



A Matter of Individuality

David L. Hull

Philosophy of Science, Vol. 45, No. 3. (Sep., 1978), pp. 335-360.

Stable URL:

<http://links.jstor.org/sici?sici=0031-8248%28197809%2945%3A3%3C335%3AAMOI%3E2.0.CO%3B2-X>

Philosophy of Science is currently published by The University of Chicago Press.

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/about/terms.html>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/journals/ucpress.html>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

The JSTOR Archive is a trusted digital repository providing for long-term preservation and access to leading academic journals and scholarly literature from around the world. The Archive is supported by libraries, scholarly societies, publishers, and foundations. It is an initiative of JSTOR, a not-for-profit organization with a mission to help the scholarly community take advantage of advances in technology. For more information regarding JSTOR, please contact support@jstor.org.

Philosophy of Science

September, 1978

A MATTER OF INDIVIDUALITY*

DAVID L. HULL†

University of Wisconsin-Milwaukee

Biological species have been treated traditionally as spatiotemporally unrestricted classes. If they are to perform the function which they do in the evolutionary process, they must be spatiotemporally localized individuals, historical entities. Reinterpreting biological species as historical entities solves several important anomalies in biology, in philosophy of biology, and within philosophy itself. It also has important implications for any attempt to present an “evolutionary” analysis of science and for sciences such as anthropology which are devoted to the study of single species.

1. Introduction. The terms “gene,” “organism” and “species” have been used in a wide variety of ways in a wide variety of contexts. Anyone who attempts merely to map this diversity is presented with a massive and probably pointless task. In this paper I consciously ignore “the ordinary uses” of these terms, whatever they might be, and concentrate on their biological uses. Even within biology, the variation and conflicts in meaning are sufficiently extensive to immobilize all but the most ambitious ordinary language philosopher. Thus, I have narrowed my focus even further to concentrate on the role which these terms play in evolutionary biology. In doing so, I do not mean to imply that this usage is primary or that all other biological uses which conflict with it are mistaken. Possibly evolutionary theory

*Received August, 1977; revised October, 1977.

†The research for this paper was supported by NSF grant Soc 75 03535. I am indebted to the following people for reading and criticizing early versions of this paper: Michael Ghiselin, Stephen Gould, G. C. D. Griffiths, John Koethe, Ernst Mayr, Bella Selan, W. J. van der Steen, Gareth Nelson, Michael Perloff, Mark Ridley, Michael Ruse, Thomas Schopf, Paul Teller, Leigh Van Valen, Linda Wessels, Mary Williams, and William Wimsatt. Their advice and criticisms are much appreciated.

Philosophy of Science, 45 (1978) pp. 335–360.

Copyright © 1978 by the Philosophy of Science Association.

is *the* fundamental theory in biology, and all other biological theories must be brought into accord with it. Possibly all biological theories, including evolutionary theory, eventually will be reduced to physics and chemistry. But regardless of the answers to these global questions, at the very least, various versions of evolutionary theory are sufficiently important in biology to warrant an investigation of the implications which they have for the biological entities which they concern.

Genes are the entities which are passed on in reproduction and which control the ontogenetic development of the organism. Organisms are the complex systems which anatomists, physiologists, embryologists, histologists, etc. analyze into their component parts. Species have been treated traditionally as the basic units of classification, the natural kinds of the living world, comparable to the physical elements. But these entities also function in the evolutionary process. Evolution consists in two processes (mutation and selection) which eventuate in a third (evolution). Genes provide the heritable variation required by the evolutionary process. Traditionally organisms have been viewed as the primary focus of selection, although considerable disagreement currently exists over the levels at which selection takes place. Some biologists maintain that selection occurs exclusively at the level of genes; others that supragenetic, even supraorganismic units can also be selected. As one might gather from the title of Darwin's book, species are the things which are supposed to evolve. Whether the relatively large units recognized by taxonomists as species evolve or whether much less extensive units such as populations are the effective units of evolution is an open question. In this paper when I use the term "species," I intend to refer to those supraorganismic entities which evolve regardless of how extensive they might turn out to be.

The purpose of this paper is to explore the implications which evolutionary theory has for the ontological status of genes, organisms and species. The only category distinction I discuss is between individuals and classes. By "individuals" I mean spatiotemporally localized cohesive and continuous entities (historical entities). By "classes" I intend spatiotemporal unrestricted classes, the sorts of things which can function in traditionally-defined laws of nature. The contrast is between Mars and planets, the Weald and geological strata, between Gargantua and organisms. The terms used to mark this distinction are not important, the distinction is. For example, one might distinguish two sorts of sets: those that are defined in terms of a spatiotemporal relation to a spatiotemporally localized focus and those that are not. On this view, historical entities such as Gargantua become sets. But they are sets of a very special kind—sets defined

in terms of a spatiotemporal relation to a spatiotemporally localized focus. Gargantua, for instance, would be the set of all cells descended from the zygote which gave rise to Gargantua.

The reason for distinguishing between historical entities and genuine classes is the differing roles which each plays in science according to traditional analyses of scientific laws. Scientific laws are supposed to be spatiotemporally unrestricted generalizations. No uneliminable reference can be made in a genuine law of nature to a spatiotemporally individuated entity. To be sure, the distinction between accidentally true generalizations (such as all terrestrial organisms using the same genetic code) and genuine laws of nature (such as those enshrined in contemporary versions of celestial mechanics) is not easy to make. Nor are matters helped much by the tremendous emphasis placed on laws in traditional philosophies of science, as if they were the be-all and end-all of science. Nevertheless, I find the distinction between those generalizations that are spatiotemporally unrestricted and those that are not fundamental to our current understanding of science. Whether one calls the former "laws" and the latter something else, or whether one terms both sorts of statements "laws" is of little consequence. The point I wish to argue is that genes, organisms *and* species, as they function in the evolutionary process, are necessarily spatiotemporally localized individuals. They could not perform the functions which they perform if they were not.

The argument presented in this paper is metaphysical, not epistemological. Epistemologically red light may be fundamentally different from infrared light and mammals from amoebae. Most human beings can see with red light and not infrared light. Most people can see mammals; few if any can see amoebae with the naked eye. Metaphysically they are no different. Scientists know as much about one as the other. Given our relative size, period of duration and perceptual acuity, organisms appear to be historical entities, species appear to be classes of some sort, and genes cannot be seen at all. However, after acquainting oneself with the various entities which biologists count as organisms and the roles which organisms and species play in the evolutionary process, one realizes exactly how problematic our common sense notions actually are. The distinction between an organism and a colony is not sharp. If an organism is the "total product of the development of the impregnated embryo," then as far back as 1899, T. H. Huxley was forced to conclude that the medusa set free from a hydrozoan" are as much organs of the latter, as the multitudinous pinnules of a *Comatula*, with their genital glands, are organs of the Echinoderm. Morphologically, therefore, the equivalent of the individual *Comatula* is the Hydrozoic stock and all the

Medusae which proceed from it” ([24]). More recently, Daniel Janzen [25] has remarked that the “study of dandelion ecology and evolution suffers from confusion of the layman’s ‘individual’ with the ‘individual’ of evolutionary biology. The latter individual has ‘reproductive fitness’ and is the unit of selection in most evolutionary conceptualizations” (see also [2]). According to evolutionists, units of selection, whether they be single genes, chromosomes, organisms, colonies or kinship groups are individuals. In this paper I intend to extend this analysis to units of evolution.

If the ontological status of space-time in relativity theory is philosophically interesting in and of itself (and God knows enough philosophers have written on that topic), then the ontological status of species in evolutionary theory should also be sufficiently interesting philosophically to discuss without any additional justification. However, additional justification does exist. From Socrates and Plato to Kripke and Putnam, organisms have been paradigm examples of primary substances, particulars and/or individuals, while species have served as paradigm examples of secondary substances, universals and/or classes. I do not think that this paper has any necessary implications for various solutions to the problem of universals, identity and the like. However, if the main contention of this paper is correct, if species are as much spatiotemporally localized individuals as organisms, then some of the confusion among philosophers over these issues is understandable. One of the commonest examples used in the philosophical literature is inappropriate. Regardless of whether one thinks that “Moses” is a proper name, a cluster concept or a rigid designator, “*Homo sapiens*” must be treated in the same way.

2. The Evolutionary Justification. Beginning with the highly original work of Michael Ghiselin ([12], [13], [14]), biologists in increasing numbers are beginning to argue that species as units of evolution are historical entities ([15], [20], [21], [22], [23], [34], [38]). The justification for such claims would be easier if there were one set of propositions (presented preferably in axiomatic form) which could be termed *the* theory of evolution. Unfortunately, there is not. Instead there are several, incomplete, partially incompatible versions of evolutionary theory currently extant. I do not take this state of affairs to be unusual, especially in periods of rapid theoretical change. In general the myth that some one set of propositions exists which can be designated unequivocally as Newtonian theory, relativity theory, etc. is an artifact introduced by lack of attention to historical development and unconcern with the primary literature of science. The only

place one can find *the* version of a theory is in a textbook written long after the theory has ceased being of any theoretical interest to scientists.

In this section I set out what it is about the evolutionary process which results in species being historical entities, not spatiotemporally unrestricted classes. In doing so I have not attempted to paper over the disagreements which currently divide biologists working on evolutionary theory. For example, some disagreement exists over how abruptly evolution can occur. Some biologists have argued that evolution takes place saltatively, in relatively large steps. Extreme saltationists once claimed that in the space of a single generation new species can arise which are so different from all other species that they have to be placed in new genera, families, classes, etc. No contemporary biologist to my knowledge currently holds this view. Extreme gradualists, on the other side, argue that speciation *always* occurs very slowly, over periods of hundreds of generations, either by means of a single species changing into a new species (phyletic evolution) or else by splitting into two large subgroups which gradually diverge (speciation). No contemporary biologist holds this view either. Even the most enthusiastic gradualists admit that new species can arise in a single generation, e.g., by means of polyploidy. In addition, Eldredge and Gould [11], building on Mayr's founder principle [36], [37], have recently argued that speciation typically involves small, peripheral isolates which develop quite rapidly into new species. Speciation is a process of "punctuated equilibria."

However, the major dispute among contemporary evolutionary theorists is the level (or levels) at which selection operates. Does selection occur *only* and *literally* at the level of genes? Does selection take place *exclusively* at the level of organisms, the selection of genes being only a consequence of the selection of organisms? Can selection also take place at levels of organization more inclusive than the individual organism, e.g., at the level of kinship groups, populations and possibly even entire species? Biologists can be found opting for every single permutation of the answers to the preceding questions. I do not propose to go through all the arguments which are presented to support these various conclusions. For my purposes it is sufficient to show that the points of dispute are precisely those which one might expect if species are being interpreted as historical entities, rather than as spatiotemporally unrestricted classes. Richard Dawkins puts the crucial issue as follows:

Natural selection in its most general form means the differential survival of entities. Some entities live and others die but, in order

for this selective death to have any impact on the world, an additional condition must be met. Each entity must exist in the form of lots of copies, and at least some of the entities must be *potentially* capable of surviving—in the form of copies—for a significant period of evolutionary time. ([8], p. 35)

The results of evolution by natural selection are *copies* of the entities being selected, not *sets*. Elements in a set must be characterized by one or more common characteristics. Even fuzzy sets must be characterized by at least a “cluster” of traits. Copies need not be.¹ A particular gene is a spatiotemporally localized individual which either may or may not replicate itself. In replication the DNA molecule splits down the middle producing two new molecules composed *physically* of half of the parent molecule while *largely* retaining its structure. In this way genes form lineages, ancestor-descendant copies of some original molecule. The relevant genetic units in evolution are not *sets* of genes defined in terms of structural similarity but lineages formed by the imperfect copying process of replication.² Genes can belong to the same lineage even though they are structurally different from other genes in that lineage. What is more, continued changes in structure can take place indefinitely. If evolution is to occur, not only can such indefinite structural variation take place within gene lineages but also it must. Single genes are historical entities, existing for short periods of time. The more important notion is that of a *gene lineage*. Gene lineages are also historical entities persisting while changing indefinitely through time. As Dawkins puts this point:

Genes, like diamonds, are forever, but not quite in the same way as diamonds. It is an individual diamond crystal which lasts, as an unaltered pattern of atoms. DNA molecules don't have that kind of permanence. The life of any one physical DNA molecule is quite short—perhaps a matter of months, certainly not more than one lifetime. But a DNA molecule could theoretically live on in the form of *copies* of itself for a hundred million years. ([8], p.36)

Exactly the same observations can be made with respect to organ-

¹Once again, I am excluding from the notion of class those “classes” defined by means of a spatiotemporal relation to a spatiotemporally localized individual. Needless to say, I am also excluding such constructions as “similar in origin” from the classes of similarities. I wish the need to state the obvious did not exist, but from past experience it does.

²In population genetics the distinction between structurally similar genes forming a single lineage and those which do not is marked by the terms “identical” and “independent”; see [41] pp. 56–57.

isms. A particular organism is a spatiotemporally localized individual which either may or may not reproduce itself. In asexual reproduction, part of the parent organism buds off to produce new individuals. The division can be reasonably equitable as in binary fission or else extremely inequitable as in various forms of parthenogenesis. In sexual reproduction gametes are produced which unite to form new individuals. Like genes, organisms form lineages. The relevant organismal units in evolution are not sets of organisms defined in terms of structural similarity but lineages formed by the imperfect copying processes of reproduction. Organisms can belong to the same lineage even though they are structurally different from other organisms in that lineage. What is more, continued changes in structure can take place indefinitely. If evolution is to occur, not only can such indefinite structural variation take place within organism lineages but also it must. Single organisms are historical entities, existing for short periods of time. Organism lineages are also historical entities persisting while changing indefinitely through time.

Both replication and reproduction are spatiotemporally localized processes. There is no replication or reproduction at a distance. Spatiotemporal continuity through time is required. Which entities at which levels of organization are sufficiently cohesive to function as units of selection is more problematic. Dawkins presents one view:

In sexually reproducing species, the individual [the organism] is too large and too temporary a genetic unit to qualify as a significant unit of natural selection. The group of individuals is an even larger unit. Genetically speaking, individuals and groups are like clouds in the sky or dust-storms in the desert. They are temporary aggregates of federations. They are not stable through evolutionary time. Populations may last a long while, but they are constantly blending with other populations and so losing their identity. They are subject to evolutionary change from within. A population is not a discrete enough entity to be a unit of natural selection, not stable and unitary enough to be 'selected' in preference to another population. ([8], p. 37)

From a common sense perspective, organisms are paradigms of tightly organized, hierarchically stratified systems. Kinship groups such as hives also seem to be internally cohesive entities. Populations and species are not. Dawkins argues that neither organisms (in sexually reproducing species) nor populations in any species are sufficiently permanent and cohesive to function as units in selection. In asexual species, organisms do not differ all that much from genes. They subdivide in much the same way that genes do, resulting in progeny

which are identical (or nearly identical) with them. In sexual species, however, organisms must pool their genes to reproduce. The resulting progeny contain a combined sample of parental genes. Populations lack even this much cohesion.

Other biologists are willing to countenance selection at levels more inclusive than the individual gene, possibly parts of chromosomes, whole chromosomes, entire organisms or even kinship groups ([32]). The issues, both empirical and conceptual, are not simple. For example, G. C. Williams in his classic work ([61]) argues that selection occurs only at the level of individuals. By "individual" biologists usually mean "organism." However, when Williams is forced to admit that kinship groups can also function as units of selection, he promptly dubs them "individuals." One of the commonest objections to E. O. Wilson's ([62]) equally classic discussion of evolution is that he treats kin selection as a special case of group selection. According to the group selectionists, entities more inclusive than kinship groups can also function as units of selection ([63]).³ Matters are not improved much by vagueness over what is meant by "units of selection." Gene frequencies are certainly altered from generation to generation, but so are genotype frequencies. Genes cannot be selected in isolation. They depend on the success of the organism which contains them for their survival. Most biologists admit that similar observations hold for certain kinship groups. Few are willing to extend this line of reasoning to include populations and entire species.

As inconclusive as the dispute over the level(s) at which selection takes place is, the points at issue are instructive. In arguing that neither organisms nor populations function as units of selection in the same sense that genes do, Dawkins does not complain that the cells in an organism or the organisms in a population are phenotypically quite diverse, though they frequently are. Rather he denigrates their cohesiveness and continuity through time, criteria which are relevant to individuating historical entities, not spatiotemporally unrestricted classes. Difficulties about the level(s) at which selection can operate to one side, the issue with which we are concerned is the ontological status of species. Even if entire species are not sufficiently well

³Until recently even the most ardent group selectionists admitted that the circumstances under which selection can occur at the level of populations and/or entire species are so rare that group selection is unlikely to be a major force in the evolutionary process ([30], [32], [33]). Michael Wade ([59]), however, has presented a convincing argument to the effect that the apparent rarity of group selection may be the result of the assumptions commonly made in constructing mathematical models for group selection and not an accurate reflection of the actual state of nature. In his own research, the differential survival of entire populations has produced significant divergence.

integrated to function as units of selection, they are the entities which evolve as a result of selection at lower levels. The requirements of selection at these lower levels place constraints on the manner in which species can be conceptualized. Species as the results of selection are necessarily lineages, not sets of similar organisms. In order for differences in gene frequencies to build up in populations, continuity through time must be maintained. To some extent genes in sexual species are reassorted each generation, but the organisms which make up populations cannot be. To put the point in the opposite way, if such shuffling of organisms were to take place, selection would be impossible.

The preceding characteristic of species as evolutionary lineages by itself is sufficient to preclude species being conceptualized as spatiotemporally unrestricted sets or classes. However, if Eldredge and Gould are right, the case for interpreting species as historical entities is even stronger. They ask why species are so coherent, why groups of relatively independent local populations continue to display fairly consistent, recognizable phenotypes, and why reproductive isolation does not arise in every local population if gene flow is the only means of preventing differentiation:

The answer probably lies in a view of species and individuals [organisms] as homeostatic systems—as amazingly well-buffered to resist change and maintain stability in the face of disturbing influences In this view, the importance of peripheral isolates lies in their small size and the alien environment beyond the species border that they inhabit—for only here are selective pressures strong enough and the inertia of large numbers sufficiently reduced to produce the “genetic revolution” (Mayr, 1963, p. 533) that overcomes homeostasis. The coherence of a species, therefore, is not maintained by interaction among its members (gene flow). It emerges, rather, as an historical consequence of the species’ origin as a peripherally isolated population that acquired its own powerful homeostatic system. ([11], p. 114)

Eldredge and Gould argue that, from a theoretical point of view, species appear so amorphous because of a combination of the gradualistic interpretation of speciation and the belief that gene exchange is the chief (or only) mechanism by which cohesion is maintained in natural populations. However, in the field, species of both sexual and asexual organisms seem amazingly coherent and unitary. If gene flow were the only mechanism for the maintenance of evolutionary unity, asexual species should be as diffuse as dust-storms in the desert. According to Eldredge and Gould, new species arise through the

budding off of peripheral isolates which succeed in establishing new equilibria in novel environments. Thereafter they remain largely unchanged during the course of their existence and survive only as long as they maintain this equilibrium.

Another possibility is that evolutionary unity is maintained by both internal and external means. Gene flow and homeostasis within a species are internal mechanisms of evolutionary unity. Perhaps the external environment in the form of unitary selection pressures also contributes to the integrity of the entities which are evolving ([10]). For example, Jews have remained relatively distinct from the rest of humankind for centuries, in part by internal means (selective mating, social customs, etc.) but also in part by external means (discrimination, prejudice, laws, etc). An ecological niche is a relation between a particular species and key environmental variables. A different species in conjunction with the same environmental variables could define quite a different niche. In the past biologists have tended to play down the integrating effect of the environment, attributing whatever unity and coherence which exists in nature to the integrating effect of gene complexes. At the very least, if the coherence of asexual species is not illusory, mechanisms other than gene flow must be capable of bringing about evolutionary unity.

3. Individuating Organisms and Species. By and large, the criteria which biologists use to individuate organisms are the same as those suggested by philosophers—spatiotemporal continuity, unity and location. Differences between these two analyses have three sources: first, philosophers have been most interested in individuating persons, the hardest case of all, while biologists have been content to individuate organisms; second, when philosophers have discussed the individuation of organisms, they have usually limited themselves to adult mammals, while biologists have attempted to develop a notion of organism adequate to handle the wide variety of organisms which exist in nature; and finally, philosophers have felt free to resort to hypothetical, science fiction examples to test their conceptions, while biologists rely on actual cases. In each instance, I prefer the biologists' strategy. A clear notion of an individual organism seems an absolute prerequisite for any adequate notion of a person, and this notion should be applicable to all organisms, not just a minuscule fraction. But most importantly, real examples tend to be much more detailed and bizarre than those made up by philosophers. Too often the example is constructed for the sole purpose of supporting the preconceived intuitions of the philosophers and has no life of its own. It cannot force the philosopher to improve his analysis the way that real examples can. Biologists

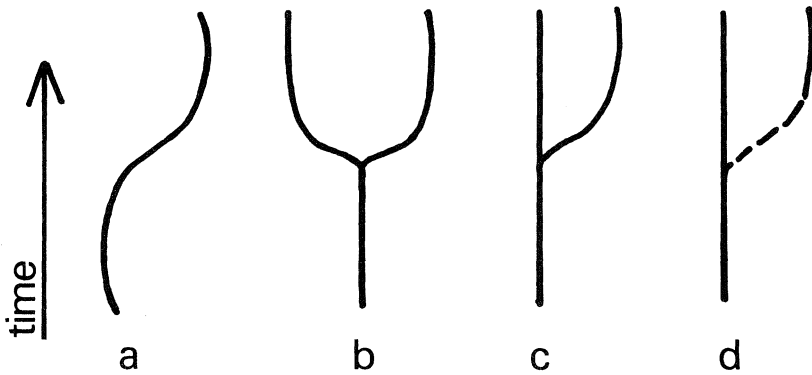


Figure 1: Diagrams which can be interpreted alternately as organisms undergoing ontogenetic change and the production of new organisms and as species undergoing phylogenetic change and speciation.

are in the fortunate position of being able to test their analyses against a large stock of extremely difficult, extensively documented actual cases.

Phenotypic similarity is irrelevant in the individuation of organisms. Identical twins do not become one organism simply because they are phenotypically indistinguishable. Conversely, an organism can undergo massive phenotypic change while remaining the same organism. The stages in the life cycles of various species of organisms frequently are so different that biologists have placed them in different species, genera, families and even classes—until the continuity of the organism was discovered. If a caterpillar develops into a butterfly, these apparently different organisms are stages in the life cycle of a single organism regardless of how dissimilar they might happen to be (see *Figure 1a*). In ontogenetic development, a single lineage is never divided successively in time into separate organisms. Some sort of splitting is required. In certain cases, such as transverse fission in paramecia, a single organism splits equally into two new organisms (see *Figure 1b*). In such cases, the parent organism no longer exists, and the daughter organisms are two new individuals. Sometimes a single individual will bud off other individuals which are roughly its own size but somewhat different in appearance, e.g., strobilization in certain forms of Scyphozoa (see *Figure 1c*). At the other extreme, sometimes a small portion of the parent organism buds off to form a new individual, e.g., budding in Hydrozoa (see *Figure 1d*). In the latter two cases, the parent organism continues to exist while budding off new individuals. The relevant consideration is how much of the parent organism is lost and its internal organization disrupted.

Fusion also takes place at the level of individual organisms. For example, when presented with a prey too large for a single individual to digest, two amoebae will fuse cytoplasmically in order to engulf and digest it. However, the nuclei remain distinct and the two organisms later separate, genetically unchanged. The commonest example of true fusion occurs when germ cells unite to form a zygote. In such cases, the germ cells as individuals cease to exist and are replaced by a new individual (see *Figure 2a*). Sometimes one organism will invade another and become part of it. Initially, these organisms, even when they become obligate parasites, are conceived of as separate organisms, but sometimes they can become genuine parts of the host organism. For example, one theory of the origin of certain cell organelles is that they began as parasites. Blood transfusions are an unproblematic case of part of one organism becoming part of another; conjugation is another (see *Figure 2b*). Sometimes parts of two different organisms can merge to form a third. Again, sexual reproduction is the commonest example of such an occurrence (see *Figure 2c*). In each of these cases, organisms are individuated on the basis of the amount of material involved and the effect of the change on the internal organization of the organisms. For example, after conjugation two paramecia are still two organisms and the same two organisms even though they have exchanged some of their genetic material.

If species are historical entities, then the same sorts of considerations

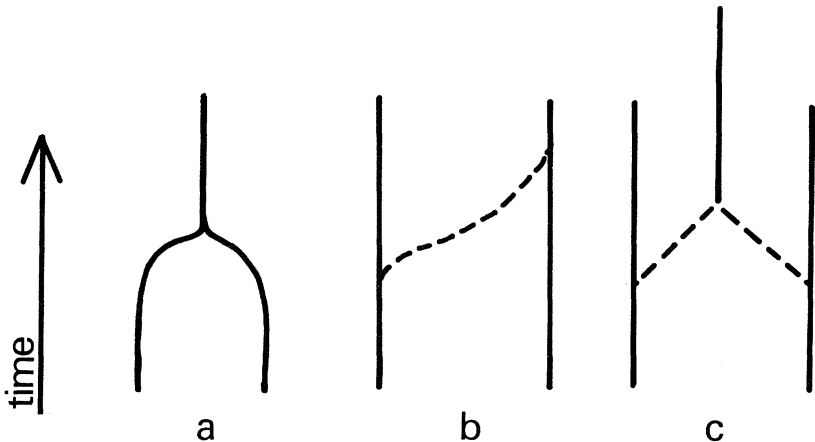


Figure 2: Diagrams which can be interpreted alternately as organisms merging totally or partially to give rise to new organisms and as species merging totally or partially to give rise to new species.

which apply in the individuation of organisms should also apply to them, and they do ([35]). The only apparent discrepancy results from the fact that not all biologists have been totally successful in throwing off the old pre-evolutionary view of species as classes of similar organisms and replacing it with a truly evolutionary view. However, even these discrepancies are extremely instructive. For example, G. G. Simpson ([50]) maintains that a single lineage which changes extensively through time without speciating (splitting) should be divided into separate species (see *Figure 1a*). Willi Hennig ([17]) disagrees: new species should be recognized only upon splitting. This particular debate has been involved, touching upon both conceptual and empirical issues. For example, how can a gradually evolving lineage be divided into discrete species in an objective, nonarbitrary way? Are later organisms considered to belong to different species from their ancestors because they are sufficiently dissimilar or because they can no longer interbreed with them even if they coexisted? Can such extensive change take place in the absence of speciation?

I cannot attempt to answer fully all of these questions here. Instead, I must limit myself to the remark that, on Simpson's view, species and organisms are quite different sorts of things. An organism undergoes limited change, constrained by its largely unchanging genotype. A single species is capable of indefinite, open-ended development. Although the course of a species' development is constrained from generation to generation by its gene pool, this gene pool is indefinitely modifiable. However, if Eldredge and Gould are right, species are more like organisms than anyone has previously supposed. Both are finite and can undergo only limited change before ceasing to exist. Significant evolutionary change can take place only through a series of successive species, not within the confines of a single species. Species lineages, not species, are the things which evolve. On this view, Hennig's refusal to divide a single lineage into two or more species is preferable to Simpson's alternative.

No disagreement exists between Simpson and Hennig over the situation depicted in *Figure 1b*, a single species splitting equally into two. Both agree that the ancestor species is extinct, having given rise to two new daughter species. However, this figure is drawn as if divergence always takes place upon speciation. When this diagram was interpreted as depicting the splitting of one organism into two, divergence was not presupposed. Two euglenae resulting from binary fission are two organisms and not one even though they may be phenotypically and genotypically identical. The same is true of species. Sometimes speciation takes place with no (or at least extremely minimal) divergence; e.g., sibling species are no less two species

simply because they look alike. The assumption is, however, that in reproductively isolated species some divergence, at least in the mechanisms of reproduction, must have taken place, even if we cannot detect it. The role of similarity becomes controversial once again when speciation takes place and one species remains unchanged, while the other diverges from the parental type (see *Figure 1c*). According to Hennig ([17]), when speciation occurs, the ancestor species must be considered extinct regardless of how similar it might be to one of its daughter species. Simpson ([50]) disagrees.

The factor which is causing the confusion in the preceding discussion is the role of similarity in the individuation of species. If species are classes defined by sets (or clusters) of traits, then similarity should be relevant. At one extreme, the pheneticists ([54]) argue that all that matters is phenetic similarity and dissimilarity, regardless of descent, reproduction, evolutionary cohesiveness, etc. Highly polytypic species such as dogs must be considered to be numerous different "species" because of the existence of so many reasonably discrete clusters. Sibling species must be considered a single "species" because they form a single cluster. At the other extreme, the Hennigians (commonly termed "cladists") concentrate solely on the splitting of phylogenetic lineages regardless of phenetic similarity. Polytypic species are single species because they form a single clade; sibling species are separate species because they form more than one clade. The evolutionists, represented by Simpson and Mayr, argue that somehow the two considerations must be balanced against each other.

However, on the historical entity interpretation, similarity is a red herring. It is not the issue at all. What really matters is how many organisms are involved and how much the internal organization of the species involved is disrupted. If speciation takes place when a small, peripheral isolate succeeds in bringing about a genetic revolution (see *Figure 1d*), then the parent species can still be said to persist unchanged. It has not lost significant numbers of organisms, nor has its internal organization been affected much. One Hennigian, at least, has come to this conclusion for precisely these reasons ([60]). If, however, the species is split into two or more relatively large subgroups, then it is difficult to see how the ancestral species can still be said to exist, unless one of these subgroups succeeds in retaining the same organization and internal cohesion of the ancestral species. Incidentally, it would also be phenetically similar to the ancestral species, but that would be irrelevant.

Fusion can also take place at the level of species. The breaking down of reproductive isolation sufficient to permit two entire species to merge into one is extremely unlikely (see *Figure 2a*). If it did

occur, the consideration would be the same as those raised in connection with *Figure 1b*. However, introgression and speciation by polyploidy are common (see *Figures 2b* and *2c*). In such cases, a few organisms belonging to separate species mate and produce fertile offspring. Contrary to popular opinion, the production of an occasional fertile hybrid is not enough for biologists to consider two species one. What matters is how extensive the introgression becomes—exactly the right consideration if species are historical entities. As Dobzhansky remarks, “What matters is not whether hybrids can be obtained but whether the Mendelian populations do or do not exchange genes, and if they do whether at a rate which destroys the adaptive equilibrium of the populations concerned” ([9], p. 586).

One final parallel between organisms and species warrants mentioning. Organisms are unique. When an organism ceases to exist, numerically that same organism cannot come into existence again. For example, if a baby were born today who was identical in every respect to Adolf Hitler, including genetic makeup, he still would not be Adolf Hitler. He would be as distinct and separate a human being as ever existed because of his unique “insertion into history,” to use Vendler’s propitious phrase ([58]; see also [57]). But the same observation can be made with respect to species. If a species evolved which was identical to a species of extinct pterodactyl save origin, it would still be a new, distinct species. Darwin himself notes, “When a species has once disappeared from the face of the earth, we have reason to believe that the same identical form never reappears” ([7], p. 313). Darwin presents this point as if it were a contingent state of affairs, when actually it is conceptual. Species are segments of the phylogenetic tree. Once a segment is terminated, it cannot reappear somewhere else in the phylogenetic tree. As Griffiths observes, the “reference of an individual to a species is determined by its parentage, not by any morphological attribute” ([15], p. 102).

If species were actually spatiotemporally unrestricted classes, this state of affairs would be strange. If all atoms with atomic number 79 ceased to exist, gold would cease to exist, although a slot would remain open in the periodic table. Later when atoms with the appropriate atomic number were generated, they would be atoms of gold regardless of their origins. But in the typical case, to *be* a horse one must be *born* of horse. Obviously, whether one is a gradualist or saltationist, there must have been instances in which non-horses (or borderline horses) gave rise to horses. The operative term is still “gave rise to.” But what of the science fiction examples so beloved to philosophers? What if a scientist made a creature from scratch identical in every respect to a human being including consciousness,

emotionality, a feeling of personhood, etc.? Wouldn't it be included in *Homo sapiens*? It all depends. If all the scientist did was to make such a creature and then destroy it, it was never part of our species. However, if it proceeded to mate with human beings born in the usual way and to produce offspring, introducing its genes into the human gene pool, then it would become part of our species. The criterion is precisely the same one used in cases of introgression. In the evolutionary world view, unlike the Aristotelian world view, an organism can change its species while remaining numerically the same individual (see [19]).

One might complain that being born of human beings and/or mating with human beings are biological criteria, possibly good enough for individuating *Homo sapiens*, but inadequate for the humanistic notion of a human being. We are a social species. An entity which played the role of a human being in a society would *be* a "human being," even if it was not born of human beings or failed to mate with human beings. I'm not sure how one makes such decisions, but the conclusion is not totally incompatible with the position being presented in this paper. Species as they are commonly thought of are not the only things which evolve. Higher levels of organization also exist. Entities can belong to the same cultural system or ecosystem without belonging to the same biological species. As Eugene Odum has put it, "A human being, for example, is not only a hierarchical system composed of organs, cells, enzyme systems, and genes as subsystems, but is also a component of supra-individual hierarchical systems such as populations, cultural systems, and ecosystems" ([44], p. 1289). If pets or computers function as human beings, then from certain perspectives they might well count as human beings even though they are not included in the biological species *Homo sapiens*.

4. Biological and Philosophical Consequences. Empirical evidence is usually too malleable to be very decisive in conceptual revolutions. The observation of stellar parallax, the evolution of new species right before our eyes, the red shift, etc. are the sorts of things which are pointed to as empirical reasons for accepting new scientific theories. However, all reasonable people had accepted the relevant theories in the absence of such observations. Initial acceptance of fundamentally new ideas leans more heavily on the increased coherence which the view brings to our general world picture. If the conceptual shift from species being classes to species being historical entities is to be successful, it must eliminate longstanding anomalies both within and about biology. In this section, I set out some of the implications of viewing species as historical entities, beginning with those that are most strictly biological and gradually working my way toward

those that are more philosophical in nature.

The role of type specimens in biological systematics puzzles philosophers and biologists alike. As R. A. Crowson remarks, "The current convention that a single specimen, the Holotype, is the only satisfactory basic criterion for a species would be difficult to justify logically on any theory but Special Creation" ([5], p.29). According to all three codes of biological nomenclature, a particular organism, part of an organism, or trace of an organism is selected as the type specimen for each species. In addition, each genus must have its type species, and so on. Whatever else one does with this type and for whatever reasons, the name goes with the type.⁴ The puzzling aspect of the type method on the class interpretation is that the type need not be typical. In fact, it can be a monster. The following discussion by J. M. Schopf is representative:

It has been emphasized repeatedly, for the benefit of plant taxonomists, at least, that the nomenclatural type (holotype) of a species is not to be confused or implicated in anyone's concept of what is "typical" for a taxon. A nomenclatural type is simply *the specimen*, or other element, with which a name is permanently associated. This element need not be "typical" in any sense; for organisms with a complicated life cycle, it is obvious that no single specimen could physically represent all the important characteristics, much less could it be taken to show many features near the mean of their range of variation (see also [6], [39], [50], [51]). ([49], p. 1043)

Species are polymorphic. Should the type specimen for *Homo sapiens*, for instance, be male or female? Species are also polytypic. What skin color, blood type, etc. should the type specimen for *Homo sapiens* have? Given the sort of variability characteristic of biological species, no one specimen could possibly be "typical" in even a statistical sense ([37], p. 369). On the class interpretation, one would expect at the very least for a type specimen to have many or most of the more important traits characteristic of its species ([16], p. 565-56), but on the historical entity interpretation, no such similarity

⁴The three major codes of biological nomenclature are the International Code of Botanical Nomenclature, 1966, International Bureau for Plant Taxonomy and Nomenclature, Utrecht; the International Code of Nomenclature of Bacteria, 1966, *International Journal of Systematic Bacteriology*, 16:459-490; and the *International Code of Zoological Nomenclature*, 1964, International Trust for Zoological Nomenclature, London. In special circumstances the priority rule is waived, usually because the earlier name is discovered only long after a later name has become firmly and widely established.

is required. Just as a heart, kidneys and lungs are included in the same organism because they are part of the same ontogenetic whole, parents and their progeny are included in the same species because they are part of the same genealogical nexus, no matter how much they might differ phenotypically. The part/whole relation does not require similarity.

A taxonomist in the field sees a specimen of what he takes to be a new species. It may be the only specimen available or else perhaps one of a small sample which he gathers. The taxonomist could not possibly select a typical specimen, even if the notion made any sense, because he has not begun to study the full range of the species' variation. He selects a specimen, any specimen, and names it. Thereafter, if he turns out to have been the first to name the species of which this specimen is part, that name will remain firmly attached to that species. A taxon has the name it has *in virtue of* the naming ceremony, not *in virtue of* any trait or traits it might have. If the way in which taxa are named sounds familiar, it should. It is the same way in which people are baptized.⁵ They are named in the same way because they are the same sort of thing—historical entities (see Ghiselin [13], [14]).

But what then is the role of all those traits which taxonomists include in their monographs? For example, Article 13 of the Zoological Code of Nomenclature states that any name introduced after 1930 must be accompanied by a statement that “purports to give characteristics differentiating the taxon.” Taxonomists distinguish between descriptions and diagnoses. A description is a lengthy characterization of the taxon, including reference to characteristics which are easily recognizable and comparable, to known variability within a population and from population to population, to various morphs, and to traits which can help in distinguishing sibling species. A diagnosis is a much shorter and selective list of traits chosen primarily to help differentiate a taxon from its nearest neighbors of the same rank. As important as the traits listed in diagnoses and descriptions may be for a variety

⁵Although the position on the names of taxa argued in this paper might sound as if it supported S. Kripke's ([26]) analysis of general terms, it does not. Taxa names are very much like “rigid designators,” as they should be if taxa are historical entities. However, Kripke's analysis is controversial because it applies to *general* terms. It is instructive to note that, during the extensive discussion of the applicability of Kripke's notion of a rigid designator to such terms as “tiger,” no one saw fit to see how those scientists most intimately concerned actually designated tigers. According to Putnam's principle of the linguistic division of labor ([47]), they should have. If they had, they would have found rules explicitly formulated in the various codes of nomenclature which were in perfect accord with Kripke's analysis—but for the wrong reason. That no one bothered tells us something about the foundations of conceptual analysis.

of purposes, they are not definitions. Organisms could possess these traits and not be included in the taxon; conversely, organisms could lack one or more of these traits and be clear cut instances of the taxon. They are, as the name implies, *descriptions*. As descriptions, they change through time as the entities which they describe change. Right now all specimens of *Cygnus olor* are white. No doubt the type specimen of this species of swan is also white. However, if a black variety were to arise, *Cygnus olor* would not on that account become a new species. Even if this variety were to become predominant, this species would remain the same species and the white type specimen would remain the type specimen. The species description would change but that is all. Organisms are not included in the same species *because* they are similar to the type specimen or to each other but *because* they are part of the same chunk of the genealogical nexus (Ghiselin [13], [14]).

On the class interpretation, the role of particular organisms as type specimens is anomalous. The role of lower taxa as types for higher taxa is even more anomalous. On the class interpretation, organisms are members of their taxa, while lower taxa are included in higher taxa ([3]). How could entities of two such decidedly different logical types play the same role? But on the historical entity interpretation, both organisms and taxa are of the same logical type. Just as organisms are part of their species, lower taxa are part of higher taxa. Once again, parts do not have to be similar, let alone typical, to be part of the same whole.

A second consequence of treating species as historical entities concerns the nature of biological laws. If species are actually spatio-temporally unrestricted classes, then they are the sorts of things which can function in laws. "All swans are white," if true, might be a law of nature, and generations of philosophers have treated it as such. If statements of the form "species *X* has the property *Y*" were actually laws of nature, one might rightly expect biologists to be disturbed when they are proven false. To the contrary, biologists expect exceptions to exist. At any one time, a particular percentage of a species of crows will be non-black. No one expects this percentage to be universal or to remain fixed. Species may be classes, but they are not very important classes because their names function in no scientific laws. Given the traditional analyses of scientific laws, statements which refer to particular species do not count as scientific laws, as they should not if species are spatiotemporally localized individuals ([20], [21]).

Hence, if biologists expect to find any evolutionary laws, they must look at levels of organization higher than particular taxa.

Formulations of evolutionary theory will no more make explicit reference to *Bos bos* than celestial mechanics will refer to Mars. Predictions about these entities should be derivable from the appropriate theories but no uneliminable reference can be made to them. In point of fact, no purported evolutionary laws refer to particular species. One example of such a law is the claim that in diploid sexually-reproducing organisms, homozygotes are more specialized in their adaptive properties than heterozygotes ([31], p. 397). Evolutionary theory deals with the rise of individual homeostasis as an evolutionary mode, the waxings and wanings of sexuality, the constancy or variability of extinction rates, and so on. People are dismayed to discover that evolutionists can make no specific predictions about the future of humankind *qua* humankind. Since that's all they are interested in, they conclude that evolutionary theory is not good for much. But dismissing evolutionary theory because it cannot be used to predict the percentage of people who will have blue eyes in the year 2000 is as misbegotten as dismissing celestial mechanics because it cannot be used to predict the physical make-up of Mars. Neither theory is designed to make such predictions.

The commonest objection raised by philosophers against evolutionary theory is that its subject matter—living creatures—are spatio-temporally localized ([52], [53]; see also [42]). They exist here on earth and nowhere else. Even if the earth were the only place where life had arisen (and that is unlikely), this fact would not count in the least against the spatiotemporally unrestricted character of evolutionary theory. "Hitler" refers to a particular organism, a spatio-temporally localized individual. As such, Hitler is unique. But organisms are not. Things which biologists would recognize as organisms could develop (and probably have developed) elsewhere in the universe. "*Homo sapiens*" refers to a particular species, a spatio-temporally localized individual. As such, it is unique. But species are not. Things which biologists would recognize as species could develop (and probably have developed) elsewhere in the universe. Evolutionary theory refers explicitly to organisms and species, not to Hitler and *Homo sapiens* (see [43], [48]).

One advantage to biologists of the historical entity interpretation of species is that it frees them of any necessity of looking for any lawlike regularities at the level of particular species. Both "Richard Nixon has hair" and "most swans are white" may be true, but they are hardly laws of nature. It forces them to look for evolutionary laws at higher levels of analysis, at the level of *kinds* of species. It also can explain certain prevalent anomalies in philosophy. From the beginning a completely satisfactory explication of the notion of

a natural kind has eluded philosophers. One explanation for this failure is that the traditional examples of natural kinds were a mixed lot. The three commonest examples of natural kinds in the philosophical literature have been geometric figures, biological species and the physical elements. By now, it should be clear that all three are very different sorts of things. No wonder a general analysis, applicable equally to all of them, has eluded us.

Some of the implications of treating species as historical entities are more philosophical in nature. For example, one of Ludwig Wittgenstein's most famous (or infamous) contributions to philosophy is that of family resemblances, a notion which itself has a family resemblance to cluster concepts and multivariate analysis ([64]). Such notions have found their most fertile ground in ethics, aesthetics and the social sciences. Hence, critics have been able to claim that defining a word in terms of statistical covariation of traits merely results from ignorance and informality of context. If and when these areas become more rigorous, cluster concepts will give way to concepts defined in the traditional way. The names of biological species have been the chief counter-example to these objections. Not only are the methods of contemporary taxonomists rigorous, explicit, objective, etc., but also good reasons can be given for the claim that the names of species can never be defined in classical terms. They are inherently cluster concepts ([18]). On the analysis presented in this paper, advocates of cluster analysis lose their best example of a class term which is, nevertheless, a cluster concept. If "*Homo sapiens*" is or is not a cluster concept, it will be for the same reason that "Moses" is or (more likely) is not.

A second philosophical consequence of treating species as historical entities concerns the nature of scientific theories. Most contemporary philosophers view scientific theories as atemporal conceptual objects. A theory is a timeless set of axioms and that is that. Anyone who formulates a theory consisting of a particular set of axioms has formulated that theory period. Theories in this sense cannot change through time. Any change results in a new theory. Even if one decides to get reasonable and allow for some variation in axioms, one still must judge two versions of a theory to be versions of the "same" theory because of similarity of axioms. Actual causal connections are irrelevant. However, several philosophers have suggested that science might profitably be studied as an "evolutionary" phenomenon ([4], [21], [27], [28], [29], [45], [46], [56]). If one takes these claims seriously and accepts the analysis of biological species presented in this paper, then it follows that whatever conceptual entities are supposed to be analogous to species must also be historical entities.

Theories seem to be the most likely analog to species. Because biological species cannot be characterized intelligibly in terms of timeless essences, it follows that theories can have no essences either. Like species, theories must be individuated in terms of some sort of descent and cohesiveness, not similarity.

The relative roles of similarity and descent in individuating scientific theories goes a long way in explaining the continuing battle between historians and philosophers of science. Philosophers individuate theories in terms of a set (or at least a cluster) of axioms. Historians tend to pay more attention to actual influence. For example, we all talk about contemporary Mendelian genetics. If theories are to be individuated in terms of a single set (or even cluster) of axioms, it is difficult to see the justification of such an appellation. Mendel's paper contained three statements which he took to be basic. Two of these statements were rapidly abandoned at the turn of the century when Mendel's so-called "laws" were rediscovered. The third has been modified since. If overlap in substantive claims is what makes two formulations versions of the "same" theory, then it is difficult to see the justification for interpreting all the various things which have gone under the title of "Mendelian genetics" versions of the same theory. Similar observations are appropriate for other theories as well, including Darwin's theory of evolution. The theory which was widely accepted in Darwin's day differed markedly from the one he originally set out. Modern theories of evolution differ from his just as markedly. Yet, some are "Darwinian" and others not.

When presented with comparable problems, biologists resort to the type specimen. One organism is selected as the type. Any organism related to it in the appropriate ways belongs to its species, regardless of how aberrant the type specimen might turn out to be or how dissimilar other organisms may be. Males and females belong to the same species even though they might not look anything like each other. A soldier termite belongs in the same species with its fertile congeners even though it cannot mate with them. One possible interpretation of Kuhn's notion of an exemplar ([27]) is that it is designed to function as a type specimen. Even though scientific change is extremely complicated and at times diffuse, one still might be able to designate particular theories by reference to "concrete problem-solutions," as long as one realizes that these exemplars have a temporal index and need not be in any sense typical.⁶ Viewing theories as

⁶Kuhn himself ([28]) discusses taxa names such as "*Cygnus olor*" and the biological type specimen. Unfortunately, he thinks swans are swans because of the distribution of such traits as the color of feathers.

sets (or clusters) of axioms does considerable damage to our intuitions about scientific theories. On this interpretation, most examples of scientific theories degenerate into unrelated formulations. Viewing scientific theories as historical entities also results in significant departures from our usual modes of conception. Perhaps scientific theories really cannot be interpreted as historical entities. If so, then this is just one more way in which conceptual evolution differs from biological evolution. The more that these disanalogies accumulate the more doubtful the entire analogy becomes.

Finally, and most controversially, treating species as historical entities has certain implications for those sciences which are limited to the study of single species. For instance, if enough scientists were interested, one might devote an entire science to the study of *Orycteropus afer*, the African aardvark. Students of aardvarkology might discover all sorts of truths about aardvarks: that it is nocturnal, eats ants and termites, gives birth to its young alive, etc. Because aardvarks are highly monotypic, aardvarkologists might be able to discover sets of traits possessed by all and only extant aardvarks. But could they discover the essence of aardvarks, the traits which aardvarks must have necessarily to be aardvarks? Could there be scientific laws which govern aardvarks necessarily and exclusively? When these questions are asked of aardvarks or any other non-human species, they sound frivolous, but they are exactly the questions which students of human nature treat with utmost seriousness. What is human nature and its laws?

Early in the history of learning theory, Edward L. Thorndike ([55]) claimed that learning performance in fishes, chickens, cats, dogs and monkeys differed only quantitatively, not qualitatively. Recent work tends to contradict his claim ([1]). Regardless of who is right, why does it make a difference? Learning, like any other trait, has evolved. It may be universally distributed among all species of animals or limited to a few. It may be present in all organisms included in the same species or distributed less than universally. In either case, it may have evolved once or several times. If "learning" is defined in terms of its unique origin, if all instances of learning must be evolutionarily homologous, then "learning" is limited by definition to one segment of the phylogenetic tree. Any regularities which one discovers are necessarily descriptive. If, on the other hand, "learning" is defined so that it can apply to any organism (or machine) which behaves in appropriate ways, then it *may* be limited to one segment of the phylogenetic tree. It *need* not be. Any regularities which one discovers are at least candidates for laws of learning. What matters is whether the principles are generalizable. Learning may be species

specific, but if learning theory is to be a genuine scientific theory, it cannot be limited *necessarily* to a single species the way that Freud's and Piaget's theories seem to be. As important as descriptions are in science, they are not theories.

If species are interpreted as historical entities, then particular organisms belong in a particular species because they are part of that genealogical nexus, not because they possess any essential traits. No species has an essence in this sense. Hence there is no such thing as human nature. There may be characteristics which all and only extant human beings possess, but this state of affairs is contingent, depending on the current evolutionary state of *Homo sapiens*. Just as not all crows are black (even potentially), it may well be the case that not all people are rational (even potentially). On the historical entity interpretation, retarded people are just as much instances of *Homo sapiens* as are their brighter congeners. The same can be said for women, blacks, homosexuals and human fetuses. Some people may be incapable of speaking or understanding a genuine language; perhaps bees can. It makes no difference. Bees and people remain biologically distinct species. On other, non-biological interpretations of the human species, problems arise (and have arisen) with all of the groups mentioned. Possibly women and blacks are human beings but do not "participate fully" in human nature. Homosexuals, retardates and fetuses are somehow less than human. And if bees use language, then it seems we run the danger of considering them human. The biological interpretation has much to say in its favor, even from the humanistic point of view.

REFERENCES

- [1] Bitterman, M. E. "The Comparative Analysis of Learning." *Science*. 188(1975): 699-709.
- [2] Boyden, A. "The Significance of Asexual Reproduction." *Systematic Zoology*. 3(1954): 26-37.
- [3] Buck, R. C., and D. L. Hull. "The Logical Structure of the Linnaean Hierarchy." *Systematic Zoology*. 15(1966): 97-111.
- [4] Burian, R. M. "More than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science." *Philosophy of Science*. 44(1977): 1-42.
- [5] Crowson, R. A. *Classification and Biology*. New York: Atherton Press, 1970.
- [6] Davis, P. H., and V. H. Heywood. *Principles of Angiosperm Taxonomy*. Princeton: Van Nostrand, 1963.
- [7] Darwin, C. *On the Origin of Species*. Cambridge, Mass.: Harvard University Press, 1966.
- [8] Dawkins, R. *The Selfish Gene*. New York and Oxford: Oxford University Press, 1976.
- [9] Dobzhansky, T. "Mendelian Populations and their Evolution." In L. C. Dunn (ed.) *Genetics in the 20th Century*. New York: Macmillan, 1951. pp. 573-589.
- [10] Ehrlich, P. R., and P. H. Raven. "Differentiation of Populations." *Science* 165(1969): 1228-1231.
- [11] Eldredge, N., and S. J. Gould. "Punctuated Equilibria: An Alternative to Phyletic

- Gradualism." In T. J. M. Schopf (ed.) *Models in Paleobiology*. San Francisco: Freeman, Cooper and Company. pp. 82–115.
- [12] Ghiselin, M. T. "On Psychologism in the Logic of Taxonomic Controversies." *Systematic Zoology*. 15(1966): 207–215.
- [13] Ghiselin, M. T. *The Triumph of the Darwinian Method*. Berkeley and London: University of California Press, 1969.
- [14] Ghiselin, M. T. "A Radical Solution to the Species Problem." *Systematic Zoology*. 23(1974): 536–544.
- [15] Griffiths, G. C. D. "On the Foundations of Biological Systematics." *Acta Biotheoretica*. 23(1974): 85–131.
- [16] Heise, H., and M. P. Starr. "Nomenifers: Are They Christened or Classified?" *Systematic Zoology*. 17(1968): 458–467.
- [17] Hennig, W. *Phylogenetic Systematics*. Urbana, Illinois: University of Illinois Press, 1966.
- [18] Hull, D. L. "The Effect of Essentialism on Taxonomy." *The British Journal for the Philosophy of Science*. 15(1965): 314–326; 16(1966): 1–18.
- [19] Hull, D. L. "The Conflict between Spontaneous Generation and Aristotle's Metaphysics." *Proceedings of the Seventh Inter-American Congress of Philosophy*. Québec City: Les Presses de l'Université Laval. 2(1968): 245–250.
- [20] Hull, D. L. *Philosophy of Biological Science*. Englewood Cliffs: Prentice-Hall, 1974.
- [21] Hull, D. L. "Central Subjects and Historical Narratives." *History and Theory*. 14(1975): 253–274.
- [22] Hull, D. L. "Are Species Really Individuals?" *Systematic Zoology*. 25(1976): 174–191.
- [23] Hull, D. L. "The Ontological Status of Biological Species." In R. Butts and J. Hintikka (eds.) *Boston Studies in the Philosophy of Science*, vol. 32. Dordrecht: D. Reidel, 1976. pp. 347–358.
- [24] Huxley, T. H. "Biology." *Encyclopedia Britannica* (1889).
- [25] Janzen, Daniel, "What Are Dandelions and Aphids?" *American Naturalist*. 111(1977): 586–589.
- [26] Kripke, S. S. "Naming and Necessity." In D. Davidson and H. Harman (eds.) *Semantics and Natural Language*. Dordrecht, Holland: D. Reidel, 1972. pp. 253–355.
- [27] Kuhn, T. S. *The Structure of Scientific Revolutions*. Chicago: The University of Chicago Press, 2nd ed., 1969.
- [28] Kuhn, T. S. "Second Thoughts on Paradigms." In F. Suppe (ed.) *The Structure of Scientific Theory*. Urbana, Illinois: University of Illinois Press, 1974.
- [29] Laudan, L. *Progress and Its Problems*. Berkeley and London: University of California Press, 1977.
- [30] Levins, R. *Evolution in Changing Environments*. Princeton: Princeton University Press, 1968.
- [31] Lewontin, R. C. "Evolution and the Theory of Games." *Journal of Theoretical Biology*. 1(1961): 382–403.
- [32] Lewontin, R. C. "The Units of Selection." *The Annual Review of Ecology and Systematics*. 1(1970): 1–18
- [33] Lewontin, R. C. *The Genetic Basis of Evolutionary Change*. New York: Columbia University Press, 1974.
- [34] Löther, R. *Die Beherrschung der Mannigfaltigkeit*. Jena: Gustav Fischer, 1972.
- [35] Mayr, E. (ed.) *The Species Problem*. Washington, D.C.: American Association for the Advancement of Science Publication Number 50, 1957.
- [36] Mayr, E. "Isolation as an Evolutionary Factor." *Proceedings of the American Philosophical Society*. 103(1959): 221–230.
- [37] Mayr, E. *Animal Species and Evolution*. Cambridge, Mass.: The Belknap Press of Harvard University Press, 1963.
- [38] Mayr, E. "Is the Species a Class or an Individual?" *Systematic Zoology*. 25(1976): 192.
- [39] Mayr, E., E. G. Linsley and R. L. Usinger. *Methods and Principles of Systematic*

- Zoology*. New York: McGraw-Hill Book Company, 1953.
- [40] Meglitsch, P. A. "On the Nature of Species." *Systematic Zoology*. 3(1954): 49-65.
- [41] Mettler, L. E., and T. G. Gregg. *Population Genetics and Evolution*. Englewood Cliffs: Prentice-Hall, 1969.
- [42] Monod, J. L. "On the Molecular Theory of Evolution." In R. Harré (ed.) *Problems of Scientific Revolution*. Oxford: Clarendon Press, 1975. pp. 11-24.
- [43] Munson, R. "Is Biology a Provincial Science?" *Philosophy of Science*. 42(1975): 428-447.
- [44] Odum, E. P. "The Emergence of Ecology as a New Integrative Discipline." *Science*. 195(1977): 1289-1293.
- [45] Popper, K. R. *Objective Knowledge*. Oxford: Clarendon Press, 1972.
- [46] Popper, K. R. "The Rationality of Scientific Revolutions." In R. Harré (ed.) *Problems of Scientific Revolution*. Oxford: Clarendon Press, 1975. pp. 72-101.
- [47] Putnam, H. "The Meaning of 'Meaning.'" In K. Gunderson (ed.) *Minnesota Studies in the Philosophy of Science*, vii. Minneapolis: University of Minnesota Press, 1974. pp. 131-193.
- [48] Ruse, M. J. *The Philosophy of Biology*. London: Hutchinson University Library, 1973.
- [49] Schopf, J. M. "Emphasis on Holotype." *Science*. 131(1960): 1043.
- [50] Simpson, G. G. "The Principles of Classification and a Classification of Mammals." *Bulletin of the American Museum of Natural History*, Vol. 85, 1945. pp. 1-350.
- [51] Simpson, G. G. *Principles of Animal Taxonomy*. New York: Columbia University Press, 1961.
- [52] Smart, J. J. C. *Philosophy and Scientific Realism*. London: Routledge and Kegan Paul, 1963.
- [53] Smart, J. J. C. *Between Science and Philosophy*. New York: Random House, 1968.
- [54] Sneath, P. H. A. and R. R. Sokal. *Numerical Taxonomy*. San Francisco: W. H. Freeman and Company, 1973.
- [55] Thorndike, E. L. *Animal Intelligence*. New York: Macmillan, 1911.
- [56] Toulmin, S. *Human Understanding*. Princeton: Princeton University Press, 1972.
- [57] Van Fraassen, Bas. "Probabilities and the Problem of Individuation." In S. A. Luckenbach (ed.) *Probabilities, Problems and Paradoxes*. Encino, Cal.: Dickinson Publishing Co., 1972. pp. 121-138.
- [58] Vendler, Z. "On the Possibility of Possible Worlds." *Canadian Journal of Philosophy*. 5(1976): 57-72.
- [59] Wade, M. J. "A Critical Review of the Models of Group Selection." *Quarterly Review of Biology*. 1978, forthcoming.
- [60] Wiley, E. O. "The Evolutionary Species Concept Reconsidered." *Systematic Zoology*, forthcoming.
- [61] Williams, G. C. *Adaption and Natural Selection*. Princeton: Princeton University Press, 1966.
- [62] Wilson, E. O. *Sociobiology: The New Synthesis*. Cambridge, Mass.: The Belknap Press of Harvard University Press, 1975.
- [63] Wynne-Edwards, V. C. *Animal Dispersion in Relation to Social Behaviour*. Edinburgh & London: Oliver & Boyd, 1962.
- [64] Wittgenstein, L. *Philosophical Investigations*. New York: Macmillan, 1953.