluralism that requires consilience should be rejected.

One other item from Ruse's version of pluralism is worth mentioning. Ruse assumes that the alleged coincidence of species concepts indicates the naturalness of species taxa. However, a major point of this paper is that the naturalness of some objects does not lie at the intersection of various scientific concepts. In particular, the lack of consilience among various species concepts does not show that the taxa they pick out are not real. The taxa are real; they just do not fall under a single category (the species category).

Mishler and Donoghue (1982) and Mishler and Brandon (1987) offer a more liberal form of species pluralism. Unlike Ruse, they recognize that different species concepts often pick out different groups of organisms. Recall their phylogenetic species concept which requires that all species taxa form monophyletic taxa. But Mishler and Donoghue recognize that "because different factors may be 'most important' in the evolution of different groups, a universal criterion for delimiting fundamental, cohesive evolutionary units does not exist" (1982, 495). Thus, some species taxa owe their existence to reproductive factors, other species taxa are the result of ecological forces, still others are due to homeostatic inertia. Their phylogenetic species concept is monistic in that all species taxa are monophyletic, but it is pluralistic in that different types of processes cause lineages to be species. The result is a single taxonomy of the organic world consisting of different types of basal monophyletic lineages.

Mishler, Donoghue, and Brandon's form of pluralism presents two problems. First, it requires that all species taxa form monophyletic taxa. Consequently, any taxa that are not monophyletic, despite their forming good interbreeding or ecological units, should not be formally recognized. As mentioned previously, however, instances of nonmonophyletic basal taxa that form interbreeding or ecological units abound (see, for example, Tajima 1983, Neigel and Avise 1986, de Queiroz and Donoghue 1988, and Frost and Hillis 1990). Mishler, Donoghue, and Brandon's form of pluralism is inadequate because it ignores nonmonophyletic basal taxa that satisfy classic population genetic parameters for specieshood (namely, gene flow and exposure to common selection regimes).

The second problem with their form of species pluralism is its commitment to a *single* taxonomy of the organic world. As noted in section 2, different species approaches often cross classify the same group of organisms. As a result, different species approaches produce incompatible taxonomies of the organic world. This incompatibility is not limited to the discrepancy between a taxonomy containing only monophyletic taxa (as in the case of Mishler, Donoghue, and Brandon) and a taxonomy containing both monophyletic and nonmonophyletic taxa. Even within a strictly monophyletic taxonomy, there are monophyletic interbreeding and ecological units that are not coextensive (see Templeton 1989). Because Mishler, Donoghue, and Brandon's pluralism does not allow for the existence of incompatible but empirically significant taxonomies, their pluralism does not go far enough.

Another form of species pluralism is found in Dupré (1981). Dupré describes his version of pluralism as "promiscuous realism", "The realism derives from the fact that there are many sameness relations that serve to distinguish classes of organisms in ways that are relevant to various

concerns; the promiscuity derives from the fact that none of these rela-
tions is privileged" (1981, 82). What are some of those sameness rela-
tions? One sameness relation is the phenetic measurement of overall sim-
ilarity (ibid., 82–83; also 89–90). Other sameness relations consist of
more limited ranges of properties, for example, the properties of "texture
or flavor" that gourmets use to classify organisms (ibid., 83). Then there
are the familiar properties of interbreeding behavior (ibid., 85–87) and
phylogenetic relations (ibid., 87–89). Dupré's form of pluralism is cer-
tainly more liberal than the forms advocated by Mishler, Donoghue and
Brandon, and Ruse, but Dupré's pluralism is too promiscuous. Taxon-
omies based on cooking lore are taken on a par with those based on con-
temporary evolutionary biology. Dupré's pluralism is just the sort of plu-
ralism that Ghiselin and Hull worry about in their "no criterion objection"
(see section 3): It legitimizes taxonomies that are in no way based on
scientific reasoning. Dupré's pluralism needs to be supplemented with
criteria for judging the adequacy of sameness relations, otherwise it con-
dones any taxonomic approach.

Recently Philip Kitcher has become a prominent advocate of species
pluralism (see 1984a, 1984b, 1987, and 1989). Kitcher organizes the spe-
cies concepts he accepts as legitimate into two types: historical and struc-
tural (1984b, 321ff.). Historical species concepts require that species are
genealogical entities. The interbreeding, ecological, and phylogenetic ap-
proaches are historical concepts: Each requires that species taxa form his-
torically (spatiotemporally) continuous entities. Structural species con-
cepts, on the other hand, do not require that species taxa form historically
continuous entities. Instead, structural concepts require that the organisms
of a species have important functional similarities; Kitcher suggests ge-
netic, chromosomal, or developmental similarities.

Those familiar with the biological literature may wonder why Kitcher
accepts structural species concepts as legitimate, for all currently pro-
posed species concepts fall under the historical heading—they all require
that species form historically continuous entities (even Kitcher's 1984b,
325, own taxonomy of species concepts illustrates this). Be that as it may,
Kitcher wants to stress that biological practice could, and should, allow
the legitimacy of historical *and* nonhistorical (structural) species con-
cepts. To make the legitimacy of nonhistorical species concepts intuitive,
Kitcher cites a hypothetical case of lizard lineages (1984b, 314–315). The
lineages are spatiotemporally disconnected from one another, neverthe-
less their organisms are very similar along morphological, behavioral,
ecological, and genetic parameters. Kitcher writes that "to hypothesize
'sibling species' in this case (and in like cases) seems to me not only to
multiply species beyond necessity but also to obfuscate all the biological
similarities that matter" (ibid., 315). Thus Kitcher suggests that we allow

the existence of spatiotemporally disconnected species taxa and accept
the legitimacy of nonhistorical (structural) species concepts. (Ruse 1987,
235–236, provides a similar argument.)

Kitcher's argument for the legitimacy of nonhistorical species concepts
overlooks the theoretical reason that biologists reject such concepts. Hull
(1976, 1978, 1987) presents this reason in the following argument (also
see Sober 1984). Since the inception of evolutionary theory, species taxa
have been considered evolutionary units, that is, groups of organisms
capable of evolving. The evolution of such groups requires that the or-
ganisms of a species taxon be connected by heredity relations. Heredity
relations, whether they be genetic or not, require that the generations of
a taxon be historically connected, otherwise information will not be trans-
mitted. The upshot is that if species taxa, or any taxa, are to evolve, they
must form historically connected entities. By allowing nonhistorical spe-
cies concepts, Kitcher's pluralism falls outside the domain of evolution-
ary biology and should be rejected.[6]

In summary, Dupré's and Kitcher's forms of pluralism are too liberal,
while the forms advocated by Mishler, Donoghue and Brandon, and Ruse
are not liberal enough. Eliminative pluralism charts a middle course be-
tween these forms of pluralism. It acknowledges that the forces of evo-
lution create different types of basal taxa. It also recognizes that these
different types of taxa give rise to taxonomies that cross classify the or-
ganic world. Eliminative pluralism, however, is prudent enough to place
constraints on pluralism: Only taxonomic approaches that satisfy the cri-
teria suggested in section 3 are allowed into the store of legitimate tax-
onomic approaches. Moreover, eliminative pluralism avoids ambiguity by
designating different types of taxa with different terms, and it preserves
consistency by requiring that taxonomic approaches be internally consis-
tent. Some may view eliminative pluralism as just a complicated form of
monism. If that is the case, then the arguments of this paper have been
successful.

  [6]Kitcher (1989) has recently offered a further argument for accepting nonhistorical spe-
cies concepts. As Kitcher points out, asserting that species are historical entities does not
sufficiently specify the nature of species, for each organism, population, and all of life on
this planet is an historical entity. To understand fully the nature of species, we need suf-
ficient conditions that distinguish species from other historical entities. Kitcher then shows
that the conditions for segmenting the tree of life into species are vague and problematic.
As a result, Kitcher concludes that we should allow the legitimacy of nonhistorical species
concepts (1989, 204). But it is important to note that our lack of fully adequate conditions
for segmenting the tree of life into species in no way nullifies the requirement that species
must be historical entities. (In more general terms, a condition's insufficiency does not
imply that it is unnecessary.) Species, whether they be basal interbreeding, ecological or
phylogenetic taxa, are historical entities. We are just uncertain on how to draw the bound-
aries of such taxa. Indeed, such boundaries may be naturally vague (see Ereshefsky 1991).

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