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## THE EFFECT OF ESSENTIALISM ON TAXONOMY— TWO THOUSAND YEARS OF STASIS (II) \*

DAVID L. HULL

### 5 *The Species Problem*

THUS far all that has been shown is that with respect to taxa names taxonomists have rejected Aristotelian definition. It has yet to be shown that they have failed to eliminate completely their predisposition for Aristotelian definition and that this failure has been at least in part responsible for the persistence of the species problem. Species will be treated in this paper only from the point of view of phylogenetic taxonomy; that is, from the point of view that classification must have some systematic relationship to phylogeny and that the unit of classification must be the unit of evolution. In order for such a position on the status of 'species' to be justified, not only must an adequate definition of 'species' as an evolutionary unit be given but also the phylogenetic programme itself must be shown to be feasible and significant. Only the former will be attempted in this paper.

G. G. Simpson defines an evolutionary species as 'a lineage (an ancestral-descendant sequence of populations) evolving separately from others and with its own unitary evolutionary role and tendencies'.<sup>1</sup> Unfortunately, Simpson's definition itself does not provide any explicitly formulated criteria for determining exactly how unitary an evolutionary role is unitary enough for species status. He defines roles in terms of niches and niches in terms of whole ways of life. Simpson's definition is important because it brings to the fore one of

\* Part I appeared in the previous number.

<sup>1</sup> Simpson, 1961, p. 153

the basic principles of phylogenetic taxonomy (that the unit of classification must be the unit of evolution), but it does not contribute much toward determining what level of evolutionary unity is to be considered specific. In short it is not 'operational'. The purpose of the following sections will be to provide criteria to implement Simpson's definition.

Dobzhansky provided one such criterion in his biological definition. When a group of organisms which usually reproduce by interbreeding interbreeds consistently and produces reasonably fertile offspring, this group of organisms is as distinct an evolutionary unit as there is in phylogeny. The members of the group are *genetically* affecting each other's phylogenetic development. But the biological definition is not without its faults. Cain summarises the major difficulties confronting the criterion of actual interbreeding as follows: 'The biospecies is a definable concept only if time and allopatric populations are ignored and asexual forms are excluded from consideration'.<sup>1</sup> These are not the *only* difficulties confronting a successful definition of 'species', but they are certainly three of the major ones. If they can be surmounted, the others will be relatively easy to account for.

Before each of these major difficulties is treated, reference must be made to Dobzhansky's second criterion for species status. Dobzhansky was well aware that actual interbreeding applied to only a small percentage of organisms. (Our ability to observe such actual interbreeding directly applies to an even smaller percentage.) To accommodate all other cases (that is, to accommodate a majority of the cases) he introduced the criterion of potential interbreeding. In doing so he adopted a non-traditional form of definition, a disjunctive definition. He did not, however, depart very radically from traditional definition, since the two disjuncts were considered to be the only pertinent conditions. Each was sufficient, the fulfilment of at least one necessary. Dobzhansky's definition has much to recommend it. One of the criteria is at least the basis for a sufficient condition for delineating species as units of evolution. The form of the definition, although not completely adequate, is more appropriate than the form of all previous definitions. Unfortunately, Dobzhansky chose potential interbreeding as his second criterion.

If it had not other faults, the criterion of potential interbreeding would be undesirable on the grounds of vagueness alone. It is intended to cover too many too varied exceptions to the first criterion.

<sup>1</sup> Cain, 1954, p. 24

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In one sense a population of mice on one island is potentially interbreeding with a population of mice on another island. They would interbreed if they could get at each other. In a second sense a population of *Drosophila* living in 1942 is potentially interbreeding with a population of that species living in 1922 or 1962. If they had lived at the same time in the same area, they would have interbred. In still another sense, two interfertile sibling species which are both sympatric and synchronic are potentially interbreeding. They could interbreed if they were so inclined. In another sense all the breeds of domestic dogs are potentially interbreeding. They would (and do) interbreed whenever their masters permit. The list could be extended indefinitely.

There is, however, a more fundamental philosophical reason for abandoning potential interbreeding as a criterion for species status. Bertrand Russell says, for example:

The concept of potentiality is convenient in some connections, provided it is so used that we can translate our statements into a form in which the concept is absent. 'A block of marble is a potential statue' means 'from a block of marble, by suitable acts, a statue is produced'. But when potentiality is used as a fundamental and irreducible concept it always conceals confusion of thought. Aristotle's use of it is one of the bad points in his system.<sup>1</sup>

Whether or not such a blanket indictment of the concept of potentiality is completely justified, there are reasons peculiar to evolutionary taxonomy for avoiding it. What is important in evolution is not which organisms could interbreed but which organisms do interbreed. The fact that two groups of organisms *cannot* interbreed (regardless of the isolating mechanism) is important only in the respect that it follows deductively that they *are not* interbreeding. On the other hand the fact that two groups of organisms can or could interbreed even though they are not so interbreeding (regardless of how this is to be ascertained) is important in only two respects. First, it means that the two groups of organisms have not diverged appreciably from each other in interbreeding habits since they had a common ancestry which was actually interbreeding. Second, it means that if in the future the isolating mechanisms are removed, then the two groups would interbreed. But taxonomists are not obliged to predict the future course of evolution. Taxonomists are obliged to classify only those species that have evolved given the environment that did pertain, not to classify all possible species that might have evolved in some possible

<sup>1</sup> Bertrand Russell, *A History of Western Philosophy*, New York, 1945, p. 167

environment. Until potentially interbreeding organisms actually use this potentiality, it is of only 'potential' interest in classification. In evolutionary taxonomy *unrealised* potentialities don't count.

### A. *Allopatric Populations*

Synchronic populations can be geographically separated in two ways. Either they can be separated but connected by intermediate populations or else they can be completely isolated from each other. Chains of contiguous or overlapping populations are termed geographic *Rassenkreise*. Such chains may vary with respect to any property or type of property, but the two most important kinds of *Rassenkreise* are those that vary with respect to morphological similarity and those that vary with respect to interbreeding. Douglas Gasking defines a morphological species as all those forms that are serially very like each other<sup>1</sup> and a biological species as all those populations that are serially crossable with each other.<sup>2</sup> A serial relation is a relation such that the simple relation holds between any two consecutive terms in the series but need not hold between any two terms that are not consecutive.

For example in a morphological *Rassenkreise*, population *A* may be very like population *B*, which is very like population *C*, which is very like population *D*, and so on, but *A* and a distant population, say, *G* may not be very like each other at all. Such continuous variation among contemporary populations presents no problem for the biological definition because interbreeding status is the criterion for species status among contemporary populations, not morphological similarity. However, interbreeding *Rassenkreise* do present a minor difficulty. In some cases population *A* is interbreeding with population *B*, *B* with *C*, *C* with *D* and so on; but not only is it the case that *A* is not interbreeding with a distant member of the chain, but also it is sometimes the case that they are not even interfertile. Nevertheless, genes are exchanged via intermediate populations. All members of the interbreeding *Rassenkreise* belong to the same 'gene pool' and are evolving with a sufficient degree of separateness and unity to be classed as species. They are genetically affecting each other's evolutionary development.

The second respect in which two synchronic populations can be separated geographically is by complete isolation. In such cases the populations are neither interbreeding nor serially interbreeding, and

<sup>1</sup> Douglas Gasking, 'Clusters', *Australasian Review of Psychology*, 1960, 38, 13, 18

<sup>2</sup> Gasking, 1960, p. 38

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yet taxonomists class these isolated populations in the same species if they are morphologically similar to each other and distinct from other species; that is, if they possess a degree of morphological similarity and difference usually indicative of species status in organisms of that type. The degree of morphological similarity and difference usually indicative of species status is determined by the degree of morphological similarity and difference present among contemporary interbreeding species.

The question of greatest interest to the biologist is how good morphological similarity and difference is at indicating species status if species are to be units of evolution. Although this is primarily an empirical question and can be answered only after extensive empirical investigation, its solution has been hindered by logical and philosophical confusions. The phylogeneticists themselves have been responsible for some of the confusion by treating consistent interbreeding with the production of fertile offspring as both a necessary and a sufficient condition when it is obviously only sufficient. For example, true sibling species have been extremely troublesome to the phylogeneticists. From a practical standpoint, taxonomists would like to treat them as single species although they do not interbreed even when given every opportunity. On the other hand, from a theoretical point of view, classing them as a single species seems to run counter to the biological definition.<sup>1</sup> Classing two groups of organisms which are morphologically and ecologically quite similar as a single species although they never interbreed conflicts with the biological definition only if interbreeding is mistakenly considered to be a necessary condition. As Simpson has pointed out, a pair of sibling species which are distinguishable neither morphologically nor ecologically are evolving as an evolutionary unit—albeit without the aid of genetic interchange.<sup>2</sup> As will be seen shortly, the same can be said for species of asexual organisms.

A second confusion must be laid at the doorstep of the opponents of phylogenetic taxonomy. Although the point has been made often and well, it bears repeating: it does not follow from the fact that morphological similarity and difference are used as the evidence from which species status is inferred that the morphological definition of 'species' has been substituted for the biological definition. Morphological similarity and difference is only the *evidence* being used to

<sup>1</sup> Mayr (1957), p. 376; note Mayr, 1942, p. 200, for his treating interbreeding as if it were a necessary condition at least with respect to sibling species.

<sup>2</sup> Simpson, 1961, p. 160

determine species status. How similar is similar enough is determined by the criterion of interbreeding and *this* is what is logically important.

Finally, A. J. Cain voices a common complaint against inferring species status from morphological similarity when he says, 'We can determine by observation the specific or merely varietal status of partially or completely sympatric forms, but can only guess at the status of wholly allopatric ones'.<sup>1</sup> Several factors contribute to this view, including such basic philosophical problems as the justification of induction and the role of deduction in science. The inferences taxonomists make from morphological similarity to species status are not readily put into a form which lends itself to easy manipulation within any of the various theories of probability which have presently been devised. This fact says more against the present development of probability theory than it does against such scientific inferences. Most of the inferences scientists make cannot be accounted for in probability theory. It might also be fruitfully mentioned that the most scathing critics of inferring that two populations belong to the same species from morphological similarity advocate a comparable inference that two different instances of a property are instances of the same property.<sup>2</sup>

The importance of justifying the inference from morphological similarity to phylogenetic relationship should not be underestimated. If it is not justified in a good percentage of the cases, then the entire phylogenetic programme becomes untenable since in most cases morphological properties are *all* the taxonomist has to go on. But before such a harsh judgment is passed, one should make sure that phylogenetic taxonomy is not being measured against an unrealistic standard. On the one hand, science is not as empirical as many scientists seem to think it is. Unobserved and even unobservable entities play an important part in it. Science is not just the making of observations: it is the making of inferences on the basis of observations within the framework of a theory. On the other hand, most of the inferences made by scientists are not deductions as many logicians and philosophers seem to think they are. All inferences made by scientists need not match the accuracy possible in certain restricted areas of physics to be justified.<sup>3</sup> Inductive inferences are not deductively certain.

<sup>1</sup> Cain, 1954, p. 73

<sup>2</sup> For example, Sokal and Sneath, 1963. Either both types of inference are justified or else neither is.

<sup>3</sup> Michael J. Scriven, 'The Key Property of Physical Laws—Inaccuracy', H. Feigl and G. Maxwell (eds.), *Current Issues in the Philosophy of Science*, New York, 1961

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If the inferences made by phylogeneticists are guessing, then so are those of meteorologists, economists, historians, pollsters and so on. According to the definition of 'guessing' implied by Cain's comment, most of what is known as science, including all of the social sciences, becomes guessing. Perhaps the scepticism with which Cain and others view inference from morphological similarity to species status arises from the confusion wrought by the criterion of potential interbreeding. If the status which taxonomists are to determine for wholly allopatric forms is whether or not they are potentially interbreeding, then perhaps the critics are justified in terming such inference 'guessing'. If taxonomists are expected to predict the future development of the organisms being classified, then they are guessing in the strictest sense of the word, since both gene and evolutionary theory are not predictive but retrodictive theories. If on the other hand taxonomists are expected only to infer what has actually happened, what species have actually evolved, what groups were actually interbreeding, then these inferences are well outside the range of guessing.

In any case the purpose of this paper is to present a *type* of definition appropriate for evolutionary taxonomy. Even if the opponents of phylogenetic taxonomy can show that phylogenetic relationships cannot be inferred with reasonable accuracy from the type and extent of evidence that the phylogeneticists have at their disposal, this fact will have no bearing on the *logical* assertion which is the thesis of this paper that Aristotelian definition in terms of a set of necessary and sufficient conditions is inadequate for defining 'species' if species are to be the units of evolution.

### *B. Allochronic Populations*

Taxonomists are unanimous in their opinion that temporal isolation presents a more serious problem for the biological definition of 'species' than geographic isolation presents. A. J. Cain says, for example:

The palaeospecies is an expression of the attempted imposition of a hierarchy developed for classifying discrete groups, on to a continuous evolutionary series. Because of the imperfections of the fossil record many fossils do fall into morphologically discrete groups and can readily be incorporated into the hierarchy. Nevertheless, the whole concept of the species as a morphologically (and by implication genetically) discrete group is based upon the observation of present-day animals, and holds only for short periods of time which on the evolutionary scale are mere instants.<sup>1</sup>

<sup>1</sup>Cam, 1954, p. 123

Evolving lineages form what might be called temporal *Rassenkreise* both with respect to morphological similarity and with respect to interbreeding. Each successive generation in a progressively evolving lineage changes only slightly. Each generation is serially very like all of its ancestral generations. Similarly, each generation of interbreeding forms is serially interbreeding with all of its ancestral generations. Thus, 'species' cannot be defined in terms of a serial relation unless a temporal restriction is imposed on pain of classifying all organisms in a single species.

Morphological *Rassenkreise* among contemporary forms presented no problems for the biological definition because interbreeding status could be determined directly. In temporally separated populations, however, whether there is or is not a complete fossil record, interbreeding status cannot be determined directly. Yet phylogeneticists must find some way to divide progressively evolving lineages into evolutionary units. In the case of lineages broken by fossil gaps, the task is easy. In the case of lineages for which there is a reasonably complete fossil record, the task is not so easy. Even so, there is a solution to the problem of dividing progressively evolving lineages objectively and non-arbitrarily, and the key to the solution is again interbreeding.

The importance of interbreeding for determining species status has been emphasised time and again, but the extent of the criterion's significance has not been fully appreciated even by its strongest proponents. Mayr, using a suggestive metaphor, compares the biological definition to a 'yardstick' for determining species status, and he rightly maintains that division by such a yardstick is both objective and non-arbitrary.<sup>1</sup> Further, he sees that what difficulties there are concerning the biological definition are not 'with the yardstick but with its application'.<sup>2</sup> A further development of Mayr's spatial analogy comparable to Simpson's analogy of a string gradually changing colour proves quite enlightening in the determination of the roles of interbreeding and morphological similarity and difference and placing them in their proper perspective.

The paradigm case of objective measurement is the measurement of objects in space or of the distances between spatially separated objects. Space as such is an amorphous continuum with no intrinsic metric.<sup>3</sup> What this means is that there is nothing about space itself to indicate

<sup>1</sup> Mayr, 1957, pp. 15, 16, 18, 19

<sup>2</sup> Mayr, 1959, p. 375

<sup>3</sup> Adolf Grünbaum, *Philosophical Problems of Space and Time*, New York, 1963

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how long to make the unit of measurement or at what point to begin measuring once the unit of length has been chosen. With respect to objects in space, the choice of the unit of length is primarily a matter of convenience. Bacteria are measured in microns, rugs in feet (or yards or metres), roads in miles or kilometres, and interstellar distances in light years. One highly advantageous property for a system of measurement is to have the units commensurable. A mile can be divided evenly into yards, feet, or inches. At what point we begin to measure is a matter of convenience. We could begin to measure a rug by laying down our ruler anywhere on it, although it is most convenient to begin at one corner and measure one edge. Nothing about space itself dictates either the length of the unit of measurement or the point at which to begin measuring, but it does dictate what type of unit is appropriate—a spatial unit.

Comparable points can be made about what might be called 'taxonomic space', but it must be emphasised that everything that is said in terms of taxonomic space is meant merely to be explanatory and suggestive. Talk of taxonomic space, like talk of gene pools, is strictly metaphorical. Before either metaphor can actually function in taxonomic theory, a rigorous development in exclusively taxonomic terms is required. Taxonomic space is constructed by plotting 'morphological distance' on the horizontal axis and development in time on the other. 'Morphological distance' means morphological similarity and difference. Taxonomic space, like physical space, is an amorphous continuum with no intrinsic metric; there is nothing about taxonomic space to indicate how long to make the unit of measurement or at what point to begin measuring once the unit is chosen.

With respect to the objects in taxonomic space, the choice of the unit of length is neither arbitrary nor a matter of choice for the phylogeneticists. (This is not the case for the classificationists and their numerical progeny.) The unit of taxonomic space is the morphological distance usually indicative of interbreeding status among contemporary organisms which usually reproduce by interbreeding. However, interbreeding is not the unit of taxonomic space; interbreeding merely determines the length of the unit of taxonomic space. In terms of Mayr's metaphor, the yardstick is morphological distance. Interbreeding determines how long a yard of morphological space is. Nor is the choice of the point at which to begin measuring lineages arbitrary or purely a matter of convenience. In the case of lineages

which terminate in contemporary species, it is certainly most convenient to begin laying down our yardstick with them; but there are also theoretical reasons to begin with contemporary species, for only with them can we check the accuracy of our yardstick. The purpose of the yardstick is to delineate evolutionary units. The rationale for making the yardstick one length rather than another is that for the group of organisms in question a particular morphological distance is usually indicative of interbreeding status, which is indicative that the group is evolving as a unit and is, hence, rightly called a species. The nature of taxonomic space as we constructed it determines what type of unit is appropriate for measuring lineages—morphological distance. In the case of lineages which are entirely extinct, measurement begins at the point of extinction or with the most recent fossil. These points may or may not coincide. In the case of lineages which are known only by isolated groups of fossils, the length of the object to be measured is usually less than the length of the yardstick so no division is necessary.

One rather disconcerting property of both physical and taxonomic space, which has been referred to only tangentially, is that fact that the length of the unit of measurement varies. Just as spatially extended objects determine the contours of physical space, evolutionary lineages determine the topography of taxonomic space. To stretch the analogy to its breaking point, taxonomic space is 'non-euclidean'. The vertical temporal axis is divided evenly into segments, but the horizontal axis which indicates morphological distance is not. The units of morphological distance vary in length depending on what morphological distance is indicative of species status for the contemporary members of the general type of organism. Some organisms interbreed even if there is great morphological dissimilarity; others permit almost none. If the properties under consideration are weighted, still another dimension of complexity is added. The logic of the situation remains unchanged, however.

Taxonomists use the yardstick of morphological distance to indicate species status among contemporary allopatric populations. There is no reason why its use cannot be extended to allochronic or even to allochronic and allopatric forms.<sup>1</sup> Although Simpson feels that the resulting divisions will be arbitrary, he condones chopping a progressively evolving lineage into segments by use of morphological distance. He goes on to say, however, 'In practice all that is needed is some criterion as to how large (and in what sense "large") to make the

<sup>1</sup> Mayr, 1957, p. 120, and Cain, 1954, p. 111

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segments. . . . Successive species should be so defined as to make the morphological differences between them at least as great as sequential differences among contemporaneous species of the same or closely allied groups'.<sup>1</sup> In progressively evolving lineages there is no sudden discontinuity which can be used as a specific boundary, just as there is none in a synchronic morphological *Rassenkreis*; but there is a reason in both cases to make a break in the continuous series at one point rather than at another.

### C. Asexual Populations<sup>2</sup>

Taxonomists universally agree (if universal agreement among taxonomists is possible) that organisms which never reproduce by interbreeding, whether contemporary or ancestral, present an insurmountable difficulty for 'the biological definition which no amount of modification could accommodate. The following quotations are typical:

In summary, the existence of the species as an objective biological unit is not impaired by morphological indistinctness or by the continuity of the evolutionary process. The loss of sexuality, on the other hand, removes the very foundations on which the species exists as a type of breeding population. As a result biological species do not exist in asexual groups.<sup>3</sup>

The essence of the biological species concept is discontinuity due to reproductive isolation. Without sexuality this concept cannot be applied. Asexuality then is the most formidable and most fundamental obstacle of a biological species concept.<sup>4</sup>

The agamospecies represents an advance on the morphospecies, since the mode of reproduction is known. Unfortunately, it is a mode which allows no possibility of framing a definition of the species which is any less arbitrary than the morphospecies.<sup>5</sup>

In the case of asexual forms, taxonomists feel they have come to an impasse in applying the yardstick they have devised to determine

<sup>1</sup> Simpson, 1961, p. 165

<sup>2</sup> Mayr disapproves of calling groups of asexual organisms 'populations'. Simpson has no reservations concerning the term. For example, see his evolutionary definition of 'species' as quoted in this paper.

<sup>3</sup> Verne Grant, 'The plant species in theory and practice', *The Species Problem*, Washington, 1957, p. 61

<sup>4</sup> Mayr, 1957, p. 379

<sup>5</sup> Cain, 1954, p. 123

species status, for it is logically impossible for a non-interbreeding species to be interbreeding (not to *become* interbreeding but to *be* interbreeding). What they have neglected is that the yardstick is not interbreeding status but the degree of morphological distance usually indicative of interbreeding status. When a taxonomist says that a certain asexual species possesses a degree of morphological similarity and difference comparable to that of contemporary interbreeding species, he is not saying something that is logically impossible, although it may be empirically false. Mayr says, 'It is possible to use the same kind of inference to classify asexual organisms into species. Those asexual individuals are included in a single species that display no more morphological difference from each other than do conspecific individuals in related sexual species'.<sup>1</sup>

Although T. M. Sonneborn wants to retain the word 'species' to refer to the unit of identification and coins the word 'syngen' to refer to the unit of evolution as determined by the biological definition, he says, 'The question of whether asexual equivalents of syngens exist and can be recognized is more difficult, but not hopeless. The key to progress in this direction is to recognize in the syngen of sexual organisms a distinction between the means of ascertainment and that which is ascertained'.<sup>2</sup> He goes on to say in more detail:

The preceding attempt to generalize the biological species or syngen runs counter to the view of proponents of the biological species concept . . . that biological species do not exist among obligatory inbreeders or asexual organisms. This denial, as indicated above, is based on an operational definition of biological species. Since the operation, testing gene flow, is impossible in asexual organisms, they deny the existence in them of the thing this operation discovers in sexual organisms, i.e., the biological species or syngen. Their statement of the situation thus implies an abrupt change in the organization of nature and in the units of evolutionary divergence correlated with an abrupt change from outbreeding to obligatory inbreeding and asexual reproduction. By subordinating the method of ascertainment to the thing ascertained and by seeking methods of ascertainment in asexual reproduction, the concept of biological species or syngens was generalized.

<sup>1</sup> Mayr, 1957, p. 381. Happily organisms which reproduce sexually are scattered throughout the animal kingdom from Protozoa to Chordata enabling taxonomists to establish the morphological yardstick without making unreasonable inductive leaps.

<sup>2</sup> T. M. Sonneborn, 'Breeding Systems, reproductive methods, and species problems in Protozoa', *The Species Problem*, Washington, 1957, p. 290

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This implies the absence of an abrupt change in the organization of nature and in the units of evolutionary divergence with changes in breeding systems or method of reproduction.

No such abrupt change is in fact found in the present review of conditions in Protozoa.<sup>1</sup>

### *E. A Disjunctive Definition of 'Species'*

If the yardstick used to determine species status among allochronic and allopatric interbreeding species is accurate enough to indicate evolutionary units, and if it can be extended to apply to asexual species without any great loss in accuracy, then it would seem that the phylogeneticists at least are in a position to formulate a definition of 'species' adequate for their purposes. 'Species' could be defined *disjunctively* as populations that:

- (1) consistently interbreed producing a reasonably large proportion of reasonably fertile offspring, or
- (2) consistently serially interbreed with synchronic populations producing a reasonably large proportion of reasonably fertile offspring, or
- (3) do not fulfil either of the first two conditions but have not diverged appreciably from a common ancestry which did fulfil one of them, or
- (4) do not fulfil any of the first three conditions because they do not apply but are analogous to populations which do fulfil at least one of the first three conditions.

Conditions (1) and (2) are quite straightforward and require no application of the yardstick of morphological distance. In fact they are the basis for determining the length of the morphological yardstick. However, they apply only to contemporary sympatric populations of organisms which usually reproduce by interbreeding. Condition (3) applies only to populations which usually reproduce by interbreeding, but there are no temporal or spatial restrictions. How much divergence is 'appreciable' is determined by the morphological yardstick. Condition (4) applies only to non-interbreeding populations with no temporal or spatial restrictions. How analogous the populations have to be is determined again by the morphological yardstick.

As in the case of all disjunctive definitions, the fulfilling of *any one* of the conditions is sufficient and the fulfilling of *at least one* is necessary. Like the disjunctive definitions of taxa names, it is not insulated against

<sup>1</sup> T. M. Sonneborn, 1957, p. 296

empirical considerations, both because the objects in taxonomic space determine the length of the morphological yardstick and because new methods of reproduction might arise or be discovered. For example, some asexual forms exhibit a phenomenon called 'parasexuality', in which all or some of the genetic material of one individual is transferred to another. If it is found that these non-meiotic mechanisms of gene flow are sufficient to ensure evolutionary unity and separateness, then either an additional condition would have to be added to the disjunctive definition of 'species' or else the meaning of 'interbreeding' would have to be expanded to encompass parasexuality. The former is the more probable move.

However, instead of taxonomists concluding that 'species' can be defined, albeit disjunctively, they have concluded just the opposite. 'Species' cannot be defined because there are several *kinds* of species. Mayr says, for example, 'Two facts emerge from these and other classifications. One is that there is more than one species concept and that it is futile to search for *the* species concept'.<sup>1</sup> The reasons which taxonomists give for refusing to accept a disjunctive definition of 'species' and the moves they make to avoid defining it disjunctively are exactly the reasons that the traditionalists give and the moves they make for avoiding disjunctive definition of *any* term. According to the advocates of traditional Aristotelian definition, whenever a word can be defined only disjunctively, either one or more conditions must be accepted as necessary and sufficient or else the fuzzy concept must be divided into terms that can be defined traditionally. The first alternative is not open to phylogeneticists given evolutionary development as it is. Sonneborn outlines the possibilities given the second alternative.

- (i) Apply the word 'species' to all the various kinds of species, including species as the unit of identification and as the unit of evolution.
- (ii) Retain the term to apply to the unit of evolution as determined by the biological definition.
- (iii) Continue to use the term to apply to the unit of identification as determined by the morphological definition.<sup>2</sup>

Sonneborn rejects the first alternative 'on the grounds that a technical term should have a single meaning, and the second alternative on the grounds of priority and generality'.<sup>3</sup> Thus, 'species' is to be reserved

<sup>1</sup> Mayr, 1957, p. 10

<sup>2</sup> Sonneborn, 1957, p. 201

<sup>3</sup> *Ibid.*, p. 201

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for the units of identification and groups of organisms which fulfil the requirements of the biological definition are to be termed 'syngens'. V. Grant opts for the second alternative and suggests the term 'binom' for groups of organisms which do not fulfil the requirements of the biological definition.<sup>1</sup> Cain suggests adopting either (i) or (ii). He says, 'One can either restrict the name to one meaning (presumably the biological species) or use it with appropriate prefixes for all'.<sup>2</sup> Regardless of how it may sound, these biologists are *not* arguing about words. 'Species' has no magic power, and they are well aware of it. What they are arguing about is whether the unit of identification or the unit of evolution will be the unit of classification and, thus, will remain intact as higher taxa are constructed. In order for the purposes of phylogenetic taxonomy to be fulfilled, the unit of evolution must remain intact.<sup>3</sup>

As is usually the case with disjunctive definitions, taxonomists feel two opposing pulls. On the one hand, since several alternative conditions are each sufficient, they feel that several different terms are being defined. Thus, they conclude that there are several different kinds of species. On the other hand, these different kinds of species seem to function with amazing similarity in both evolutionary and gene theory. Almost everything that a biologist would want to say about one kind of species, he would want to say about the other, with the exception of statements directly pertaining to their defining criteria. Thus, G. G. Simpson says, 'The evolution of uniparental and biparental populations is different in many important ways. That does not alter the fact that both form species and, by appropriate definition, the same kind of species'.<sup>4</sup>

Such a situation is not unique to taxonomic terms. For example, since some grapefruit have yellow, sour flesh and others have pink, sweet flesh, the fuzzy concept 'grapefruit' could be replaced by two precise concepts . . . 'Florida-grapefruit' and 'Texas-grapefruit'. But in most contexts in which the word 'grapefruit' is used, everything that is said about one kind of grapefruit is true of the other. In other contexts the distinction between the two slightly different fruits might be important and the distinction could be made. However, 'grapefruit' cannot be defined traditionally so that it is impossible to make a true statement about grapefruit in general which is false with respect to some individual grapefruit.

<sup>1</sup> Grant, 1957, pp. 46, 61

<sup>2</sup> Cain, 1954, p. 106

<sup>3</sup> Hull, 1964, pp. 1-11

<sup>4</sup> Simpson, 1961, p. 163

An example which is more like 'species' is the word 'dishonesty'. There are at least three different criteria for dishonesty—lying, cheating, and stealing. The fulfilment of any one of these criteria is sufficient for proper application of the predicate 'dishonest'. If someone lies, he is being dishonest. If someone cheats, he is being dishonest. If someone steals, he is being dishonest. But it does not follow that if someone performs a dishonest act, he has stolen. He may have lied or cheated or both. Thus, a traditionalist would say that there are three kinds of dishonesty and want to abandon the word 'dishonesty' for three more precise words. In a sense there are three kinds of dishonesty, but it is not a sense in which the term 'dishonest' is fuzzy, meaningless, or indefinable. M. J. Scriven says of this example:

There are cases of special interest here where we are led to apply the same term even when no natural cluster exists; the classic example is that of dishonesty, where we may discover that lying, cheating and stealing, the three main criteria, are not correlated with each other. This does not destroy the social utility of the term, though it may impair its value for personality-theory.<sup>1</sup>

There are several criteria for determining species as evolutionary units. Several criteria are required because evolutionary development is extended in time and space, because numerous factors affect this development, and because there are various ways in which species reproduce themselves. This does not destroy the general utility of the term 'species' in biology, though it may impair its value in particular areas.

To some the 'solution' to the species problem presented in this paper may seem no solution at all (which means it's not the kind of solution they had in mind). Instead it might seem merely philosophical sleight of hand. All that is necessary to solve any problem in definition is to trot out a disjunctive definition. Two examples in which this procedure cannot be used may help to dispel this illusion of fakery. In the early days of genetics, genes were held to be the units of heredity. For each gross trait such as eye colour there was supposedly a discrete particle which controlled it. When a radical change occurred in a trait, a gene mutation was postulated as the cause. As genetics developed 'gene' was also defined operationally as the smallest unit of recombination. It was assumed that the smallest units of heredity, mutation, action, and crossing over were discrete units and one and the same unit. However, as larger and larger numbers were taken into

<sup>1</sup> Scriven, 1959, p. 867

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consideration as more conveniently studied organisms were investigated, the limits of what had been envisaged as a gene were passed right through. The progression stopped only at the molecular level with the nucleotide pair (in the case of DNA), and no one wanted to call a pair of nucleotides a 'gene' in its original sense.

In place of the classic gene several new units have been introduced. The smallest unit of mutation is termed the muton and consists of one nucleotide pair. The smallest unit of recombination is termed the recon and is also one nucleotide pair long. The unit of information, the codon, is most probably three nucleotides long. What comes closest to the classic gene is the cistron, the unit of function, which averages about five hundred nucleotide pairs in length. But the product of one cistron is not one trait but one protein. And the whole situation is complicated further by the possible existence of operators which control one or more cistrons, providing a larger natural unit, the operon. In any case nothing approximates the classic gene. This fact presents no serious barrier to the definition of 'gene'. Scientific words often acquire additional or even radically different meaning as science progresses, but in the case of the word 'gene' all but two of the meanings happen to conflict. Almost nothing a geneticist would want to say about a muton or recon would he want to say about a codon, a cistron or an operon, and almost nothing he would want to say about any of the new units would he want to say about the classic gene. The fate of the word 'gene' in modern genetics is that except in general discourse or in the first few chapters of an introductory text (and its use here is debatable), its function has entirely evaporated. Using such a truly ambiguous term like 'gene' in intermediate or advanced discourse can lead to nothing but confusion.

'Protoplasm' is an earlier example. It has already passed out of scientific discourse. Not too many years ago protoplasm was thought to be the stuff of life. If only its complex structure could be analysed, the secret of life would be revealed. As it turned out, it was primarily the nucleic acid constituents (a relatively simple molecule) that were of importance in heredity. Perhaps 'protoplasm' could be defined now that there is so much knowledge of the molecular make-up of the cell, but it is doubtful whether any scientist would take the time to try since the concept of protoplasm is no longer of any use in biology. Its original function has been entirely usurped by other concepts.

The fate of 'gene' and 'protoplasm' has not befallen 'species'. On appropriate definition it is not ambiguous, and it is still an important

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concept in biology. Thus, the defining of it disjunctively is both possible and justified. The definition of 'species' presented in this paper may prove inadequate for the purposes of evolutionary taxonomy, but its formulation will have shown at least in what respects a definition of its kind is superior to the typical Aristotelian definitions of 'species' thus far offered in the literature.

### *6 Conclusion*

It is commonly held that Darwin shattered the essentialist or static concept of species. Darwin shattered something all right, but it was only the first two tenets of essentialism, the ontological and methodological assertions. Darwin did not alter taxonomists' predisposition for Aristotelian definitions, either for species names or for 'species'. Contemporary taxonomists have finally abandoned attempts to define taxa names traditionally, but they still refuse to accept any but an Aristotelian definition of 'species'. To be sure, there are many objections to abandoning the clear-cut analytic-synthetic distinction which the adoption of cluster concept analysis implies, not the least of these being that it has broad philosophical ramifications; but taxonomists have already committed themselves to the necessity of defining taxa names as cluster concepts. No new logical or philosophical obstacles stand in the way of their extending this practice to include 'species'.

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