A Radical Solution to the Species Problem

Michael T. Ghiselin

Systematic Zoology, Vol. 23, No. 4. (Dec., 1974), pp. 536-544.

Stable URL:
http://links.jstor.org/sici?sici=0039-7989%28197412%2923%3A4%3C536%3AARSTTS%3E2.0.CO%3B2-B

Systematic Zoology is currently published by Society of Systematic Biologists.

A RADICAL SOLUTION TO THE SPECIES PROBLEM

MICHAEL T. GHISELIN

Abstract

Ghiselin, M. T. (Department of Zoology and Bodega Marine Laboratory, University of California, Berkeley, P. O. Box 247, Bodega Bay, California 94923). 1975. A radical solution to the species problem. Syst. Zool. 23:536–544.—Traditionally, species (like other taxa) have been treated as classes (universals). In fact they may be considered individuals (particular things). The logical term "individual" has been confused with a biological synonym for "organism." If species are individuals, then: 1) their names are proper, 2) there cannot be instances of them, 3) they do not have defining properties (intensions), 4) their constituent organisms are parts, not members. "Species" may be defined as *the most extensive units in the natural economy such that reproductive competition occurs among their parts.* Species are to evolutionary theory as firms are to economic theory: this analogy resolves many issues, such as the problems of "reality" and the ontological status of nomenclatorial types. [Biological species; systematics; philosophy.]

It would appear that the philosophy of taxonomy is about to undergo a major upheaval. Symptomatic is its Gordian knot, the species problem. Some years ago (Ghiselin, 1966a) I attempted to cut it with the sword, casually remarking that, in the logical sense, species are individuals, not classes. So entrenched had the habit become of treating species as universals rather than as particular things, and their names as general rather than proper, that it took some time before this pronouncement was taken seriously. The position in question is hardly new. It goes back at least to Buffon, but I believe this was the first explicit use of it against what has been misleadingly called the nominalistic species concept. Even Ruse (1973), a philosopher who cites the foregoing work, passes the notion over in silence. On the other hand Hull (1974) has lately endorsed the idea that, from the point of view of evolutionary theory, biological species and monophyletic taxa are individuals. And Mayr (1969a), while not going so far, strongly emphasizes the point that species are more than just nominal classes. It would appear that the subject deserves further analysis and comment.

PHILOSOPHY

There are both advantages and disadvantages to treating species as individuals. This paper, however, only aims to show that such a position has attractive qualities from the point of view of logic and biology alike, and that it is perhaps not so radical as one might think. The basic point (Ghiselin, 1969, 1974) is that multiplicity does not suffice to render an entity a mere class. In logic, "individual" is not a synonym for "organism." Rather, it means a particular thing. It can designate systems at various levels of integration. A human being is an individual in spite of being made up of atoms, molecules and cells. An individual, furthermore, need not be physically continuous. The United States of America is an individual in the class of national states, and this is true in spite of the interposition of Canadian territory and international waters between Alaska and the rest. (For a philosophical discussion of individuals see the book on that subject by Strawson, 1959.)

It is characteristic of individuals that there cannot be instances of them. Thus, the class of national states has instances: the United States of America is a national state. We would not, however, say, "California is a United States of America." This is true in spite of the fact that we may treat any number of parts of the Union as a class, as when we say that these "states" are sovereign. California is a part of the United States of America.

Equally significant is that individuals do not have "intensions." That is to say, there are no properties necessary and sufficient to define their names. Of course, people, national states and species do have properties, but these are not implied when we use the name. Much of the species problem has been the result of equivocal uses of species names as universal and proper. Thus, some biologists will never say "John Smith is a *Homo sapiens*," but insist that correct usage demands "John Smith is a specimen of *Homo sapiens*." In other words, some consider the taxonomic term to designate a class of people ("all men") others as a single thing ("humanity" or "the human race").

Some confusion, and indeed some continuing uncertainty upon the part of logicians, results from the fact that proper names can imply or suggest certain properties. It does not seem to me that this means that proper names really do have intensions, but perhaps some would disagree. A descriptive epithet facilitates communication and aids the memory. Consider, for example, *Phallus impudicus* Linnaeus, 1753. But let us not confound poetry with logic. Whether a name be apt or a misnomer is not the same question as what properties a thing must of logical necessity possess if the name is to be used.

As is well known, much confusion is avoided in dealing with such matters when one clearly distinguishes between categories and taxa. Taxonomic categories, in the sense of all taxa of a given rank, are like "national state": everybody agrees that these entities are classes. The point at issue is whether taxa are likewise classes, or whether, like California, they are individuals. It may be that some taxa (e.g. Mammalia) are classes, whereas particular species are individuals. In traditional analyses of the species problem, which presuppose that all taxa are classes, there has been some confusion over whether the names of categories should be defined so as to specify the defining properties of taxa. In the light of the present analysis, one would say that as particular species cannot have defining properties, the question is closed.

BIOLOGY

The most significant question for biologists is "Individual whats?" One answer is individual populations—in the sense of genetical populations (syngamea). These are breeding communities—composite wholes—not to be confused with classes of organisms sharing certain intrinsic genetical properties. Of course one has to add something, namely that they are reproductively isolated, to differentiate "species" from "subspecies" and other populational categories. This, the biological species definition, is adequate, but it is still affected by certain problems of interpretation and implication which suggest that a better way might be found to define this category. Some definitions, albeit technically sound, could use some improvement. "Gold," for example, could be unequivocally defined as the element with an atomic number of 79, but for most of us, this is not very informative.

One solution is to try to define the species and other categories in terms of the causes of evolution. This is one of the main reasons why the usual formulation of the biological species definition is so attractive. Gene flow and reproductive isolation obviously profoundly influence the properties of organisms. But might not something else be more important? One possibility is competition, including natural and sexual selection. Elsewhere (Ghiselin, 1974) I have argued at some length, giving historical documentation, that biologists have misinterpreted the notion of competition and failed to appreciate its full significance. Ecological data have, to be sure, entered into the thinking of systematists, but nobody seems to have considered the possibility of defining categories in terms of modes of competition. Let us consider one possibility.

One can hardly deny that different kinds of competition occur between species and within them. Interspecifically we have the

struggle for the means of existence only. Intraspecifically there occurs a competition with respect to genetical resources as well, and even the resources contended for by organisms irrespective of species in the final analysis are directed toward the intraspecific struggle for reproductive success. Species, then, are *the most extensive units in the natural economy such that reproductive competition occurs among their parts.* It would be circular, if perhaps not viciously so, to say that species and lower taxa are those entities within which intraspecific competition occurs. Some might object to the use of "reproductive" in the foregoing definition, but there seems to be no better alternative. By "reproductive competition" is meant a competition with respect to genetical resources as such, not just any competition involving reproduction.

Species are to evolutionary theory as firms are to economic theory. And organisms are productive units in both, competing in different ways within their larger units and with the parts of comparable units. Likewise some organisms exist autonomously—as asexual creatures and self-employed craftsmen. This analogy has its limitations, of course, not the least being that firms can reasonably be said to be adapted, while species cannot. Nonetheless the analogy can be shown to help resolve the paradoxes and conceptual difficulties that have so long plagued the subject.

Species are individuals, and they are real. They are as real as American Motors, Chrysler, Ford and General Motors. If it be true that only individuals compete, then species as well as organisms can compete, just as corporations and craftsmen can. But clearly there exists a profound difference between two species competing, and competition between organisms belonging to different species.

Burma (1949) claimed that because species differ from moment to moment in geological history, they are, therefore, not the same, and hence that they are mere concepts without real existence in nature. This may be dismissed as merely a version of a paradox invented by Heraclitus (fragment LXXXI in the Loeb *Classical Library* edition): one cannot step in the same river twice. Who but a madman or a philosopher would maintain that he himself does not exist because he changes? Are we all figments of our own imaginations?

The rather forced dimensional-nondimensional dichotomy emphasized by Mayr (1963) is now seen to be irrelevant. Species need not be defined solely in relation to other species. "Species" is not a relational concept like "brother," but something comparable to "man." A firm is a firm because it forms a closed system of a given kind. It can compete with craftsmen and firms outside itself, and is characterized by a particular kind of internal organization. An only child cannot be a brother, but an economy might contain but a single firm.

The difficulties surrounding "potential interbreeding" become a dead issue too. If a species is an individual it hardly matters whether it is interbreeding at any given instant. All the "members" of a species are competing reproductively with all the others, irrespective of the distance between them or the existence of temporary spatial discontinuities between their component populations. Intraspecific competition is more perfect between organisms in close proximity, but the whole gene pool is nonetheless at issue. One should realize that all organisms, everywhere, like all the productive units in the world-wide political economy, are able to compete with one another to some degree. All can do that which diverts resources from the rest to themselves. They do so more or less intensely, or directly, or perfectly, and in diverse modes, but compete they do, and they can compete at a distance.

Some theorists have expressed the view that the existence of "different kinds" of species creates a problem for the biological species definition. Some have felt that there should be special categories (or kinds of categories) for designating these. The analogy with economics is particularly use-

ful here. Differences in the range of divergence within species—as when some are monotypic, others polytypic—create no problem. After all, some corporations are like Mitsubishi, others like the local fish market, but both can be corporations. It has been most unfortunate in this context that niche theory has been so intimately bound up with the notion of species as classes, so that niches are conceived of as "abstractly inherited" attributes of sets of like organisms. Hence a typological conception of the niche could scarcely be avoided. It becomes easier to reason about such matters when one realizes that a craftsman or other organism makes his living in a sense distinct from that of operating within a sector or range of sectors of an economy. The economic activity of both can vary from time to time and place to place.

No paradox arises when a single factory constitutes the entirety of a firm. Likewise, some biologists wish to erect "agamospecies" for species-less organisms. It seems rather like creating imaginary firms for the self-employed. And no economist would ever erect "chronofirms" for the same firm at different times, although the name of a firm might be changed when it becomes reorganized.

Consider, too, the objection of Ehrlich and Raven (1969) to the biological species concept on the grounds that it overemphasizes integration and gene flow within species. Whether this be a valid criticism or not, it is circumvented by shifting the emphasis from integration and gene flow to the mode of competition. Firms are firms irrespectively of how tightly organized and closely integrated they may be, and they remain basic units of organization in spite of the fact that local productive units are important too. That limited exchange of genes may occur between species creates no more difficulty for the species concept than does the fact that someone might work for two firms create difficulties for the notion of a firm. Gradual development of reproductive barriers creates intermediate conditions, but closed economic systems exist side by side with various stages in the formation and dissolution of trusts.

The morphological, genetical and physiological species concepts suffer from a common fault. They treat composite wholes as if they were classes defined in terms of the intrinsic properties of their members. If one were to define "craftsman" in terms of the properties of cells in certain organisms, it might seem odd. Likewise, we are not interested in the properties of employees when we define "firm," but rather in the properties common to firms generally. The attributes of organisms are not defining of the names of social groups, in spite of the fact that social groups must have constituent organisms. That John Doe has a particular set of genes is about as relevant to his being a specimen of *Homo sapiens* L. as it is to his working for the manufacturers of Brand X. Hence I cannot agree with Mayr (1969a:29) that the species "receives its reality from the historically evolved, shared information content of its gene pool." The species and the firm are "real" in the sense that they designate entities which exist: particular species and firms. And organisms are no more vessels for genes than they are vessels for industrial skills. Geneticists on the one hand, and employers on the other, may view them in such light; but the disadvantages to doing so should be obvious.

One problem with the "phenetic species concept" (see especially Sokal and Crovello, 1970; Sneath and Sokal, 1973) is that it treats species as classes defined in terms of the traits of organisms, rather than as individuals having the properties necessary and sufficient for membership in the species category. The pheneticists' position is that we should put things together according to their degree of resemblance. Yet, from the point of view of evolutionary theory, it is the causal nexus that matters. Of course, erecting classes on the basis of shared traits has its place, but this is not the issue. Some taxonomists may not care whether two organisms belong to different (cryptic) species, but it certainly matters to them.

All banks are not the same when we are depositing money, no matter how much they look alike to us.

The realization that species are individuals helps us to understand some of the important daily activities of taxonomists. How, for example, do they define the names of species? If species were universals, then their names would have defining characters. The problem of there being much diversity within certain species has been solved in a rather makeshift fashion by considering them to be "disjunctively" or "polythetically" defined—to view them as "clusters" of similar objects, but without any one combination of properties being necessary and sufficient to qualify an entity for membership. But according to the view being examined here, species names are proper names—like American Motors. It is not only difficult, but logically impossible, to list the attributes necessary and sufficient to define their names. None such exist, and the only way to define these names is by an ostensive definition. That is to say, by "pointing" to the entity which bears the name. Much confusion about the role of nomenclatorial types has arisen because they are used in a peculiar kind of ostensive definition. Attention is drawn to only a part or aspect of the individual the name of which is being defined. This entity, the type specimen, is misinterpreted as an example of a class, which it is not. Likewise, the diagnosis, which in fact only "purports to differentiate the taxon," is often confused with a definition, which actually would differentiate it. It is as if one were to define "Mobil Oil Corporation" by pointing to a single service station.

The perplexing issues of the defining properties and ontological status of the categories lower than the species may be clarified in a similar vein. Subspecies are like local branches of a widespread industrial enterprise, but the degree of differentiation can hardly be so distinct as it is at the species level, for there can be all sorts and degrees of closure, and closure is not the same as differentiation. Nonetheless, subspecies may be expected to each have a characteristic local mode of competition, and what has been called their "reality" may be compared to that of any local productive unit. Yet as I have pointed out (Ghiselin, 1966b) some lower categories do not refer to populations: the morph, the *forma sexualis*, the monstrosity, etc. These are strictly nominal classes, and for that reason are best left out of the hierarchy.

Are the taxa ranked at categorical levels higher than the species merely conceptual? The economic analogy here seems rather remote. Is the automobile industry real? One's answer depends upon his metaphysics, but at least industries have not the same ontological status as firms. Whatever answer is best, it may be worth while to explore the possibility of treating higher taxa as sectors of the natural economy. Inger (1958) suggests some thoughts along these lines for the genus, as have Gisin (1960) and Whittaker (1969) for the higher categories. On the other hand we should bear in mind the momentous difference between the natural and the political economy, in that a phylogenetic nexus is the inevitable consequence of the laws governing reproductive communities. Firms are established out of heterogeneous elements, ones not necessarily arising out of a common source. Be this as it may, the economy of nature is competitive from beginning to end, and assimilating so basic a process as competition into evolutionary classification might greatly strengthen the conceptual foundations of taxonomy.

HISTORY

One might wonder why all of this has not been self-evident for years on end. To give a full answer would require an extensive historical study, but the following suggestions may help.

Elsewhere (Ghiselin, 1972) I have presented a theoretical scheme which attempts to explain scientific innovation. For the present discussion, three main features of this scheme need to be singled out. In the first place, dealing with a problem depends

upon having an appropriate way of thinking about it. Method determines success. Second, an ability to invent or discover a new way of thinking largely is contingent upon some kind of input from without. This external input gives us the third point: discovery often involves interdisciplinary transfer. But this transfer of concepts and ways of thinking may be non-existent, shallow, or profound, according to the habits and training of the scientists involved. For various reasons some fields of research and their practitioners are relatively open, others relatively closed, to the rest. It is partly a matter of personal situation, and partly a matter of innovative mentality that determines how a given scientist will behave in such respects.

The species problem has to do with biology, but it is fundamentally a philosophical problem—a matter for the "theory of universals." There are, so to speak, "different kinds of groups," and someone trained in logic should, one might think, long ago have stepped in and cleared up the confusion. Such is demonstrably not the case, for logicians ever since Aristotle, and even quite recently, have treated species as classes (e.g., Gregg, 1950; Buck and Hull, 1966; Lehman, 1967; Ruse, 1969, 1971). There may be quite a variety of reasons for this practice, but one significant point is that philosophers habitually seek only to erect internally consistent schemes. They have little to gain by attempting to discover how the living world really is organized, or how biologists in fact conceive of it. Hence although there has been much philosophizing *about* science, there has been little that truly deserves to be called the philosophy *of* science. The usual notions of logicians are adequate enough that these can be applied to scientific classification without breaking down. Something special was needed if logic were to cope with biology as it really is. This situation is unfortunate, for not only might philosophers have much to contribute, but they might learn something as well. Thus when Wittgenstein (1953:32) wrote "'games' form a family" he should have realized that whatever it may be that they form, it is anything but a family. Poker does not copulate with Canasta or mourn for the passing of Whist. Of course, Wittgenstein wasn't talking about that; but this is just the point: he should have.

It seems curious that even Kant (1776, 1785), who made outstanding contributions to both science and philosophy, commented upon species and the views of Buffon, but contributed nothing of significance to the philosophy of taxonomy. Yet one must remember that neither Buffon nor Kant understood the competitive interactions that go on between species, between parts of different species, and within them. They, like all the pre-Darwinian biologists had a teleological conception of the universe, featuring an harmonious and cooperative natural economy here on earth. Their world-view prevented biologists and philosophers alike from thinking about species in terms other than "similarity and filiation" so that these groups almost had to be classes —analogous to defining "the Smith family" as that which resembles John Smith. No amount of study from this point of view would lead one to think in terms of natural selection, a process involving interactions between the organisms making up the group.

Only when Darwin, a geologist, transcended the limitations of the traditional way of thinking was the problem of specific transmutation possible. He reached beyond the limits of biology, to economics, and realized that evolution results from a reproductive competition between individual organisms. Hence the Darwinian revolution should be viewed as an ecological, and not as a taxonomic, revolution, in the sense that a new ecology implied a new systematics, not *vice versa*.

Yet Darwin's own outlook had significant limitations. He was so concerned with the transmutation of species that he gave insufficient emphasis to their origin. This led him to conceive of species more or less as open systems, rather than as closed systems.

In his later years, he to some extent admitted this, and agreed that isolation, albeit not necessary for evolutionary change, is helpful in speciation (letters in F. Darwin, 1887: Vol. III, pp. 157–162). Confusion between these two processes continues to this day, especially in the writings of historians (see Ghiselin, 1973). The problem becomes somewhat less difficult when one treats both species and organisms as individuals.

Biologists might have been able to understand the philosophical issues and hence to solve the species problem for themselves. However, the transfer of concepts across disciplinary boundaries from philosophy to biology proved shallow to say the least. In fact, the most elementary of philosophical notions became garbled. "Organism" was confounded with "particular" because, at least in part, "individual" had designated both. However, more may be involved than just an equivocation. When someone uses a notion borrowed from an unfamiliar field of experience, he tends to confuse it with a more familiar one which somewhat resembles it. An additional class of examples is available in what is sometimes called "psychologism in logic"—confusing logical concepts with psychological namesakes or analogues (Ghiselin, 1966a).

The so-called "nominalistic species concept" is based on the idea that classes are not real, and if species are classes, it would follow that species are not real. One way of answering it is to deny the basic premise and affirm that classes are real. However, this is quite unnecessary, for species are individuals, and nominalists believe that individuals are real. The nominalistic species concept resulted from a garbled version of thirteenth century philosophy. It is impressive to see how many biologists have supported it. To give a few examples: Bessey, 1908; Britton, 1908; Lotsy, 1925; Burma, 1949, 1954; Mason, 1950; Thompson, 1952; Davidson, 1954; Cowan, 1962. Rebuttals have taken various forms. One way of answering it has been to say that species are real in the sense that they are classes with members (Plate, 1914; Turrill, 1942; Gregg, 1950). A few have maintained a kind of idealism: to Agassiz (1857) species were thoughts of the Creator, which are real. Many biologists have conceived of species as individuals, but instead of putting it in these terms, they have merely said that they are not the same kind of groups as classes are. Some have viewed them as phylogenetic units (e.g., Harper, 1923; Hall, 1926; Faegri, 1937; Simpson, 1951, 1961). Others have treated them as units of a genetical nature (e.g., Huxley, 1940; Mayr, 1940, 1957, 1963; Camp and Gilly, 1943; Dobzhansky, 1950; Carson, 1957). Originally Dobzhansky (1935) treated them as classes of individuals, while Mayr treated them as classes of populations, but nonetheless the notion of a larger unit was there. Such genetical population concepts tended to grade into notions of species as classes of genetically similar individuals.

A more extreme variant of the biological species concept might be called the "organicist species concept." By this would be meant the notion that species are "superorganisms." It asserts that species have properties such as adaptation which do exist in organisms, but which have been attributed to the higher level systems mainly by false analogy. Organicism is an old notion, one closely associated with a vitalist doctrine called "Holism" (for its history see Ghiselin, 1974:29–30), which treats the generality of classes as if they were composite wholes: as "aggregates" or individuals. This view carries strong teleological implications, for if species are presupposed to be cooperative units like organisms, then their component organisms would not compete, but would do things "for the good of the species." Holism strongly influenced Emerson (1938) and other systematists and ecologists of the Chicago school, and is particularly evident in the later writings of Du Rietz (1930). It is largely responsible for the group selectionist notions of many evolutionists (e.g., Darlington, 1940). Indeed, one can find a strong trace of it in the writings of those who certainly would

not endorse the organicist species concept were it formulated in explicit terms.

One good line of evidence suggesting that Holism has been influential is the degree to which intraspecific competition has been deemphasized in favor of supposed advantages (often, but not always, to the species) of maintaining populational integrity and specific distinctness. In his well-known book on species Mayr (1963) strongly deemphasized sexual selection in favor of isolating mechanisms in explaining sexual dimorphism. Lately (Mayr, 1972) he has changed the emphasis to a considerable extent, but some of us would feel that he has not gone far enough (Ghiselin, 1974). Likewise, we find Mayr (1969b:316) saying "The division of the total genetic variability of nature into discrete packages, the so-called species, which are separated from each other by reproductive barriers prevents the production of too great a number of disharmonious incompatible gene combinations." This could be interpreted as merely a byproduct, rather than a function or reason for existence, of species. But one might wonder about the following (Mayr, 1969b:318) "*Species are the real units of evolution*, they are the entities which specialize, which become adapted, or which shift their adaptation." Species are units, and they have evolutionary importance, but the same may be said of organisms. Doubtless both organisms and species specialize. And probably organisms become adapted but species do not, except in so far as they consist of adapted organisms. Such ambiguities represent a transitional stage in our growing appreciation of what the two levels mean. We are experiencing a rapid and fundamental restructuring of our basic concepts.

DISCUSSION

The species problem has thus involved difficulties in understanding the ontological status of fundamental biological units, and failure to interrelate levels of integration in the appropriate manner. The reason why the economic analogies have so much heuristical value is that we find it much easier to think of firms as individuals. For this very reason a host of biological problems are more readily soluble if one learns to think like an economist. In addition we should note that we often fail to solve our problems because we cannot even identify them. Under such circumstances, conceptual investigations do more than just help. They are the only way out.

ACKNOWLEDGMENTS

For comments on drafts of the manuscript, I am grateful to Theodosius Dobzhansky, Patrick M. Gaffney, David L. Hull, Ernst Mayr, David Wake and Leigh Van Valen. Support was provided by N.S.F. Grant GS 33491.

REFERENCES

AGASSIZ, L. 1857. Essay on classification. Reprint 1962, E. Lurie (Ed.). Cambridge: Harvard University Press.

BESSEY, C. E. 1908. The taxonomic aspect of the species question. Amer. Nat. 42:218–224.

BRITTON, N. L. 1908. The taxonomic aspect of the species question. Amer. Nat. 42:224–242.

BUCK, R. C., AND D. L. HULL. 1966. The logical structure of the Linnaean hierarchy. Syst Zool. 15:97–111.

BURMA, B. 1949. The species concept: a semantic review. Evolution 3:369–370.

BURMA, B. H. 1954. Reality, existence, and classification: a discussion of the species problem. Madroño 127:193–209.

CAMP, W. H., AND C. L. GILLY. 1943. The structure and origin of species with a discussion of intraspecific variability and related nomenclatural problems. Brittonia 4:323–385.

CARSON, H. L. 1957. The species as a field for gene recombination. *In:* E. Mayr (Ed.), The species problem. Washington: A.A.A.S. Pp. 23–38.

COWAN, S. T. 1962. The microbial species—a macromyth? Symp. Soc. Gen. Microbiol. 12: 433–455.

DARLINGTON, C. D. 1940. Taxonomic species and genetic systems. *In:* J. S. Huxley (Ed.), The new systematics. Oxford: University Press. Pp. 137–160.

DARWIN, F. 1887. The life and letters of Charles Darwin, including an autobiographical chapter. Vol. III.

DAVIDSON, J. F. 1954. A dephlogisticated species concept. Madroño 12:246–251.

DOBZHANSKY, TH. 1935. A critique of the species concept in biology. Phil. Sci. 2:344–355.

DOBZHANSKY, TH. 1950. Mendelian populations and their evolution. Amer. Nat. 84:401–418.

DU RIETZ, G. E. 1930. The fundamental units of biological taxonomy. Svensk Bot. Tidskr. 24: 333–428.

EHRLICH, P. R., AND P. H. RAVEN. 1969. Differentiation of populations. Science 165:1228–1231.

EMERSON, A. E. 1938. The origin of species. Ecology 19:152–154.

FAEGRI, K. 1937. Some fundamental problems of taxonomy and phylogenetics. Bot. Rev. 3: 400–423.

GHISELIN, M. T. 1966a. On psychologism in the logic of taxonomic controversies. Syst. Zool. 26: 207–215.

GHISELIN, M. T. 1966b. On some taxonomic categories which have been proposed for the classification of large gastropod genera. Veliger 9:141–144.

GHISELIN, M. T. 1969. The triumph of the Darwinian method. Berkeley: University of California Press.

GHISELIN, M. T. 1972. The individual in the Darwinian revolution. New Lit. Hist. 113–134.

GHISELIN, M. T. 1973. Mr. Darwin's Critics, old and new. J. Hist. Biol. 6:155–165.

GHISELIN, M. T. 1974. The economy of nature and the evolution of sex. Berkeley: University of California Press.

GISIN, H. 1966. Signification des modalités de l'évolution pour la théorie de la systématique. Z. Zool. Syst. Evol. 4:1–12.

GREGG, J. R. 1950. Taxonomy, language and reality. Amer. Nat. 84:419–435.

HALL, H. M. 1926. The taxonomic treatment of units smaller than species. Proc. Int. Congr. Plant Sci. Ithaca 2:1461–1468.

HARPER, R. A. 1923. The species concept from the point of view of a morphologist. Amer. J. Bot. 10:229–233.

HULL, D. L. 1974. Philosophy of Biological Science. Englewood Cliffs: Prentice-Hall.

HUXLEY, J. S. 1940. Towards the new systematics. *In:* J. S. Huxley (Ed.), The new systematics. Oxford: University Press. Pp. 1–46.

INGER, R. F. 1958. Comments on the definition of genera. Evolution 12:370–384.

KANT, I. 1776. Von den verschiedenen Rassen der Menschen. Reprinted *in* K. Vorländer (Ed.), 1922, Immanuel Kant: vermischte Schriften. Leipzig: Felix Meiner. Pp. 79–98.

KANT, I. 1785. Bestimmung des Begriffs einer Menschenrasse. Reprinted *in* K. Vorländer (Ed.), 1922. Immanuel Kant: vermischte Schriften. Leipzig: Felix Meiner. Pp. 111–127.

LEHMAN, H. 1967. Are biological species real? Phil. Sci. 34:157–167.

LOTSY, J. P. 1925. Species or Linneon. Genetica 7:487–506.

MASON, H. L. 1950. Taxonomy, systematic botany and biosystematics. Madroño 10:193–208.

MAYR, E. 1940. Speciation phenomena in birds. Amer. Nat. 74:249–278.

MAYR, E. 1957. Difficulties and importance of the biological species concept. *In:* E. Mayr (Ed.), The species problem. Washington: A.A.A.S. Pp. 371–388.

MAYR, E. 1963. Animal species and evolution. Cambridge: Harvard University Press.

MAYR, E. 1969a. Principles of systematic zoology. New York: McGraw-Hill.

MAYR, E. 1969b. The biological meaning of species. Biol. J. Linn. Soc. 1:311–320.

MAYR, E. 1972. Sexual selection and natural selection. *In:* B. G. Campbell (Ed.), Sexual selection and *The Descent of Man* 1871–1971. Chicago: Aldine. Pp. 87–104.

PLATE, L. 1914. Prinzipien der Systematik mit besonderer Berücksichtigung des Systems der Tiere. *In:* P. Hinneberg (Ed.), Die Kultur der Gegenwart, (3:4)4:92–164.

RUSE, M. 1969. Definitions of species in biology. Brit. J. Phil. Sci. 20:97–119.

RUSE, M. 1971. The species problem: a reply to Hull. Brit. J. Phil. Sci. 22:369–371.

RUSE, M. 1973. The philosophy of biology. London: Hutchinson University Library.

SIMPSON, G. G. 1951. The species concept. Evolution 5:285–298.

SIMPSON, G. G. 1961. Principles of animal taxonomy. New York: Columbia University Press.

SNEATH, P. H. A., AND R. R. SOKAL. 1973. Numerical taxonomy: the principles and practice of numerical classification. San Francisco: W. H. Freeman.

SOKAL, R. R., AND T. J. CROVELLO. 1970. The biological species concept: a critical evaluation. Amer. Nat. 104:127–153.

STRAWSON, P. F. 1959. Individuals: an essay on descriptive metaphysics. London: Methuen.

THOMPSON, W. R. 1952. The philosophical foundations of systematics. Canad. Entomol. 84: 1–16.

TURRILL, W. B. 1942. Taxonomy and phylogeny. Bot. Rev. 8:247–270, 473–532, 655–707.

WHITTAKER, R. H. 1969. New concepts of kingdoms of organisms. Science 163:150–160.

WITTGENSTEIN, L. 1958. Philosophical investigations. Second ed. New York: Macmillan.

Manuscript received April, 1974