CERTAINTY AND CIRCULARITY IN EVOLUTIONARY TAXONOMY

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After a considerable period of uncritical acceptance, the principles and procedures of evolutionary taxonomy have been subjected to careful scrutiny and, none too surprisingly, have been found not completely adequate or totally free from vagueness and ambiguity. One of the more serious criticisms currently popular is that evolutionary reasoning is inherently circular. For example, Robert R. Sokal and P. H. A. Sneath (1963) say:

> "In recent years three comprehensive analytic studies of systematic principles have been published in books by Hennig (1950), Remane (1956), and Simpson (1961). . . .
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> "All three authors mentioned above are fully aware of the dilemma of circular reasoning inherent in systematic procedure. They are not satisfied with solutions based on 'groping.' Simpson (1961) thinks that taxonomy is an evolutionary science, and he attempts to outline a series of phylogenetic principles on the basis of which taxonomic evidence should be examined to yield evolutionary interpretations and classifications. We shall examine these principles in detail in Chapter 8. However, Simpson nowhere in his book is able to present a logical and consistent defense of the circularity of reasoning inherent in such procedures. By calling the process of classification an art, rather than a science, he defines the problem out of existence.
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> "Hennig (1950) describes the dilemma in even greater detail. He defends the circularity of reasoning by the 'method of reciprocal illumination.' By this he means that some light is thrown from one source of logical illumination onto a natural situation kindling another, brighter light in the latter, which in turn will throw added illumination onto the first source. Thus, in a self-reinforcing, positive feedback type of analysis, the relationships under study are eventually clarified. Hennig feels that phylogenetic relationships are the entity of systematics whose parts consist of morphological, ecological, physiological, and zoogeographic similarities. Each of these parts mirrors phylogenetic relationships, which are to be investigated by the method of reciprocal illumination. But we cannot see how the principle of reciprocal illumination differs from the much-condemned vertical construction of hypothesis upon hypothesis.
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> "Remane (1956), in spite of his fundamentally phylogenetic orientation, has also realized that phylogenetic reasoning cannot serve as the basis for erecting a natural system. He similarly rejects affinity (based on a few characters) as the basis of a natural classification. He considers that while both of these approaches enter on occasion into the techniques practiced by 'good systematists,' the exclusive application of only one of them is likely to lead to misclassification. Affinity or resemblance when based on one or a few characters can lead the systematist astray, Remane claims, as he would be too easily deceived by chance convergences resulting from poor sampling of the characters. Remane attempts escape from the *circulus vitiosus* by basing his taxonomy on non-phylogenetic criteria of homology."

The charge of vicious circularity has been common throughout the history of taxonomy. For example, Darwin (1859) accused his predecessors and contemporaries of arguing in a circle when they claimed that important organs never vary and decided which organs were important by which did not vary. Sachs (1890) repeated the charge and directed it specifically at Linnaeus. It was only a matter of time until the evolutionists themselves were accused of reasoning in vicious circles. Thompson (1952) seems to have been the first to do so. The charge, however, has been made most often and most forcefully by A. J. Cain. Initially it was directed against Cuvier; Darwin was expressly exempted (1959a). Cuvier classified living organisms by means of properties ordered according to their presumed physiological importance. Cain alleges that Cuvier decided which properties were physiologically important by observing which characters were constant in a classification already constructed according to overall similarity, making phenetic classification fundamental. Darwin, on the other hand, proposed that living organisms be classified by means of properties ordered according to their presumed phyletic importance. In principle which properties are phyletically important and, hence, indicative of evolutionary descent can be discovered independently of the constancy of characters in any pre-established classification. Cain (1962a) summarizes his views as follows:

"By equating taxonomic with ancestral relationship, Darwin, like Cuvier, adopted two criteria of the importance of characters. On the one hand, he said that those characters common to large groups (i.e. *natural* groups, although he did not say so explicitly) are more important than those common to groups containing little diversity. This is always right if only natural groups are to be made. But he also said (a) that those least likely to have been modified in relation to particular modes of life will be more important in showing ancestral affinity, and (b) that they can be recognized because they will be the most constant ones within a natural group. But this assumes that natural groups are always phyletic or, in other terms, that convergence is never so great as to obscure or outweigh ancestral resemblance even in poorly known groups. He did not commit the earlier error of arguing in a circle. His principles of evolutionary importance were not derived from a pre-existing taxonomy, but from the results of artificial selection and from the study of heredity, variation and ecology. This is a point worth emphasizing. In many elementary textbooks of biology, classification is treated as one of the lines of evidence for evolution. Darwin did not treat it thus; he discussed it quite late in the '*Origin*' as consonant with the theory of evolution, and explicable as a consequence of it. He never regarded it as primary evidence for evolution, and his caution was certainly justified."[1]

But later Cain brings the charge of vicious circularity full circle. In principle phyletic development can be discerned without recourse to the constancy of properties in a phenetic classification but too often in practice it cannot. Cain (1962b) concludes:

"If there were any method of analysing animals so that the construction and behavior of each could be shown to follow from a few great principles, then ideally each form could be expressed in terms of these principles in such a way as to define it and its properties completely. Something of this sort has happened in the classification of the elements by their atomic structure. Such a taxonomy of analyzed entities has been attempted many times. In Linnaeus's period and before, logical analysis, plus Aristotelian physiology, was thought to help in this. About the beginning of the nineteenth century, physiological criteria of what must be the most important characters were widely used. But in both periods there was far too little information for such analytical taxonomy to be attempted, and workers were in fact arguing in a circle from the observed constancy of certain characters in already recognized 'natural groups' to their physiological importance. Darwin rightly rejected such attempts, but he tried to recognize which characters were more likely to remain constant during evolution and to use them as the best indicators of ancestry—and his criterion again, in the absence of a really good fossil record, could only be constancy within 'natural groups.' "[2]

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[1] Darwin discussed the hierarchical arrangement of taxa quite early in the *Origin* (1859: 128) and treated it as he treated all evidence for evolution, as consonant with his theory. The group subordinate to group relation evident in nature was just one more fact inexplicable on the special creation view but to be expected on his view. Cain is right, however, that he did not treat classification as primary evidence for evolution, as say George J. Romanes (1892) was to do later. As he did with so many of the arguments put forth to justify evolutionary theory, Fleeming Jenkin (1897) found a hole in this one. Initial *success* in a rough hierarchical classification might be evidence for evolution, but just the opposite should be the case under continued efforts. He argues cogently (and in agreement with Darwin) that evolution by gradual change entails difficulty, not success, in classification. Hence, if anything, continued *difficulty* in classification should be considered evidence for evolution.

[2] Cain's distinction between classifications of analyzed and unanalyzed entities reflects an acceptance of the Aristotelian view of the relation between mathematics and the empirical sciences which relativity theory has made untenable. The reason that essential definitions in the Aristotelian manner are possible in geometry is that the various geometries are pure deductive systems with no empirical import. Such "entities" as pure Euclidean triangles can be completely analyzed into the species scalene, isosceles and equilateral because there is nothing to analyze but some axioms and definitions. Such is not the case with any of the entities in empirical science. The classification of the physical elements according to their atomic numbers is not in the least like a taxonomy of analyzed entities. One end of the periodic table is open. There are about a hundred elements stable enough to exist for any length of

Sokal and Sneath, and Cain have been quoted at some length to make clear exactly which lines of reasoning common in evolutionary taxonomy are supposed to be viciously circular. According to Sokal and Sneath no logical and consistent defense has been presented for such "circularity in reasoning." It will be the purpose of this paper to provide just such a defense. By the very nature of the accusation, the defense will have to be in terms of logic and logical distinctions. In the first half of the paper, it will be explained what constitutes a logical fallacy and why it is undesirable to reason fallaciously and then several lines of reasoning which have been termed viciously circular will be examined to see if they actually are circular. It will be seen that these lines of reasoning degenerate into vicious circles *only* if certain evolutionary laws and principles are ignored. The justification for ignoring these laws and principles is that they are not warranted. The second half of the paper will deal with the problem of what in general makes an inductive inference warranted and specifically whether those inferences used in evolutionary reconstructions are warranted.

Logical Fallacies

Since evolutionary procedure is accused of being circular and reasoning in vicious circles is a logical fallacy, it is certainly worthwhile to explain what logical fallacies are and why it is undesirable to commit them. The general statement that a logical fallacy is any erroneous process of reasoning or arguing is not very helpful. What is important are the reasons for considering arguments erroneous. They are of two types. Logicians are mainly concerned with *formal fallacies*, those lines of reasoning which are erroneous solely because of the form of the propositions and arguments, regardless of the subject matter. For example, it is fallacious to argue that since Communists disapprove of abstract art, anyone who disapproves of abstract art is a Communist. A person need know nothing of either Communism or abstract art to decide that this argument is invalid. The fallacy of reasoning in vicious circles does *not* belong to this class of fallacies. Instead it is an example of what logicians call a *material fallacy*. In diagnosing material fallacies both content and the use to which the argument is being put play central roles. An argument can fulfill all the requirements of formal validity and still be fallacious if it fails to perform the task it is intended to perform. One of the commonest uses of arguments is to prove conclusions that are in some way unknown or doubtful or that have been called into question. A prerequisite for fulfilling this purpose is that the argument cannot assume in the premises what it proposes to prove. An argument which fails to prove anything because it somehow takes for granted what it is supposed to prove is called begging the question or *petitio principii*. More subtle versions of this fallacy are often singled out and termed vicious circles.

An argument can presuppose or depend upon its conclusion in two ways. In the simplest case the dependence is straightforwardly logical. One of the premises is just a restatement of the conclusion so that anyone doubtful of the truth of the conclusion would have to be equally doubtful of the truth of the premises. For example, any proposition can be deduced validly from itself, but we have not thereby *proved* anything. It is formally correct to conclude that the Pope is infallible when he speaks *ex cathedra* from the premises that

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time under ordinary conditions; there are three and only three species of triangles on the differentia implied. Further, the superficial neatness of the periodic table is destroyed by the existence of isotopes and isomers. No such borderline cases can occur in a pure deductive system like Euclidean geometry. The entities classified in the periodic table have been more thoroughly analyzed than have most of the entities classified in the Linnaean hierarchy, but there is not the slightest hope of ever completely analyzing either of them, in the sense in which the "entities" of pure geometry can be completely analyzed.

he said so *ex cathedra* and everything that the Pope says *ex cathedra* is true, but such an argument would still be fallacious.

The dependence needn't be logical, however. Often the circle is epistemological. The only way that you could *know* that the premises are true would be to *know* that the conclusion is true. All cases of deduction from genuine enumerative generalizations (generalizations arrived at by complete enumeration) are examples of arguing in vicious circles. For example, it is formally correct to argue that a certain ball in a jar is red because all of the balls in the jar are red, but such an argument would be viciously circular if the generalization had been established *only* through the examination of each of the balls in the jar. The only way that you could know that all of the balls were red would be by knowing that the ball in question was red. However, arguing that *Tamias striatus* evolved because all species evolved is *not* an example of a vicious circle. The generalization was not arrived at simply by enumerative induction. Some species were examined to establish the truth of the generalization to be sure and many have been examined since, but the acceptance of the generalization rests primarily on the explanatory power of evolutionary theory.

Homology and Phylogeny

There are three lines of reasoning common in evolutionary taxonomy which are often singled out as circular: (a) the defining of "homology" in terms of phylogeny and then using characters claimed to be homologous to infer phylogeny, (b) the basing of an evolutionary classification on a phenetically constructed classification, and (c) the inferring of phyletic descent from overall phenetic similarity. The first two lines of reasoning are derivative of the third. They shall be discussed in order. Sokal and Sneath (1963) say with respect to the first:

"Any attempt to decide the phylogeny on one set of characters, in particular those believed to be homologous (derived from a common ancestor, by the common definition of the term), or to decide a priori which characters are important or are reliable guides to phylogeny, soon leads to a tangle of circular arguments from which there is no escape. Even Simpson (1961), who strongly supports a phylogenetically based taxonomy, is aware of and points out the *circulus vitiosus* of this procedure."

Sokal and Sneath's reference to the definition of "homology" in terms of phylogeny might make it sound as if they were arguing that such a definition is circular, when their intent seems to be that the definition can lead to circular arguments. As Ghiselin (1966a and 1966b) has pointed out, the definition of "homology" in terms of phylogeny is not circular because "phylogeny" is not defined in terms of homology. Sokal and Sneath's point is that one is often inferred via the other and that *this* can lead to circular arguments.[3] In another place they make it clear that they have an argument in mind and that the argument is supposed to be epistemologically circular. They say:

"We do not know of any infallible criteria for overall phenetic convergence that may be obtained from a study of living forms of organisms alone. To detect convergence, we have to distinguish those features which do accurately reflect the phylogeny from those features which do not. This, however, is a question which can only be answered by knowing the phylogeny first. The problem therefore is insoluble within this logical framework, and one must have independent evidence (not derived from phenetic relations) in order to attack it."

It is tautological to say that homologous resemblances are indicative of common line of descent, since by definition homologous resemblances are those resemblances due to

[3] The difference between the two assertions is worth pointing out, since there is considerable difference between a definition and an argument. The reasons for the undesirability of circular definitions and circular arguments are, however, the same. If you don't know what the word "explanation" means, it doesn't help to be told that an explanation is something which explains something. Similarly, if you are in doubt of God's existence, the argument that if God is all-good and existent, then He exists won't prove very convincing.

common line of descent. To be sure, any evidence to the effect that a particular resemblance is homologous would necessarily be evidence to the effect that it was due to common line of descent and vice versa. But, the evidence from which phylogeny is inferred is not limited just to homologous resemblances. For example, two taxa which in point of fact are very closely related cladistically could exhibit a striking dissimilarity. Any evidence (e.g., genetic evidence) to the effect that this particular type of dissimilarity can arise quite quickly would contribute to the correct reconstruction of the phylogeny but would not depend on homologies at all. Fossil evidence that it did so arise would be even more conclusive. On the other hand, evidence that the genetic mechanism necessary for such a dissimilarity to arise is such that the change could arise only very gradually would disprove the hypothesis that the two share a recent common ancestry even in the absence of fossil evidence. But too often the only evidence that is readily available is phenetic similarity, and the first line of reasoning which has been called circular merges into the third.

Vicious Circles and Successive Approximation

The second line of reasoning which has been termed circular is spelled out in some detail by Sokal and Sneath (1963) as follows:

"It may be advantageous at this stage to outline an important logical fallacy underlying current taxonomic procedure. It is the self-reinforcing circular arguments used to establish categories [taxa], which on repeated application invest the latter with the appearance of possessing objective and definable reality. This type of reasoning is, of course, not restricted to taxonomy—but it is no less fallacious on that account. Let us illustrate this point. An investigator is faced with a group of similar species. He wishes to show relationships among the members of the group and is looking for characters which will subdivide it into several mutually exclusive taxa. A search for characters reveals that within a subgroup A certain characters appear constant, while varying in an uncorrelated manner in other subgroups. Hence a taxon A is described and defined on the basis of this character complex, say X. It is assumed that taxon A is monophyletic or a 'natural' taxon. Thus every member of A (both known and unknown forms) is expected to possess X; conversely, possession of the character complex X defines A.

"Henceforth group A, as defined by X, assumes a degree of permanence and reality quite out of keeping with the tentative basis on which it was established. Subsequently studied species are compared with A to establish their affinities; they may be within A, close to it, or far from it. It is quite possible that a species not showing X would be excluded from A, although it was closer overall to most of the members of A than some were to each other. It may be said that such problems would arise only when A was an 'artificial' group erected on the basis of 'unsuitable' characters. However, except in long-established taxa or those separated by very wide gaps from their closest relatives, the effect of the last classification carried out with a limited number of characters is quite pervasive. The circular reasoning arises from the fact that the new characters, instead of being evaluated on their own merits, are inevitably prejudiced by the prior erection of taxon A on other characters (X). Such prejudgment ignores the fact that the existence of A as a natural (or 'monophyletic') group defined by character complex X has been *assumed* and not *demonstrated*."

In all due respect, the preceding is not a characterization of the best in evolutionary taxonomy but a parody of the worst. Perhaps the practice of some evolutionists sometimes degenerates to this level, but it need not and if the principles of evolutionary taxonomy are given proper consideration, it should not. Sokal and Sneath are well aware that even the most carefully formulated procedures run the danger of unimaginative application if the principles on which they are based have been imperfectly understood. The principles of evolutionary taxonomy should not be judged any more by their misapplication than should those of numerical taxonomy. There are lines of reasoning in evolutionary taxonomy which come close to being circular. In the preceding quotation Sokal and Sneath have extracted one of the most important of these lines of reasoning, but their description tends to disguise rather than reveal the outlines of this potential circle.

For example, according to their account, after A has been defined in terms of X, any species which has a sufficient number of the properties in X is automatically placed in A (although it might not be similar in other respects to the other species already in A) and any species which lacks a sufficient number of these properties is excluded from A (although it may be similar in other respects to the other species). The classification and reclassification which goes on all the time in evolutionary taxonomy in the light of the discovery of previously unknown species and additional evidence belies this extreme position. However, what is wrong with this procedure, even if evolutionary taxonomists were guilty of it, is not that a self-reinforcing circle is involved but that *no* reinforcement is involved. If anything, the alleged procedure is contradictory. On the one hand, evolutionary taxonomists are supposed to reason that the degree of covariance in X is indicative of phyletic similarity. On the other hand, they are supposed to maintain that in the light of more evidence and a slightly different distribution of characters, that this new complex Y is *not* indicative of phyletic similarity. The whole point of evolutionary procedure is that if X is indicative of phyletic similarity, then Y is *more* indicative.

Now in ordinary discourse such self-reinforcing procedures are often called "vicious circles" if the consequences are undesirable. For example, in a recent issue of a popular magazine it was reported that doctors had "found that the more overweight the diabetic gets, the more insulin there is in his blood. And the more insulin, the more he tends to eat and thus store up more fat in an ever-widening vicious circle." The situation may be vicious, but it is not circular since the diabetic gets fatter and fatter. Similarly, evolutionary taxonomists classify and reclassify in an attempt to represent evolutionary descent with an ever increasing accuracy.[4] This goal may be undesirable. In this sense, the practice is vicious, but it is not circular and, hence, not logically fallaceous. The criticism at issue is whether or not evolutionary taxonomists are making a logical error, not an error in tactics.

As Sokal and Sneath say, the type of reasoning which they have indicated is not restricted to taxonomy. It is inherent in any attempt to obtain objective knowledge, including the efforts of the numerical taxonomists. Abraham Kaplan (1964) has called this special problem the paradox of conceptualization and considers it an existential dilemma:

> "The proper concepts are needed to formulate a good theory, but we need a good theory to arrive at the proper concepts. Long before the scientific revolutions of the twentieth century, Jevons (1892) remarked that 'almost every classification which is proposed in the early stages of a science will be found to break down as the deeper similarities of the objects come to be detected.' Every taxonomy is a provisional and implicit theory (or family of theories). As knowledge of a particular subject-matter grows, our conception of the subject-matter changes; as the concepts become more fitting, we learn more and more. Like all existential dilemmas in science, of which this is an instance, the paradox is resolved by a process of approximation: the better our concepts, the better the theory we can formulate with them, and in turn, the better the concepts available for the next, improved theory. V. F. Lenzen (1938) has spoken explicitly of 'successive definition.' It is only through such successions that the scientist can hope ultimately to achieve success."

There are several possible ways to avoid the dilemma of which Kaplan speaks, none of which have engendered any great enthusiasm among scientists. The easy way is to introduce some metaphysical faculty such as intuition which is capable of directly and infallibly apprehending reality. This was Aristotle's way. At least one contemporary taxonomist still advocates such

[4] The claim that classification is to represent phylogeny is merely the claim that there must be some systematic relationship between phylogeny and some system of classification. The more powerful the system of classification, the more extensive and precise this relationship can be made. Unfortunately, the Linnaean hierarchy is not a very powerful means of classification. See Hull (1965) for further details.

a procedure (Sattler, 1963). Most philosophers and scientists, however, have long since come to the conclusion that this "solution" just won't do for science in general. Cain (1958) and Mayr (1959) have argued quite effectively that it won't do for taxonomy in particular. (See also, Hull, 1965.)

Another alternative is to deny that a classification is intended to approximate anything but that any classification established on objective criteria is "true" in its own right. At times, numerical taxonomists have seemed to be arguing for this position. At other times, however, they maintain that a phenetic classification is to approximate something called phenetic similarity. On this view, the evolutionists and numerical taxonomists share the methodological problem of justifying the process of approximation by successive definition, the process called "groping" by Cain and "reciprocal illumination" by Hennig. This justification is no easy matter, but it should be kept in mind that the problem is one shared by evolutionary and numerical taxonomists equally.[5]

To be sure, any classification based upon a previous tentative classification is inevitably prejudiced by it. Any error in the early classification might well infect all later reclassifications, but the effect of the early classification *decreases* as reclassification takes place in the light of additional evidence. A gradual decrease in error might not sound good enough for some taxonomists, but there is no other alternative short of complete reclassification each time a single taxonomic boundary is re-evaluated. Although Sokal and Sneath have advocated some rather extensive revisions in taxonomic procedure, even they have not gone so far as to suggest the complete dissolution of all classification and reclassification *de novo* each time a single taxon is re-evaluated on the basis of a single bit of additional evidence. Until such a drastic step is taken, early classifications will color later classifications be they phenetic or phyletic.

[5] Sokal and Sneath object only to Hennig's particular use of the process of successive definition, the process which he calls reciprocal illumination, not to the process itself.

Vicious Circles and Empirical Certainty

There remains the primary line of reasoning, which has been termed circular, to defend. Once again we turn to Sokal and Sneath for a vigorous statement of the criticism:

> "The difficulty with the use of the phylogenetic approach in systematics emerged after the first wave of enthusiasm for it subsided and has remained apparent to perceptive observors ever since. *We cannot make use of phylogeny for classification, since in the vast majority of cases phylogenies are unknown.* This is one of the statements most commonly heard at meetings of taxonomists, yet it is most consistently ignored. Let us restate it in other words for emphasis. The theoretical principle of descent with modification —phylogenetics—is clearly responsible for the existence and structure of a natural classification; we may even agree with Tschulok (1922) that the natural system can be considered as proof of the theory of evolution. However, since we have only an infinitesimal portion of phylogenetic history in the fossil record, it is almost impossible to establish natural taxa on a phylogenetic basis. Conversely, it is unsound to derive a definitive phylogeny from a tentative natural classification. We have described this fallacy of circular reasoning earlier." [See also Cain, 1962; Cain and Harrison, 1960; and Bigelow, 1956 and 1958.]

This quotation contains two distinct criticisms of evolutionary taxonomy. The first, which has already been discussed, concerns the erection of a phyletic classification on the basis of a phenetic classification. The effect of the original classification may never be eliminated as reclassification takes place. Since reclassification *does* take place and the later classifications seldom circle back on the original, this procedure is not circular. Definition by successive approximation provides several logical difficulties, but none of these involves vicious circularity. The second criticism is that inferring phyletic relationships for a group in the absence of a really good fossil record *for that group* is unwarranted. In most instances, no fossil record is available.

The only observations which evolutionists have to go on are phenetic properties (usually morphological), and phenetic similarity is exactly the criterion used by classificationists and numerical taxonomists to construct their classifications. If an evolutionary taxonomist *begins* with a phenetic classification, *reclassifies* exclusively on the basis of phenetic similarity, then he will *end up* with a phenetic classification and, perhaps, also a phyletic classification, depending on how good phenetic similarity is at indicating phyletic similarity. If phyletic relationships cannot be inferred from phenetic similarity with sufficient certainty, then the line of reasoning just sketched is unwarranted. It is plainly not circular!

These two criticisms, though distinct, are related. They are distinct, since it is one thing to argue that a line of reasoning is circular; it is another to argue that it is unwarranted. In fact, the two errors are mutually exclusive. No one argument could be both circular and unwarranted. The two criticisms are related, however, since those lines of reasoning which have been termed circular become circular only if certain other lines of reasoning can be shown to be unwarranted. Thus, the really important criticism of evolutionary taxonomy does not concern circularity in classification at all but the justification of inferring phyletic development from phenetic similarity on the basis of certain evolutionary principles in the absence of a fossil record for the group. If a good fossil record for the group in question is necessary before its evolutionary development can be reconstructed with sufficient certainty, then in a vast majority of the cases phylogeny cannot be reconstructed and the evolutionary program in taxonomy becomes untenable. It will be argued that fossil records for all or even a majority of the taxa to be classified are not necessary. All that is required is that there be some really good fossil sequences from which to derive principles concerning the trends and tendencies of evolutionary development and others which can be used later to check them. Further, the evolutionary principles used to infer phyletic development needn't be completely general universal statements. Much less will have to do. Nor do they have to be either verified or verifiable in the early positivistic sense. Partial confirmation will have to do. To demand more of evolutionary taxonomy would be to demand more than any science can deliver—including physics.

The Quest for Absolute Certainty

Philosophers and scientists through the centuries *have* demanded more, but these demands have stemmed from a failure to understand the nature of empirical science. Such "apriorists" as Aristotle awarded the honorific title "science" only to deduction from purely universal generalizations to purely universal generalizations, because such inferences necessarily resulted in true conclusions *if* the premises were true. Aristotle assured the truth of his premises by claiming that we intuit them and intuition is always true! Largely in reaction to the excesses of Aristotelianism, some philosophers and many scientists adopted an extremely empirical view of science in the 18th and 19th centuries. These extreme empiricists or "Baconians" as they called themselves looked upon science as an "inductive process," but they too demanded absolute certainty and attempted to get it by never going beyond the evidence. Scientific laws were just empirical generalizations, just summations of the data. Anything else was mere speculation. Even though Darwin himself claimed to have proceeded in the true Baconian manner, the main criticism leveled against his theory by the biologists of his day was that it was speculation. He had gone beyond his evidence in a manner unbecoming an inductive scientist and an Englishman. (See Ellegard, 1957.)

To a philosopher it sounds strange to hear a neo-Platonist like Richard Owen criticizing Darwin for idle speculation, calling himself an inductive scientist and complimenting Cuvier for not being "the man to draw conclusions beyond his premises" (Owen, 1860). Inductive inferences are just those inferences which *do* go beyond the evidence at hand and which *do* run the risk of leading to false conclusions from true premises. As Peter Caws (1965) put it recently:

> "One might define deduction as a process of inference in which one never goes beyond the given facts, and in which therefore there is never any loss of certainty. Induction, on the other hand, does go beyond the given facts, and therefore runs the risks that deduction does not. We shall find these risks at the root of scientific theory."

Apriorists and extreme empiricists have tried to obtain absolute certainty although by opposite means. Neither of these extreme positions is appropriate for the actual practice of any science, including taxonomy.

During the early years of this century, extreme empiricism in biology became quiescent but it burst forth in psychology under the name of behaviorism. The behaviorists demanded "operational definitions" of all the terms in psychology, definitions of a type which they mistakenly thought were common in physics. Under careful and repeated criticism by philosophers and their fellow psychologists (e.g., Bergmann, 1954 and 1956), and after being abandoned by the founder of operationism, P. W. Bridgman (1954), behaviorists began to modify their extreme position. It has now become little more than an emphasis on a particular form of experiment and a cry for some empirical content in psychological laws. The history of operationism should be an object lesson to those taxonomists who are attempting to introduce it into taxonomy. *If* it is intended as a demand that the basic concepts in taxonomy be operationally defined in the strict sense, then it is untenable. *If* on the other hand, all that is intended is that the definitions and laws in taxonomy be operational (i.e., have some empirical consequences which can be checked), then it is an admirable program but one that is neither new nor startling.

The Language of Logical Criticism

In his early critical writings A. J. Cain exhibits the same contradiction which has always been characteristic of the writings of extreme empiricists. At times he demands absolute certainty of the inferences made by the evolutionists; at other times he seems to be arguing more reasonably that too many of these inferences are not sufficiently warranted. A confusion in the language of logical criticism emphasizes this vacillation. For example, after arguing against the use of deduction in taxonomy (Cain, 1958), the only kind of inference which permits categorical demonstration and apodeictic certainty, Cain (1959a) complains that phyletic inferences are not "apodeictically certain" or "categorically demonstrated." Furthermore, after quoting two Aristotelian philosophers on the impossibility of applying Aristotelian logic to biological classification, Cain (1959) says:

> "The relevance of these quotations from logicians for the whole history of biological taxonomy from Aristotle to the present day can hardly be over-estimated. They epitomize the most important change in taxonomic theory that has occurred, namely the gradual abandoning of attempts to set up classifications on *a priori* principles agreeable to the rules of logic and some particular theory, and the partial substitution of an empirical attitude. This substitution was not complete when the theory of evolution arrived to provide a new theoretical approach to the problem of classifying organisms, the full implications of which have still not been completely thought out. [Taxonomists should] proceed empirically, simply finding out what subjects exist and what are their attributes, not deducing them from known principles and axioms."

Cain terms the principles used by theoretically oriented taxonomists, from Aristotle to the evolutionists, *a priori* and their inferences deductions, when in point of fact the thrust of his objections to evolutionary taxonomy is that the principles are *a posteriori* and the inferences inductions. Cain objects to evolutionary principles because they are supposedly *a priori* and then complains that they do not have certain properties which *only a priori statements can have*. He objects to evolutionary inferences because they are supposedly deductions and then complains that they do not exhibit a type of certainty that *only deductions can have*. Cain finds Aristotelian logic inadequate for the purposes of science, but he turns around and *uses* its basic distinctions to criticize evolutionary taxonomy. If Aristotelian logic won't do for the working taxonomist in his everyday pursuits, then it won't do for the taxonomist when he steps back to evaluate the structure of his science. Terminological confusions like those just mentioned pervade the taxonomic literature, and a reasonable decision as to the justification of evolutionary reconstructions depends upon just those distinctions blurred by these confusions.

The principles used by evolutionists to reconstruct phylogeny are not *a priori* in the philosophical sense of this avowedly philosophic expression. According to accepted philosophic usage, the phrase *a priori* applies only to our knowledge of the truth of the statement although it is often used elliptically to refer to the statement itself. A statement is *a priori* true if its truth can be decided prior to experience. For example, the statement that either it is raining somewhere on the plain in Spain right now or it is not is *a priori* true. Of course, its truth cannot be known prior to all experience, since we must know what the words in the statement mean and we must understand English syntax, but its truth can be decided *without reference to the weather*. If the truth of a statement can be decided *a priori*, verification is irrelevant. Empirical considerations just don't matter. As a consequence, such statements are necessarily true. We can be apodeictically certain of their truth. The scientific principles (as distinct from the logical and mathematical principles) used by the evolutionists to reconstruct phylogeny, like all scientific principles, are not *a priori* in this sense. Hence, they are not necessarily true in and of themselves. At best they can be only highly confirmed or made highly probable.

In addition to this philosophic use of the expression, there is also a use in everyday discourse according to which a belief is termed *a priori* if the person who holds it refuses to entertain counterevidence. For example, the belief of many people that smoking will not increase their chance of contracting lung cancer is *a priori* in this sense. The belief is not *a priori* in the philosophic sense, since evidence *is* relevant. The problem is that the person refuses to acknowledge the relevant evidence. He is certain that his belief is true, but his certainty is not justified. Perhaps some evolutionists have treated their principles as *a priori* in this second sense. If they have, it is unfortunate. But sometimes the appearance of ignoring counterevidence stems from a misconstrual of the logical nature of both evolutionary principles and the inferences made from them. The critics of evolutionary reconstructions complain that even though every single principle used by the evolutionists is known to have exceptions, the evolutionists continue to use them. It might be noted in conjunction with this claim that every single principle in Newtonian mechanics is known to have exceptions, but physicists continue to use them. (See Scriven, 1961 and Kaplan, 1964.)

The misunderstanding is expressed most clearly by R. S. Bigelow (1959) when he argues that a hypothesis to the effect that all the balls in a particular bag are white is not probably true once it is known that one ball in the bag is red; the hypothesis is false. Similarly, the hypothesis that phenetic similarity corresponds to recency of common ancestry is not probably true once it is known that there are numerous exceptions to this rule; it is false. This line of reasoning follows, however, only if the statement relating phenetic similarity and recency of common ancestry is taken to be categorical in form. If it is intended to be a tendency statement (as it certainly is), then a single exception does not falsify it. Even numerous exceptions are permissible. Consequently, if Bigelow still wishes to register an objection to evolutionary reasoning, he is left in the unhappy position of arguing (as some philosophers have argued) that the only principles properly admitted into the pandects of science are true universal generalizations and concluding (as some philosophers have concluded) that since there are almost no such generalizations in biology, biology is almost entirely devoid of scientific laws.

The logic behind such a line of reasoning is interesting. If scientific laws are restricted to true universal generalizations, then deduction to the particular case is possible and these particular conclusions follow necessarily. If the conclusion turns out to be false and the argument is valid, then it follows necessarily that at least one of the premises is false. Hence, universal generalizations are in principle easily falsifiable. Such is not the case with inferences from generalizations which are less than universal in form. Inferences to particular statements are only more or less probable and falsification is not so easy. They are falsifiable, but a single observation won't do it. For example, from the universal generalization that all parents with blue eyes produce only blue-eyed children, it can be deduced with apodeictic certainty that if a certain child is biologically the offspring of these parents, it will have blue eyes.[6] But from the statements that blue-eyed parents almost always produce blue-eyed children, it cannot be deduced that if a certain child is biologically theirs, it will have blue eyes. The inference is only inductive and the conclusion follows from the premises with only a degree of certainty or probability. As G. G. Simpson (1961) has said:

> "Scientists themselves frequently seemed confused as to the degree and indeed also the kinds of 'certainty' (actually always probability) that are required or are possible in science."

The moral of the preceding discussion is that if such empiricists as Cain are taken at their word, then they do not hold an empirical view of empirical certainty. Scientists have a right to say that they are certain of the truth of a particular statement even though they may find out later that the statement was false. Any use of "certainty" which makes it impossible for a scientist to be justifiably certain of the truth of a statement which is later discovered to be false is a view of certainty inappropriate to empirical science.

It would seem unlikely that the founders of numerical taxonomy, a movement which relies so heavily on the use of statistics, should hold a view of inference totally at variance with the foundations of statistics. Yet, at least in his early writings, Cain seems to do just this. In his later works, Cain clearly reveals an empirical notion of certainty, for example, in Cain and Harrison (1960). But in this very paper, the authors state with respect to the recognition of convergence, "But we have no way of estimating these probabilities; even if we did, we should obtain only probabilities. . ." It may well be true that presently there is no way to estimate these probabilities, but to say that even if we did, we should obtain *only* probabilities is to label oneself an extreme empiricist. Probabilities are all that scientists ever have to go on, probabilities which sometimes are so high that they can be termed certainties.

Perhaps statements such as those cited are only due to careless expression; perhaps they reveal a deep-seated antipathy to the kind and degree of certainty possible in empirical science, an antipathy which

[6] It should be noted that as is usually the case, this universal generalization has exceptions and, hence, is not strictly true. One way to eliminate the obvious exceptions is to add the phrase "except in cases of mutations," but if "mutation" is then defined as it once was as any variation from the regularities of inheritance, then the law degenerates to a tautology and is no longer an empirical law. In order to retain its status as an empirical law, the generalization in question must be such that it is at least logically possible for there to be exceptions. If in point of fact, there are none, then it is true.

has been traditionally characteristic of the apriorists. Considering the important contribution which Cain has made to taxonomy by pointing out the dilatory effect of the *a priori* outlook in taxonomy, one hopes it is the former. However, when the point of contention is precisely those issues which the verbal confusion blur, the results are fatal.

Empirical Certainty and Empirical Laws

With such distinctions in mind, we are now in a position to evaluate the evolutionary laws and principles which are at the heart of the charge of vicious circularity. When the critics term the laws and principles of evolutionary theory *a priori*, what they usually have in mind is that evolutionists formulate them from the study of certain groups of organisms on less than total evidence for those groups and then extend them to new groups for which confirmatory observations have not been made and may not even be possible. Hence, with respect to these latter groups, the application of the principles is prior to experience. But in this sense all scientific principles are *a priori*. They go beyond the evidence at hand. They would be useless as principles if they didn't. This is the sense of *a priori* used when weightings given to a character before the taxon has been constructed are termed *a priori* and those assigned after the taxon has been constructed *a posteriori*. It is also the sense of *a priori* used by William Coleman (1964) in his book on Cuvier when he says, "The natural system can be prepared in two ways: 1. a posteriori, by direct observation; 2. a priori, by the principle of the subordination of characters."

How inappropriate this particular usage is in criticizing science can be seen in the following example (for the sake of brevity only one example will be given). Kepler enunciated his laws of planetary motion which were to apply to all the planets in the solar system on the basis of less than a handful of observations made by Tycho Brahe on a single planet, Mars. Later astronomers checked his laws for other planets in the solar system and for other positions of Mars, but they also extended them to cover all planets revolving around all stars. We have little hope of ever verifying or even confirming this hypothesis *by observation* for more than an infinitesimal portion of the star systems in the universe, and yet no one would want to call the inference unwarranted or the hypothesis meaningless.

Evolutionary laws have been devised on the basis of innumerable observations but observations which have been made primarily in restricted areas of the plant and animal kingdoms. Certain portions of phylogeny are as inaccessible as the farthest corners of the universe. These laws have been formulated on less than total evidence, but what is more they have been found to be not completely accurate even for those groups from which they were derived. Even so, evolutionists extrapolate to other groups for which little or no evidence is available. Phenetic similarity has been found to be a fairly good indicator of evolutionary relationships in certain groups. Cases are known, however, in which this is not true. Even so, evolutionists use phenetic similarity to infer phylogeny for all taxa. We have little hope of ever verifying or even confirming the correspondence by observation for more than an infinitesimal portion of taxa that have evolved, and yet the inference is not unwarranted, nor the assertion meaningless. Evolutionists *know* that phyletic similarity is not directly proportional to phenetic similarity in all cases, but it is good enough for their purposes. Astronomers *knew* that Kepler's laws were not completely accurate, but they were close enough for their purposes. What justifies evolutionary inferences is exactly what justifies astronomical inferences—theory. The inductive leap which astronomers made concerning planetary motion is much greater than those evolutionists would ever think of making—and for a very good reason. Evolutionary theory, even combined with modern genetic theory, does not have anything like the power of Newtonian theory. But the justification in both cases remains the same.

The crux of the dispute between the evolutionists and the empiricists is the kind and degree of certainty necessary to justify scientific inferences. Are the inductive inferences made by evolutionists in reconstructing phylogeny sufficiently warranted? The literature is full of arguments for and against the justification of particular phyletic reconstructions; examples of them needn't be reiterated here. The basic problem—and it is a philosophic problem—is the justification of induction. Are inductive inferences in general justified and, if so, which ones? Philosophers have argued these questions at some length and have attempted to formulate calculi to handle inductive inferences without overwhelming success. Neither these arguments nor the calculi need be presented here. There *is* a way to decide the issue but a way which leaves itself open to the charge of vicious circularity. Any decision between the extreme empiricism sometimes advocated by Cain and perhaps other members of the numerical school and more temperate versions of empiricism must rest on the advances of the various sciences using the techniques of discovery and justification which they do use. Hence, induction is justified by an induction! The arguments presented by the empiricists against evolutionary reconstructions if sound would annihilate not just evolutionary taxonomy but all empirical science. No empirical science has ever proceeded in the manner advocated by the empiricists and it seems unlikely that it could.

Conclusion

The purpose of this paper has been to show that the "tangle of circular arguments" referred to in evolutionary taxonomy by Sokal and Sneath, once untangled are not circular at all—perhaps unwarranted but definitely not circular. The criticism that the reasoning of the evolutionists is circular stems from several misunderstandings, the most important concerning the logic of discovery. What Hennig refers to as the method of reciprocal illumination is nothing new in science. Hypotheses are formed on the basis of extremely insufficient evidence and then modified in the face of additional evidence or the discernment of additional relationships in the original evidence. This procedure is not a vertical construction of hypothesis on hypothesis. Prejudice against hypothesizing has been common in science, expressed somewhat cryptically as *hypotheses non fingo* by Newton, one of the greatest hypothesizers of all time. This prejudice arises in part from a justifiable reaction to the excesses of some scientists and philosophers, especially in the scholastic period. But it also seems to stem from an unjustifiable desire to make science not just mistake-free but mistake-proof. One of the most carefully explored avenues in philosophy has been the unsuccessful attempt to reconstruct all experience in sense data language. The chief advantage of such a language is that a simple sense data statement is incorrigible. A person cannot be mistaken about his own sense data. Similarly, some taxonomists want to reformulate the purposes of taxonomy until there is nothing anyone could make a mistake about. Any classification erected according to any objective criterion would be "true."

The important criticism of evolutionary taxonomy does not, however, concern the logic of discovery but the logic of justification. How highly confirmed must scientific hypotheses be to be justified? The criticism has been formulated in both extreme and reasonable versions. W. R. Thompson (1952), although he realizes that the only way to escape the "Idealist prison" is by abandoning the quest for absolute certainty, argues that evolutionary taxonomy is untenable because its hypotheses are not verifiable in the early positivist tradition. This extreme version is reflected in Cain's complaints that the inferences made by evolutionists are not infallible, categorically demonstrated or apodeictically certain. To be sure, evolutionists are not infallible, but it is one of the distinguishing marks of science in contrast to orthodox religion that it does not claim infallibility.

Both of these men have also expressed themselves more reasonably. They have claimed that the inferences made by evolutionists in reconstructing phylogeny are not warranted. Sokal and Sneath are completely justified when they warn that "taxonomists often reason facilely back and forth among these criteria without stopping to think how slender the evidence is on which their arguments are based." A decisive settlement of the dispute between evolutionists and numerical taxonomists on this point is complicated by the lack of a definite criterion of empirical certainty and the fact that phyletic inferences are not yet couched in quantitative terms. All that can be done in the first instance is to compare the degree of certainty present in phyletic reconstructions with the degree of certainty present in other sciences. As far as the standards of probability established for science as a whole are concerned, the inferences made by the evolutionists rate quite highly—even in the absence of fossil evidence. In fact, the previously quoted philosopher, Abraham Kaplan, uses Darwin's line of reasoning which led him to conclude that all domestic pigeons are descended from the rock pigeon *Columba livia* as a paradigm of a warranted inductive inference.[7] It is certainly true that all Darwin had to go on was probabilities, but the weight of evidence was so strong that no reasonable man could reject his conclusion. Who can say that the forelimb of the bat was ever anything but a wing? Well, who can say that the fluoridation of water has no serious side effects, or that smoking is one of the major causes of lung cancer, or that Oswald killed Kennedy? Any view of empirical certainty which makes answers to these questions "guesswork" is unacceptable. Indeed, continued controversy in these areas is a good indication that many people, including some scientists, are confused as to the degree and kind of certainty that is required or is possible in science.

In conjunction with their predisposition for empiricism (and perhaps even extreme empiricism), numerical taxonomists have also advocated making taxonomy more quantitative and objective. Future advances in taxonomy certainly lie in this direction, but the task is a formidable one and no easier for phenetic than phyletic taxonomists. An investigation of the bibliographies of several of the major figures in numerical taxonomy shows that they were working on the problem of making phyletic weightings quantitative immediately before they began to advocate the abandonment of the methodology and purposes of evolutionary taxonomy for the more easily quantifiable methods and less ambitious purposes of numerical taxonomy. Perhaps it was the difficulties which they encountered in attempting to make phyletic weightings quantitative which led them to abandon phyletic weightings and evolutionary taxonomy, and substitute in their stead "equal weightings," one of the basic

[7] Himmelfarb (1959: 274) shows her ignorance of inductive logic when she says, "What Darwin was doing, in effect, was creating a 'logic of possibilities.' Unlike conventional logic, where the compound of possibilities results not in a greater possibility, but in a lesser one, the logic of the *Origin* was one in which possibilities were assumed to add up to probability." Himmelfarb undoubtedly has the law of product in mind. However, this law holds only if the events are independent and those with which Darwin was dealing were not. With respect to what Darwin was doing, Kaplan (1964: 245) says, "The weight of the evidence for or against a hypothesis, however, by no means depends solely on the frequencies themselves. Other hypothesis already established may be brought to bear, providing second-level probabilities concerning the observed frequencies on the assumption that the hypothesis in question is false. By this means, what Reichenbach has called 'concatenations of evidence' are built up; a chain of probable inferences may very well be stronger than its weakest link, stronger even than its strongest."

principles of numerical taxonomy. Later developments in numerical taxonomy have shown that the maneuver was to no avail.

As numerical taxonomists began to develop more sophisticated methods of treating statistical covariance of properties, they came to realize that if they were to measure on various scales and yet keep all properties of equal weight, certain transformations would have to be employed. They too have come to practice a type of "weighting." Although the weighting practiced by the numerical taxonomists is intended to accomplish very different ends from that of the evolutionists, the methodological problems are the same. The decision to use a logarithmic rather than an arithmetic transformation, for example, is no more quantitative, objective, etc. than comparable decisions made by evolutionists in phyletic weighting. The abandonment of the purposes of evolutionary taxonomy has served only to delay the need to solve the problems of weighting.

There need be no differences in methodology between the evolutionists and the pheneticists (or numerical taxonomists). The basic difference between them is a decision as to the purposes of taxonomy. Evolutionists want to make use of both evolutionary theory and modern genetic theory in constructing their classifications. Numerical taxonomists want to limit themselves to just genetic theory. The methodological arguments which the numerical taxonomists have offered to justify their preference are of a dubious nature. The insistence of many contemporary taxonomists (and not just numerical taxonomists) on the use of certain techniques, especially advanced mathematics, and a determination *to get somewhere*, though admirable in themselves, are reminiscent of what in philosophy has been called the "hup, two, three school." (Kaplan, 1964.) There is nothing wrong with either of these desires, but it should be kept in mind that neither requires the abandonment of the purposes of evolutionary taxonomy.

The introduction of the mathematical techniques into biological classification which numerical taxonomists have fostered was long over-due. The title "numerical taxonomist" is complimentary. None of the arguments in this paper should be taken as criticizing these accomplishments. What has been at issue are some of their negative theses; for example, the thesis that since phylogeny is "unknowable," it should play no part in the construction of a classification (although inferences to it afterwards are all right) or the claim that phyletic weightings are intrinsically "subjective." The title "phenetic taxonomist" is not complimentary. In recognition of the methodological problems which they share with the evolutionists and from the secure place which their techniques now hold in taxonomy, perhaps numerical taxonomists might profitably re-evaluate some of their early criticisms of evolutionary taxonomy, including the criticism that it is viciously circular.

Summary

Certain lines of reasoning common in evolutionary taxonomy have been termed viciously circular. They are quite obviously not logically circular. They do give the superficial appearance of epistemological circularity. This appearance arises from the method of successive approximation used by evolutionary taxonomists. It is argued that this method is not epistemologically circular, even when the only evidence that the taxonomist has to go on is the phenetic similarity of contemporary forms. The important criticism of evolutionary taxonomy is rather that in the absence of fossil evidence phyletic reconstructions are not warranted. It is argued that this charge stems initially from a misunderstanding of the kind of certainty possible in empirical science. When this criticism is couched in appropriate terms, it may be seen to have some force. Many phyletic inferences are not as warranted as one might wish. However, there is a great deal of difference between arguing that a

line of reasoning is unwarranted and arguing that it is viciously circular.

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